Unemployment and Active Labor Market Policy

New Evidence on Start-up Subsidies, Marginal Employment and Programs for Youth Unemployed

Inaugural-Dissertation

zur Erlangung des akademischen Grades
eines Doktors der Wirtschafts- und Sozialwissenschaft (Dr. rer. pol.)
der Wirtschafts- und Sozialwissenschaftlichen Fakultät
der Universität Potsdam

vorgelegt von

Diplom-Volkswirt Steffen Künn
geboren am 16. Januar 1980 in Neustrelitz
wohnhaft in Bonn

— eingereicht im Juni 2012 —

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Published online at the
Institutional Repository of the University of Potsdam:
URL http://opus.kobv.de/ubp/volltexte/2012/6271/
URN urn:nbn:de:kobv:517-opus-62718
http://nbn-resolving.de/urn:nbn:de:kobv:517-opus-62718

Acknowledgements

I am very grateful to my supervisor Prof. Dr. Marco Caliendo. He inspired me to become a PhD student and to write this thesis. Throughout my dissertation, he provided me with guidance, support and advice not only in scientific, but also in personal and administrative questions. Marco, thanks for your support. Furthermore, I would like to thank Prof. Dr. Alexander S. Kritikos for acting as the second supervisor. I was very happy and proud that he immediately agreed to supervise my thesis when I approached him.

I would like to thank numerous other people. First of all, many thanks to all my colleagues at the Institute for the Study of Labor (IZA) for very useful discussions and comments. In particular, I would like to thank Dr. Ulf Rinne, Ricarda Schmidl and Dr. Arne Uhlendorff. I am also indebted to my student assistants who patiently carried out numerous literature and data research tasks. I would furthermore like to emphasize that I considerably benefited from being part of the IZA Scholarship Program. Besides financial support, IZA provided me with data access, additional support to attend scientific conferences, workshops and summer schools – but most importantly access to the international research community via the IZA Research Network. Moreover, I thank the Institute for Employment Research (IAB) for providing access to administrative data and financial support under the research grant No. 1007 to construct the survey on start-up subsidies in Germany. In particular, I thank Steffen Kaimer for his valuable advices with the administrative data.

Writing my doctoral thesis was a great experience, but also time-consuming and exhausting. Although I assume that most of them never really recognized their backup, I would like to thank my family and friends. I thank Robert Zimmermann, Steffen Metzdorff, Steffen Peris and Tino Koch for accompanying me since the childhood and for being my best friends. Special thanks to my mother, my sister Sandra and her husband Tony as well as my wonderful nieces Marlene and Luise for being always supportive and encouraging. I thank my grandparents for teaching me positive values and attitudes (and their patience while putting me through this). I reserved the final thank-you for Annemarie as she makes my life complete. Thank you for your love!

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1 Introduction to Unemployment and Active Labor Market Policy

In industrialized economies such as the European countries unemployment rates are very responsive to the business cycle. In the recent economic crisis starting in 2008, the drop in GDP growth in European countries of -4.4% in 2009 was reflected by an increase in unemployment rates of 30%. Among the unemployed a significant share of about 45% stays unemployed for more than one year. To fight cyclical and long-term unemployment countries spend significant shares of their budget on Active Labor Market Policies (ALMP) such as training, job creation schemes, job search assistance, wage or business subsidies. ALMP are expected to counteract temporary variations in unemployment rates by supporting a fast re-integration of unemployment entrants. Furthermore, longer lasting ALMP aim to help long-term unemployed individuals to overcome more structural problems of re-integrating into the labor market. To improve the allocation and design of ALMP it is essential for policy makers to have reliable evidence on the effectiveness of such programs available. Although improved data availability and progress in econometric methods led to an increase in evaluation studies during the last decades, policy makers lack evidence on innovative programs and for specific subgroups of the labor market. Therefore, this book extends the existing evidence in three directions. First, the promotion of self-employment among the unemployed, a relatively recent ALMP program, is considered. Second, the impact of being marginally employed and therefore having additional earnings during unemployment on labor market outcomes is investigated. And finally, this book explores the effectiveness of ALMP for unemployed youth, a subgroup of the labor market which is of high interest but often left out in existing evaluation studies.

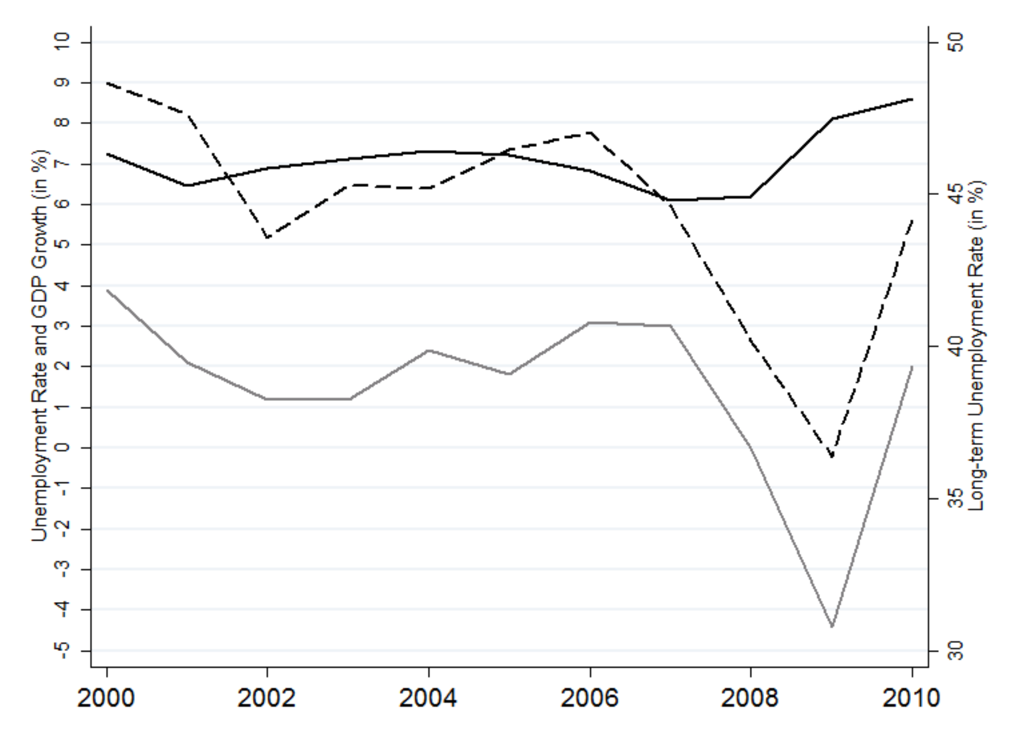
1.1 Motivation

Figure 1.1 contrasts GDP growth to the unemployment and long-term unemployment rate among prime-age individuals within the EU 15 countries[1]. It can be seen that during the last decade on average 7% of the labor force aged between 25 and 54 years was unemployed and between 40-50% stayed unemployed for more than 12 months. Moreover it can be seen that unemployment rates are very responsive to the business cycle as illustrated by the sharp increase in the aftermath of the recent economic crisis starting in 2008. In the transition from 2008 to 2009, the drop in GDP by -4.4% was reflected by an increase in unemployment rates by about 30%. Furthermore, while long-term unemployment was slightly decreasing within the period 2006 to 2009 down to its minimum of 36% in the last decade, it has risen again to 45% in 2010. All these observations unambiguously show that industrialized economies such as the European countries are characterized by unemployment rates that are very responsive to the business cycle and a problem of high shares of long-term unemployed individuals.

To fight cyclical and long-term unemployment countries primarily rely on active labor market policies such as training, job creation schemes, job search assistance, wage or business subsidies. This is illustrated by Figure 1.2. It can be seen that European countries spent significant shares of their budget on ALMP, varying from below 0.5% of GDP for countries such as Greece, UK, Italy and Luxembourg to almost 1.5% in Belgium and Denmark. ALMP are expected to counteract temporary variations in unemployment rates by supporting a fast re-integration of unemployment entrants. Furthermore, longer lasting ALMP such as retraining programs aim to help long-term unemployed individuals to overcome more structural problems of re-integrating into the labor market. This is particularly important as the employability of individuals decreases with unemployment duration which makes it harder for them to re-enter employment.

[1] The EU 15 includes all countries before the EU enlargement in 2004 took place, i.e., Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden and UK.

Figure 1.1: GDP growth, Unemployment and Long-Term Unemployment Rates Among Prime-Age Individuals Within the EU15 Countries



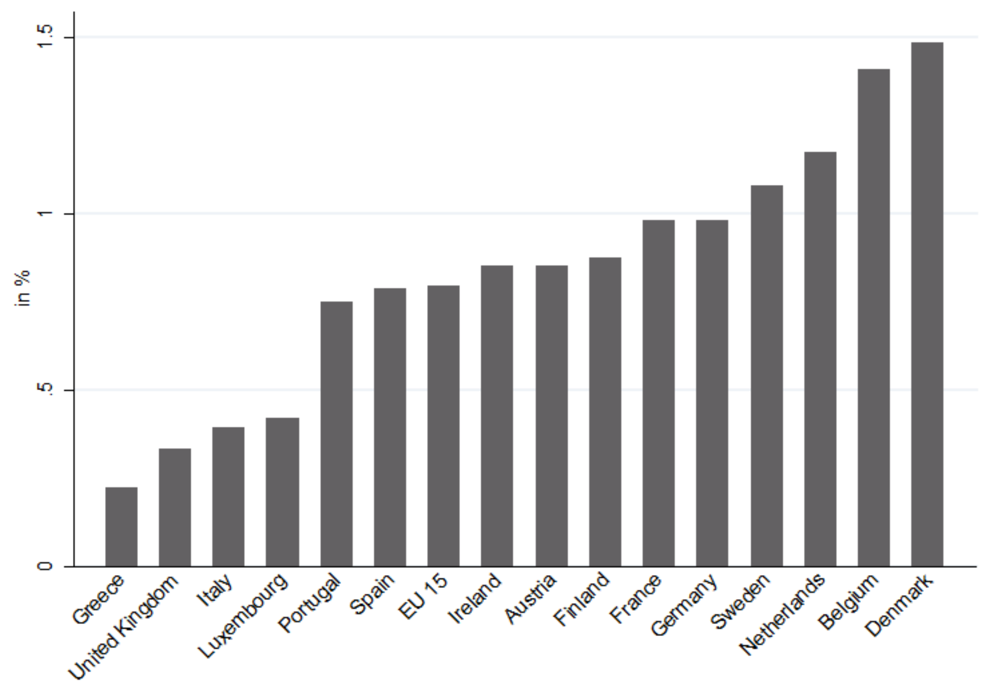
— GDP growth — Unemployment rate - - - Long-term unemployment rate

Source: Eurostat.
Note: Depicted is the GDP growth and, the unemployment and long-term unemployment rate for individuals aged between 25 and 54 years, within the EU 15 countries. GDP growth is defined as the change to the previous year. The unemployment rate is defined as the number of unemployed persons as a percentage of the labour force (the total number of people employed or unemployed). The long-term unemployment rate is given by the number of unemployed individuals with an unemployment duration of 1 year or more as a percentage of all unemployed individuals. EU 15: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden and UK.

In contrast to passive measures[2] which ensure a certain wealth level during unemployment, ALMP programs focus directly on an improvement in labor market outcomes of unemployed individuals such as the reintegration in employment or an increase in wage levels (see Cahuc and Zylberberg, 2004). Therefore, active measures aim to increase the ability or the willingness of unemployed individuals to find and take jobs which is directly linked to an increase in the efficiency of the matching process between unemployed individuals and available vacancies (Layard et al.,

[2]Providing financial assistance during unemployment induces moral hazard as it increases individual's income and therefore reduce their willingness to take jobs. This is confirmed by the empirical evidence showing that more generous unemployment benefit systems extend unemployment duration but also increase the stability of subsequent jobs as individuals have more time to search for better job (see Lalive, 2008; Tatsiramos, 2009; Caliendo et al., 2012).The strictness of the benefit system also matters and sanctions for instance are shown to have a significant impact on the job search behavior of the unemployed (e.g. Arni et al., 2012; van den Berg and Vikström, 2009).

Figure 1.2: Expenditures on ALMP as the Share of National GDP Within the EU15 Countries in 2009



Source: Eurostat.
Note: EU 15 depicts the average expenditures on ALMP within the 15 European countries.

2005; Kluve et al., 2007). Programs are expected to remove disadvantages of the unemployed compared to “insiders”, i.e., employed individuals. Those disadvantages might be in terms of human capital, employability, job search or stigmatization. For instance, training programs might increase participant’s employability by adjusting the qualification of the unemployed individual to meet the requirements of available jobs. Another example, which is part of this book, are start-up subsidies for the unemployed. Unemployed individuals are likely to face capital market imperfections and encounter discrimination by capital markets due to a bad reputation or poor debt records etc (see Meager, 1996; Perry, 2006). This results in a suboptimal rate of start-ups and/or undercapitalized businesses. Start-up subsidies aim to overcome these barriers and to remove financial disadvantages of unemployed individuals compared to more wealthy individuals, including the coverage of the cost of living and social security during the critical founding period.

In addition to the effects on the individual level, ALMP might have an impact on equilibrium unemployment which is the economically efficient, long-run level of unemployment. Theoretical models predict that any policy affecting the matching process between the unemployed and vacant jobs or the wage level will directly lead to deviations from equilibrium unemployment (Pissarides, 2000; Layard et al., 2005). Beside employment protection law, mobility barriers, unions and taxation amongst others, in particular active labor market policies play an important role in determining equilibrium unemployment due to its impact on both the matching process and wage determination. To give two examples. First, job search assistance increases participant's search intensity which makes it easier for firms to fill vacancies and firms do not have to increase wages to attract workers (Calmfors, 1994). Both is expected to impact labor demand positively. Moreover, programs such as wage subsidies reduce wage costs for firms directly which is expected to increase labor demand as it affects the wage-setting process.

Beside the very promising theoretical effects of ALMP, it is crucial to bear in mind that ALMP programs might also generate negative effects. From a basic job search model we know that the increased employability due to training programs for instance, lead participating individuals to reconsider their reservation wages upwards as they experience a higher job arrival rate (Cahuc and Zylberberg, 2004). The higher reservation wage is expected to induce longer unemployment duration. Furthermore, ALMP programs decrease the search intensity of individuals during the period of actual participation which is referred to as locking-in effects in the evaluation literature (Calmfors, 1994). Another concern are anticipation effects. The announcement of program participation might reduce the willingness of individuals to take jobs which is a phenomena known as Ashenfelter's Dip (Ashenfelter, 1978). A recent study by van den Berg et al. (2009) shows though, that a high perceived treatment probability has a positive impact on the job search behavior as individuals probably dislike participation in certain programs. This is consistent with the argument that programs attracting rather disadvantaged groups of the labor market, e.g., long-term unemployed with low levels of education, are associated with negative stigmatization which reduces the employment chances of participants (Kluve et al., 2007).

In addition to the negative effects for program participants, ALMP programs might further induce negative distortions for non-participants, such as deadweight, substitution or displacement effects which crowd out regular employment (see also Calmfors, 1994, for a discussion). These so-called equilibrium effects have found particularly prevalent with subsidy programs. In the case of a wage subsidy, deadweight effects occur if the unemployed individual would have found the job even without the subsidy. Displacement effects occur if a firm with subsidized workers would displace other firms without subsidized workers and substitution occurs if the firm replaces already employed workers by new subsidized workers.

1.2 Empirical Evaluation of Active Labor Market Policy

As theoretical considerations lead to ambiguous predictions with respect to the effectiveness of ALMP programs empirical evidence is required to assess their impact. As shown in Figure 1.2 European countries spend significant shares of their budget on ALMP which highlights the relevance of ALMP and the importance for policy makers to know which program works and which not. Beside the rising demand for reliable evidence, the progress in terms of econometric methods, simplified data access with increasing data quality and the variety of available variables, as well as an increase in computational resources during the last decades facilitated a growing number of evaluation studies (Heckman et al., 1999; Blundell and Costa Dias, 2000; Imbens and Wooldridge, 2009). For instance, 90% of the evaluation studies considered in the meta-analysis by Card et al. (2010) are published in the year 2000 or later.

Following Fay (1996), the empirical evaluation of ALMP should optimally incorporate a consideration of impacts on the individual (participant) level and on equilibrium effects, and should conclude with a cost-benefit analysis. While the analysis of the individual effects mainly focusses on participants prospective labor market outcomes, the investigation of equilibrium effects regards consequences for non-participants, e.g., deadweight, substitution and displacement effects as explained before. The difference between the individual and equilibrium effect then gives the net effect of ALMP programs (Fay, 1996). Finally, after having identified the net effect, the optimal evaluation study concludes with a cost-benefit-analysis which provides information about the financial effectiveness of the program under scrutiny. The cost-benefit-analysis compares the net program effect to fiscal costs. However, the implementation of equilibrium and cost-benefit analyses is very difficult as they require strong and often questionable assumptions. Evaluation on individual program effects in contrast are comparatively easier to implement and much more clear with respect to the program effect. This book also provides an evaluation of different ALMP programs with respect to the impact on the individual level, so that the following discussion on the econometric methodology and existing evidence focusses on the individual program impact.[3]

When evaluating ALMP programs on the individual level the main interest is in the causal effect of program participation on labor market outcomes of participants (Caliendo and Hujer, 2006 and Imbens and Wooldridge, 2009 provide insightful overviews of available methods and recent developments in the field of program evaluation). As the researcher wants to compare the labor market outcome of an individual with and without the treatment, the fundamental evaluation problem arises by the fact that one single individual is only observed either as a participant or as a non-participant. Hence the researcher has to construct a counterfactual situation using information of "control" individuals who did not receive the treatment. Comparing unconditional outcomes of treated and non-treated individuals, however, is likely to introduce a bias as participants usually differ from non-participants in some characteristics that influence participation as well as labor market outcomes. Running an experiment where program assignment occurs randomly would solve the fundamental evaluation problem and the unconditional difference between the group of treated and non-treated would then represent the treatment effect. However, experimental data on labor market policies are scarce and therefore the researcher has primarily to deal with non-experimental data. Those data are usually collected by surveys or due to administrative processing at public institutions. In particular the access to administrative data improved the quality of evaluation studies remarkably as those datasets have the advantage of large numbers of observations and reliable information, i.e., not self-reported. However, with non-experimental data at hand the researcher has to control for selection into programs to estimate causal effects (see Imbens, 2004, for an overview).[4] Different econometric approaches exists to control for selection. The methods basically differ in terms of allowing for selection due to observable (e.g. education, labor market history) and unobservable (e.g. ability, motivation) characteristics. Both types of methods have their advantages and

[3]For evidence on the macroeconomic consequences of ALMP see Calmfors and Skedinger (1995), Dahlberg and Forslund (2005) or Hujer et al. (2009). Only few studies conduct a cost-benefit analysis as this requires very strong assumptions, in particular with respect to the counterfactual behavior of participants. As an example see Jespersen et al. (2008) which includes a cost-benefit analysis.

[4]Card et al. (2010) find for existing evaluation studies on program effectiveness that results based on non-experimental methods do not significantly differ from those based on experiments.

disadvantages and it's in the researchers discretion to decide on a particular case which is the appropriate econometric approach to solve the problem at hand. The empirical analyses in this book base solely on non-experimental data and we are going to take great care of discussing the justification of the identifying assumptions required to estimate causal program effects in each chapter.

Due to the variety of ALMP programs and econometric methods, numerous empirical microeconometric evaluation studies exist whereby the evidence is often ambiguous.[5] In this case a meta-analysis is very helpful to summarize existing evaluation studies by identifying systematic patterns between the estimated effects and program types.[6] The most recent and comprehensive meta-analyses are provided by Card et al. (2010) and Kluve (2010). Both studies consider microeconometric evaluation studies in different countries and conclude that training measures, job search assistance and wage subsidies seem to improve participants labor market prospects while job creation schemes are overall ineffective. A more detailed discussion of the existing literature will be provided in each of the following chapters.

1.3 Outline and Contribution

Despite the large number of existing evaluation studies, research gaps still exist with respect to more recent programs and program effectiveness for specific subgroups of the labor market. This book contributes in this way by using Germany as a case study. Germany is a good example to study the effectiveness of ALMP due to the variety of different ALMP programs (e.g. Wunsch, 2006, provide an overview) and access to high quality data consisting of administrative and survey information. Moreover, the composition of the unemployed workforce seems to be representative towards other industrialized countries. For instance, the unemployment rate among prime-age males (low educated) was 7.1% (16.5%) in Germany compared to the EU15-average of 8.4% (15.2%) in 2010. This support the hypothesis that the revealed evidence using the German case is likely to be adoptable to other industrialized economies. Reinforcing, Kluve (2010) finds that programs either work or

[5]See Martin and Grubb (2001); Dar and Gill (1998); Dar and Tzannatos (1999); Fay (1996); Kluve and Schmidt (2002); Betcherman et al. (2004); Lechner et al. (2011); Fitzenberger et al. (2008) amongst many others.

[6]It has often been argued that meta-analyses suffer from a publication bias as the analysis takes solely published studies into account and published studies are more likely to report statistically significant results (Easterbrook et al., 1991). However, Card et al. (2010) do not find an indication for the existence of a publication bias in their study.

not and that institutional factors have only little impact on program effectiveness in general.

This book extend the existing literature in three directions. First of all, only little is know about a relatively recent ALMP program type that is the promotion of self-employment among the unemployed. The idea is to encourage unemployed individuals to exit unemployment by starting their own business. Those programs have compared to traditional programs of ALMP the advantage that not only the participant exits unemployment but also might generate additional jobs for other (unemployed) individuals. However, the empirical evidence on the effectiveness of such programs is scarce, in particular with respect to long-term evidence and effect heterogeneity. Chapter 2 aims at closing this research gap and considers two distinct start-up subsidy programs for the unemployed in Germany whereby the programs mainly differ in terms of the amount of the monetary support and duration of the payment. Based on combined administrative and survey data, Chapter 2 provides a comprehensive analysis on the effectiveness of the two start-up programs including long-term evidence and effect heterogeneity.

Second, only little attention has been paid so far to the availability of marginal employment schemes to the unemployed and its consequences for labor market outcomes. Unemployed individuals in some countries like Germany are allowed to earn additional income during unemployment without suffering a reduction in their unemployment benefits. Those additional earnings are usually earned by taking up so-called marginal employment that is employment below a certain income level subject to reduced payroll taxes. Marginal employment can therefore be considered a wage subsidy as it lowers labor costs for firms and increases work incentives for the unemployed due to higher net earnings. Additional earnings during unemployment might lead to higher reservation wages prolonging the duration of unemployment, yet also giving unemployed individuals more time to search for better and more stable jobs. Furthermore, marginal employment might lower human capital deterioration and raise the job arrival rate due to network effects. Its impact on unemployment duration and subsequent job quality is therefore from a theoretical perspective ambiguous which requires empirical evidence. Chapter 3 considers an inflow sample into unemployment in Germany and provides an empirical evaluation of the impact of marginal employment on unemployment duration and subsequent job quality.

Finally, Chapter 4 considers unemployed youth as a subgroup of the labor market. It is well known that youth are generally considered a population at risk as they have lower search skills and little work experience compared to adults. This results in above-average turnover rates between jobs and unemployment for youth which is particularly sensitive to economic fluctuations. It has been shown that unemployment spells in an early stage of the labor market career lead to persistent "scarring" effects on later labor market outcomes. In addition, high youth unemployment rates are associated with increased social costs due to the depreciation of human capital, rising crime rates, drug abuse and vandalism. Against this background, the majority of European countries spends significant resources to fight youth unemployment. However, so far only little is known about the effectiveness of ALMP for unemployed youth and with respect to Germany no comprehensive quantitative analysis exist at all (see Card et al., 2010). Extrapolating from evaluation results for the adult workforce is not an option due to the distinctive characteristics of young labor market entrants. Therefore, Chapter 4 aims to close this research gap and investigates the effectiveness of different ALMP programs to improve the labor market perspective of unemployed youth in Germany.

2 Start-Up Subsidies for the Unemployed

Turning unemployment into self-employment has become an increasingly important part of active labor market policies in many OECD countries. Germany is a good example where the spending on start-up subsidies for the unemployed accounted for nearly 17% of the total spending on ALMP in 2004. In contrast to other programs—like vocational training, job creation schemes, or wage subsidies—the empirical evidence on the effectiveness of such schemes is still scarce; especially regarding long-term effects and effect heterogeneity. This chapter aims to close this gap and based on administrative and survey data, we show that such programs significantly improve long-term labor market prospects of participants. Moreover, we show that start-up subsidies for the unemployed tend to be most effective for disadvantaged groups and within deprived labor markets. The female-specific analysis reveals that in contrast to traditional programs of ALMP, start-up programs have less detrimental effects on fertility as self-employment gives women apparently more independence and flexibility in allocating their time to work and family.[7]

[7] This chapter is based on joint work with Marco Caliendo (Caliendo and Künn, 2011, and unpublished work).

2.1 Introduction

The recent OECD report on income and poverty (OECD, 2008) illustrates an increase in poverty rates over the past decade, where the risk of becoming poor shifted from the elderly in particular towards children and people of working age. The importance of employment in this context is straightforward as poverty among non-working households increased sharply during the last decade. The poverty rate[8] for households where the head is of working age but no household member actually works amounted to 36% and was three (twelve) times higher than for households with one (two or more) worker in the mid-2000s. Despite cross-country variation in terms of the scope of poverty, the negative correlation between employment rates and poverty is throughout valid. In an earlier study, Sen (1997) presents different concepts on how unemployment may cause poverty and inequality due to social exclusion. The main idea is that specific groups of individuals are generally excluded from the labor market, for example low skilled or youth. In addition, economic conditions may also foster social exclusion. He argues that along with the abolishment of social exclusion, unemployment and therefore poverty will be reduced. Governments are fully aware of this concept and therefore spend significant amounts of their budget on active labor market policies (ALMP) to equalize labor market conditions of unemployed individuals, in which a special focus is usually put on disadvantaged groups. By removing severe differences in terms of education, work experience or productivity, existing labor market barriers are to be overcome, consequently reducing unemployment. Several labor market programs have been introduced in which the most popular programs are traditionally training measures such as retraining, classroom training or on-the-job training. Furthermore, employment subsidies, job creation schemes and job-search assistance have also been adapted by almost all OECD countries. These programs are supposed to integrate unemployed individuals in the labor market and are associated with an upward shift in income level to secure one's livelihood and an increase in life and job satisfaction. Much research has been dedicated to investigating the effectiveness of ALMP programs. Although positive results with respect to income and employment prospects were found occasionally, the overall evidence indicates that the effects of those traditional measures are rather disappointing (see Martin and Grubb, 2001; Dar and Gill, 1998; Dar and Tzannatos, 1999; or Fay, 1996 for evidence on OECD countries and Kluve and

[8]The poverty rate is defined as the share of people with an equivalised disposable income below 50% of the median of the entire population.

Schmidt, 2002 for the European experience). In particular, job creation schemes turn out to be not appropriate for improving participants' employment perspectives.

On the other hand, it is found that the promotion of self-employment among unemployed individuals is a promising tool. Unemployed individuals are likely to face capital market imperfections and encounter discrimination by capital markets due to a bad reputation or poor debt records etc (see Meager, 1996; Perry, 2006). This results in a suboptimal rate of start-ups and/or undercapitalized businesses. Start-up subsidies aim to overcome these barriers and to remove financial disadvantages of unemployed individuals compared to more wealthy individuals, including the coverage of the cost of living and social security during the critical founding period. Beside those differences to non-unemployed individuals, among the unemployed in particular women need to be supported. Theory predicts that individuals become self-employed if the expected discounted utility of being self-employed exceeds those of being in paid work (see Knight, 1921; Blanchflower and Oswald, 1998; Parker, 2009). As self-employment is considered to be very time consuming and associated with the risk of debts in case of business failure the expected utility of self-employment is particularly low for women as women are on average more risk averse and allocate less time to the labor market activities than men.[9] Consistent with this, we observe that the share of self-employed women among all working women is lower than for men. Therefore, the existence of start-up subsidies might be particularly important for unemployed women in order to consider self-employment as an alternative to dependent employment.

In addition, public authorities usually tie start-up subsidies with the hope for a "double dividend". Besides creating a job for the self-employed themselves, the newly founded businesses may potentially create further jobs and thus reduce unemployment rates even further. Moreover, individuals who receive support also increase their employability, human capital and labor market networks during the period of self-employment, which, in the case of failure, makes them more able to find regular employment. Start-up subsidies may also be promising from a macroeconomic perspective, since the entry of new firms generally increases competition and consequently productivity of firms. This potentially can promote efficient markets and technology diffusion and might finally lead to economic stability and economic growth, i.e., an increase in wealth (see Storey, 1994; Fritsch, 2008). However, there

[9] Based on a cross-country study Bönte and Jarosch (2011) provide empirical evidence that gender differences in competitiveness and risk preferences significantly contribute to the gender gap in entrepreneurship.

are also some concerns related to financial promotion of start-ups by the unemployed. First of all, supported individuals may have become self-employed even without financial support. This is referred to as deadweight loss and is usually hard to determine.[10] Another concern addresses crowding out effects, whereby incumbent or non-subsidized firms may be displaced by supported start-ups. Finally, firms may also substitute employees with subsidized self-employed workers. Due to a highly regulated labor market in Germany, however, such substitution effects are likely to play only a minor role in practice.

This chapter focusses on the effects of start-up subsidies on the participating individuals only, that is it does not address any macroeconomic or general-equilibrium effects. Most of the existing evaluation studies on start-up schemes report positive effects with respect to different labor market outcomes. The evidence varies with respect to countries and institutional design of support. A main shortcoming of previous studies is that they provide short- to medium-run evidence only and—especially in the case of industrialized countries—do not consider effect heterogeneity. If the analysis is conducted at a point at which individuals still receive the support, the results are likely to be upward biased due to locking-in effects. To properly judge the effects of the programs, the observation window needs to be (substantially) longer than the period of support. Furthermore, it can be assumed that there will be heterogeneity in the effects of these programs, which implies that some groups might benefit more and others less from participation. This is of special interest for particular disadvantaged groups, for example low educated or young individuals who are over-represented among the long-term unemployed and socially excluded. Beside heterogeneity with respect to individual characteristics of participants, effectiveness might also vary with local economic conditions. In areas with unfavorable economic conditions, business survival is generally lower but on the other hand non-participants also face lower employment probabilities due to limited job offers. Which of the two opposing impacts dominates is of high interest but unexamined so far. Knowing how start-up schemes work for disadvantaged groups and within different labor markets will help to design and assign programs more appropriate and thereby tackle long-term unemployment, social exclusion, and the associated risk of poverty.

Moreover, this chapter investigates to what extent start-up programs are help-

[10] Meager (1993) provides an estimate of the deadweight effect related to the bridging allowance in Germany and concludes that the effect is rather small (about 10%).

ful to unemployed women. This is particularly interesting against the background that women tend to leave the workforce with increasing unemployment duration and low female labor market participation in general (61% in 2008 within the OECD) on the one hand and the disappointing results with respect to the effectiveness of traditional ALMP programs for women on the other hand. Due to higher preferences for flexible working hours among women and missing part-time opportunities, it has been found that traditional ALMP programs which focus on the integration in dependent employment increase labor market attachment of unemployed women, however, reducing fertility at the same time. It seems that dependent employment does not provide sufficient flexibility to allow women to balance work and family obligations. The OECD highlights the problem of declining fertility rates within OECD countries and its societal consequences, e.g., securing generational replacement and aging population (see Sleebos, 2003). Against this background traditional programs of ALMP turn ineffective for women if fertility is considered as important as employment. The idea of supporting self-employment among unemployed women might be more promising in this regard. Unemployed women start their own business which gives them probably more flexibility and independence to reconcile work and family compared to dependent employment (which is the focus of traditional ALMP programs). Although existing evidence confirms the promising expectations in terms of employment prospects for unemployed women, long-term evidence is missing and the impact on fertility is completely unexamined.

The aim of this chapter is to close the aforementioned existing research gaps by providing long-term evidence and an extensive analysis with respect to individual and regional effect heterogeneity. Moreover, it particularly considers unemployed women and investigate to what extent start-up subsidies help unemployed women to escape unemployment and affect fertility outcomes. Therefore, two distinct start-up subsidies for unemployed individuals in Germany are considered. The first program—*bridging allowance* (BA, "Überbrückungsgeld")—provided relatively high financial support (depending on individuals' previous earnings) to unemployed workers for six months; whereas the second program—*start-up subsidy* (SUS, "Existenzgründungszuschuss")—consisted of (lower) monthly lump-sum payments for up to three years.[11] Since both schemes differ sharply in terms of financial support and duration, they also attracted different types of individuals. The empirical analysis

[11] Both programs were replaced in August 2006 by a single new program—the new start-up subsidy program (*Gründungszuschuss*)—which will not be analyzed here.

is based on a combination of administrative and survey data which allows to follow individuals for nearly five years after entering the programs. In addition to information on program participants the data also contain a suitable control group of other unemployed individuals. The structure of the data, i.e., very detailed information on both participants and non-participants, allows therefore to use propensity score (PS) matching methods for the impact analysis. As using PS matching requires the conditional independence assumption, i.e., individual outcome is independent of treatment conditional on observable characteristics, great care is taken in assessing the sensitivity of the results with respect to deviations from the identifying assumption. To preview, the results turn out to be robust and we find strong positive long-run effects nearly five years after start-up for both programs with respect to several labor market outcomes. In addition, we show that they are most effective for individuals at high risk of being excluded from the labor market, i.e., low educated and low qualified individuals, and in particular in labor markets characterized by unfavorable economic conditions. With respect to unemployed women, start-up programs improve employment prospects of female participants whereby (in contrast to traditional programs of ALMP) the impact on fertility is less detrimental as self-employment seems to give women more flexibility to reconcile work and family.

This chapter is organized as follows: Section 2.2 provides a brief literature review on the effectiveness of traditional ALMP programs in an international context. Furthermore, it gives a detailed overview on the existing evidence with respect to the promotion of self-employment among the unemployed. To set the stage for the empirical analysis, Section 2.3 provides institutional details on the two start-up programs under consideration, Section 2.4 introduces the data and Section 2.5 explains the identification strategy to estimate causal program effects and discusses its underlying assumptions. Section 2.6 starts the empirical analysis by considering the long-term impact of start-up subsidies on labor market outcomes of participants. The aim is to isolate the program effect from other distorting effects such as labor supply decisions of individuals and variations in labor demand due to macroeconomic conditions. Therefore, we restrict the main analysis to men in West Germany. Based on those results, Section 2.7 investigates in a second step the underlying effect heterogeneity with respect to both individual and regional characteristics. Section 2.8 finally relaxes the sample restriction and considers program effectiveness for female participants. Section 2.9 concludes.

2.2 Literature Review

The OECD reports an average spending of 0.6% of a country's GDP on ALMP among all OECD member states in 2007, and therefore, much research has been conducted investigating the effectiveness of such measures (see OECD, 2009). The main question is whether ALMP programs are appropriate for improving participants' labor market perspectives and in addition whether they also generate income gains for participants.

2.2.1 Evidence on Traditional Programs of ALMP

First of all, we start with a brief overview with respect to the effectiveness of traditional programs of ALMP such as training, job search assistance, wage subsidies and job creation schemes. Those programs are widespread and despite smaller nation-specific modifications usually implemented by all OECD countries. Therefore, many evaluation studies exist. Starting with evidence on developing and transition countries, Betcherman et al. (2004) provide an overview on the effectiveness of ALMP in such countries and find some positive results for employment services while training measures, public works and wage subsidies are rather unsuccessful. Turning the focus towards more developed economies, the international meta-analysis conducted by Card et al. (2010) investigates effectiveness of several ALMP programs within 26 countries and concludes that training measures are promising in the medium-term but job creation schemes are overall ineffective. With a particular focus on OECD countries, Fay (1996), Dar and Gill (1998), Dar and Tzannatos (1999) and Martin and Grubb (2001) review evaluation studies on ALMP and present mixed results for several programs. In fact, they do find overall negative results for job creation schemes and some positive results for other programs for certain subgroups, for example training for the long-term unemployed, or training, job search assistance and employment subsidies for women. The more recent study by Martin and Grubb (2001) particularly highlights the gender gap in terms of program effectiveness, i.e., although effects are small (in particular in terms of earnings) they are always more favorable for women. Focusing on Europe, Kluve and Schmidt (2002) find strong heterogenous effects for different programs and subgroups and argue that job search assistance and training might be effective. This is confirmed by the meta-analysis conducted by Kluve (2010). He finds that beside training and job search assistance, also wage subsidies are effective in European countries. The aforementioned gender gap in terms of program effectiveness is confirmed by Bergemann and van den Berg (2008) for European countries. They show that ALMP is in general associated with larger employment effects for women (in particular in regions with low female labor market participation). Interestingly, Lechner and Wiehler (2011) investigate this gender gap and find for the case of Austria that female non-participants face higher probabilities to leave the workforce compared to male non-participants. Program participation therefore increases labor market attachment of female participants but the authors also show that it reduces fertility among them at the same time.

For Germany, Fitzenberger et al. (2008) and Lechner et al. (2011) find positive effects for training measures in the long-run. Moreover, Stephan (2008) and Stephan and Pahnke (2008) provide evidence for vocational training, short-term training, wage subsidies and job creation schemes and show consistently negative effects for job creation schemes (in line with Caliendo et al., 2008) and mostly positive but not always significant effects for the other programs under consideration. In contrast, Lechner and Wunsch (2008) argue that programs such as vocational training, wage subsidies, short-term training and assessment schemes are overall ineffective for the West German labor market. With a particular focus on unemployed women in Germany, the positive evidence on training measures in the long-run is confirmed whereby employment effects are also larger for women (see Biewen et al., 2007; Fitzenberger et al., 2012).

To sum up, despite occasionally positive results, the overall evidence indicates that traditional measures are rather disappointing. In particular job creation schemes turned out to be not appropriate for improving participants' employment prospects, and training programs bring modest effects only in the (very) long-run. Moreover, ALMP programs seem to be more effective for unemployed women which is attributable to higher exit rates towards inactivity among female non-participants compared to male non-participants.

2.2.2 Evidence on Start-up Programs

In light of these findings, supporting unemployed individuals in becoming self-employed might be a promising tool among active labor market policies. The international evidence is still relatively scarce on such measures but predominantly indicates positive results. To facilitate a comprehensive overview, we summarize the exiting evidence on business promotion in Table 2.1. For developing countries for instance, Almeida and Galasso (2010) investigate the short-term impact of financial and technical assistance for welfare beneficiaries on their way to self-employment in Argentina. They find an increase in total working hours but no significant income effects due to the program. However, for young and highly educated individuals they are able to identify positive income effects. They further show that in particular women are likely to start a business parallel to having another job. Rodriguez-Planas (2010) investigates a start-up program for Romania in which the participants obtained professional assistance through counseling or short-term entrepreneurial training. In addition, working capital loans were offered. She identifies positive employment effects but no income gains for participants compared to non-participants and reveals strong positive employment effects for a subgroup of low educated individuals. O'Leary (1999) considers self-employment schemes for Poland and Hungary. The scheme in Poland provides loans at market interest rates to the unemployed combined with the attractive option that 50% of repayments will be waived if firms survive at least two years. In contrast, the Hungarian program consists of unemployment benefits paid up to 18 months. In addition, it also incurs half of the costs for training and counseling. O'Leary (1999) finds large and positive employment effects for both countries. Whilst he is also able to identify strong positive earning effects for Hungary, the income effect in Poland is negative.[12] Among participants, O'Leary (1999) finds high survival rates in self-employment and additional employment effects in both countries. The findings are similarly positive for developed countries.

With respect to developed economies, Carling and Gustafson (1999) provide a comparative study between employment subsidies and self-employment grants for the unemployed in Sweden. They find that individuals in subsidized employment have a higher probability of re-entering unemployment than recipients of self-employment grants. Therefore, they conclude that self-employment grants are more effective in avoiding unemployment. Cueto and Mato (2006) analyze the success of self-employment subsidies for particular districts in Spain. They find survival rates of approximately 93% after two years and 76% after. The drawback of this study is that they do not have a group non-subsidized firms available. In a gender-specific analysis, Cueto and Mato (2006) argue that men's survival is predominately determined by the economic situation (main source of household income) while women's

[12] O'Leary (1999) primarily attributes the negative earning effect in the case of Poland to firms' reluctance in full disclosure to the tax authorities.

survival depends mainly on individuals characteristics (marital status, education). This might indicate that women with family obligations face (e.g. due to stronger preferences towards flexible working hours) limited job offers in the regular labor market. For New Zealand, Perry (2006) evaluates enterprise allowance grants, an integrated program that provides business skills training as well as financial aid. The author's results indicate a decrease in time registered as unemployed for participants. Meager et al. (2003) evaluate business start-up subsidies by the Prince Trust to young people in the UK. The authors conclude that participating in the program does not have any significant impact on subsequent employment or earning chances. Nonetheless, descriptively they find a fraction of 69.1% in self-employment among participants after 18 months. Kelly et al. (2002) consider an allowance paid up to 52 weeks as well as training and counseling in Australia. The authors find a high integration in employment three years following start-up. Tokila (2009) considers start-ups out of unemployment in Finland who received a subsidy. Comparable to the Bridging Allowance in Germany, the subsidy in Finland consists of unemployment benefits paid for 10-15 months during start-up. She observes firms up to 14 years after start-up, runs a survival analysis and finds that the subsidy extends business survival.

Table 2.1: Existing Evidence on Business Promotion

Study	Country	Obs. period since start-up	Effects on participant's Employment prospects	Effects on participant's Income situation
Evidence on developing and transition countries				
Almeida and Galasso (2010)	Argentina	12 months	n/a	insignificant
O'Leary (1999)	Hungary	21 months	+	-
	Poland	50 months	+	+
Rodriguez-Planas (2010)	Romania	24 months	+	insignificant
Evidence on developed countries				
Carling and Gustafson (1999)	Sweden	36 months	+	n/a
Cueto and Mato (2006)	Spain	60 months	+	n/a
Kelly et al. (2002)	Australia	36 months	+	n/a
Meager et al. (2003)	UK	18 months	insignificant	insignificant
Perry (2006)	New Zealand	24 months	+	n/a
Tokila (2009)	Finland	168 months	+	n/a
Evidence on Germany				
Baumgartner and Caliendo (2008)	West Germany	28 months	+	+
Caliendo (2009b)	East Germany	28 months	+	+
Pfeiffer and Reize (2000)	Germany	12 months	insignificant (- in East Germany)	n/a

Note: "+"/"-" indicates positive/negative evidence; "n/a" indicates that the evidence is not provided by the study.

Finally, with respect to Germany only few studies are available. Baumgartner and Caliendo (2008) and Caliendo (2009b) provide an evaluation of BA and SUS for the short- and medium-run in West and East Germany, respectively. Both studies find strong positive employment and income effects for participants compared to a group of non-participants but underscore the preliminary character of their results, as the majority of start-up subsidy participants still received financial support during the observation period. Therefore, the survival rate is likely to further decrease after financial support completely expires. In an earlier study, Pfeiffer and Reize (2000) analyze the effect of BA on survival rates in self-employment during the first year after entry. They find neither differences in survival probability nor in employment growth between supported and non-subsidized firms in West Germany.

To summarize, the existing literature on start-up schemes for the unemployed mainly reports either positive or insignificant effects with respect to different labor market outcomes; whilst negative impacts are scarce (see Table 2.1). The evidence varies with respect to countries, institutional design of the support and eligibility criteria. Although many studies have been conducted already, several research gaps still exist. Effect heterogeneity is considered only by studies on developing countries and evidence on female participants is scarce. However, the main shortcoming is that existing studies provide evidence for the short to medium-run only (except two studies from which one provides no comparison to non-participants). Long-term evidence is therefore highly demanded by the literature but—due to data limitations—still missing. We are now able to observe supported firms up to five years after start-up and hence contribute long-term evidence on both employment prospects and income measures to the literature. Moreover, we contribute an extensive analysis on effect heterogeneity and provide evidence on program effectiveness for unemployed women.

2.3 Institutional Settings in Germany

The promotion of self-employment among the unemployed has a long tradition in Germany and represents until today an inherent part of the German ALMP. Since its introduction in the late 1980's, start-up programs were subject to several reforms. In the following we focus on a detailed description of the institutional settings of the two programs under scrutiny in the empirical analysis, the *bridging allowance* and the *start-up subsidy*, and explain recent changes due to labor market reforms only briefly. The most important features of both programs are also summarized in

Table 2.2.

Table 2.2: Terms and Conditions of Programs

	Start-up Subsidy	**Bridging Allowance**
Entry conditions:	-Unemployment benefit *receipt* -Income is restricted to €25,000 per year -Approval of a business plan was subsequently introduced in November 2004 -Below 65 years of age	-Unemployment benefit *entitlement* -No income restrictions -Approval of the business plan -Below 65 years of age
Support:	-Participants receive a fixed sum of €600 in the first year, €360 (€240) in the second (third) year -Claim has to be renewed every year	-Participants receive UB for six months -To cover social security liabilities, an additional lump sum of 68.5% is granted
Others:	-Participants have to become a member in the state pension insurance and take advantage of a reduced rate in the legal health insurance	-Social security is left to the individual's discretion

Source: Social Act III, §57 - Bridging Allowance, §421I - Start-up Subsidy.

The first program under consideration, the *bridging allowance* which was introduced in 1986 and remained the only program providing support to unemployed individuals who wanted to start their own business until 2003. Its main goal was to cover basic costs of living and social security contributions during the initial stages of self-employment. The recipient of BA received the same amount during the first six months he or she would have received if unemployed. Since the unemployment scheme also covers social security contributions (including health insurance, retirement insurance, etc.) a lump sum for social security is granted equal to 68.5% of the unemployment support that would have been received. Unemployed individuals were entitled to BA on condition of their business plan being externally approved, usually by the regional chamber of commerce. Thus, approval of an individual's application did not depend on the case manager at the local labor office. In January 2003, an additional program was initiated to support unemployed people in starting a new business. This *start-up subsidy* was introduced as part of a large package of ALMP programs introduced through the "Hartz reforms"[13]. The main intention for the introduction of a second program was to encourage small business start-ups in the service sector with low profit margins. Eligibility to SUS was therefore not only restricted to unemployed individuals with benefit entitlement but also to those with means-tested social assistance, i.e., primarily long-term unemployed and indi-

[13] See Caliendo (2009a) for an overview of the most relevant elements of the "Hartz reforms".

viduals with limited labor market experience (e.g. women). The support comprises of a lump sum payment of €600 per month in the first year. A growth barrier is implemented in SUS such that the support is only granted if income does not exceed €25,000 per year. The support shrinks to €360 per month in the second year and to €240 per month in the third. In contrast to the BA, SUS recipients have to pay into the statutory pension fund and may claim a reduced rate for statutory health insurance. When the SUS was introduced in 2003, applicants did not have to submit business plans for prior approval, but they have been required to do so since November 2004. Moreover, parallel receipt of BA and SUS is excluded.

Moreover, it should be mentioned that other institutions such as federal state governments or the chamber of commerce offer general programs to encourage self-employment, for example, counseling, preparatory courses or even capital loans. Additionally, in some professions self-employment is highly restrictive in Germany when compared to other countries. For some "typical" self-employed occupations (physicians, lawyers, etc.) and several handcraft occupations it is required to occupy an advanced certificate in order to be allowed to become self-employed. However, Cressy (1996) argues that such preconditions for entry into self-employment tend to significantly enhance survival of businesses.

Table 2.3: Entries into Selected Programs of ALMP in Germany

	2003		2005		2008	
	Women	Men	Women	Men	Women	Men
Vocational training	131.3	163.4	61.5	91.3	219.6	265.8
Short-term training	453.2	613.5	379.4	521.9	549.9	664.6
Job creation schemes	54.8	86.2	29.7	48.4	28.5	41.7
Wage subsidy	71.4	112.0	50.4	92.3	108.5	173.0
Promotion of self-employment						
Bridging allowance	41.3	117.4	43.0	113.9	-	-
Start-up subsidy	38.9	56.3	43.8	47.2	-	-
New start-up subsidy	-	-	-	-	43.9	75.4

Source: Statistics of the Federal Labor Agency, December 2010.
Note: Numbers in thousand.

Due to the institutional framework, it was rather rational to choose BA if unemployment benefits were fairly high or if the income generated through the start-up firm was expected to exceed €25,000 per year. Both programs were replaced in August 2006 by a single new program, the new start-up subsidy program (*Gründungszuschuss*), which was reformed already in November 2011 but will not be analyzed here.[14] Table 2.3 provides an overview of entries into start-up programs as well as other programs of ALMP in Germany. First of all, mainly due to simplified eligibility criteria, in particular unemployed women took advantage of the introduction of the start-up subsidy in 2003 (cf. Caliendo and Kritikos, 2010). For instance, in 2003 only 26% of BA participants were female in contrast to 41% in the case of SUS. As we can see, the scope of the *new start-up subsidy* is clearly below the cumulated number of entries in BA and SUS. Moreover, it is noticeable that start-up programs are comparable in terms of number of entries to other programs of ALMP. In fact, entries into SUS and BA even exceeded the number of entries into job creation schemes and wage subsidies in 2003 and 2005. On the other hand, entries into short-term training are more than three times as much; but, of course, one has to keep in mind that those measures have a maximum duration of three months and an average duration of two weeks. Accordingly, eligibility criteria are much lower.

2.4 Data

The empirical analysis bases data on entries into SUS and BA in the third quarter of 2003[15] whereby administrative data from the Federal Employment Agency (FEA) are combined with a survey such that longitudinal as well as cross-section data are available. To construct the dataset, we draw on data used by Baumgartner and Caliendo (2008) and extend it with an additional interview wave.[16] The administrative part consists of data based on the Integrated Employment Biographies (IEB) of the FEA, containing relevant register data from four sources: employment history, unemployment support recipience, participation in active labor market measures, and job seeker history. Since the administrative data do not provide any information on self-employed individuals, the IEB data are complemented by information from a computer-assisted telephone interview.

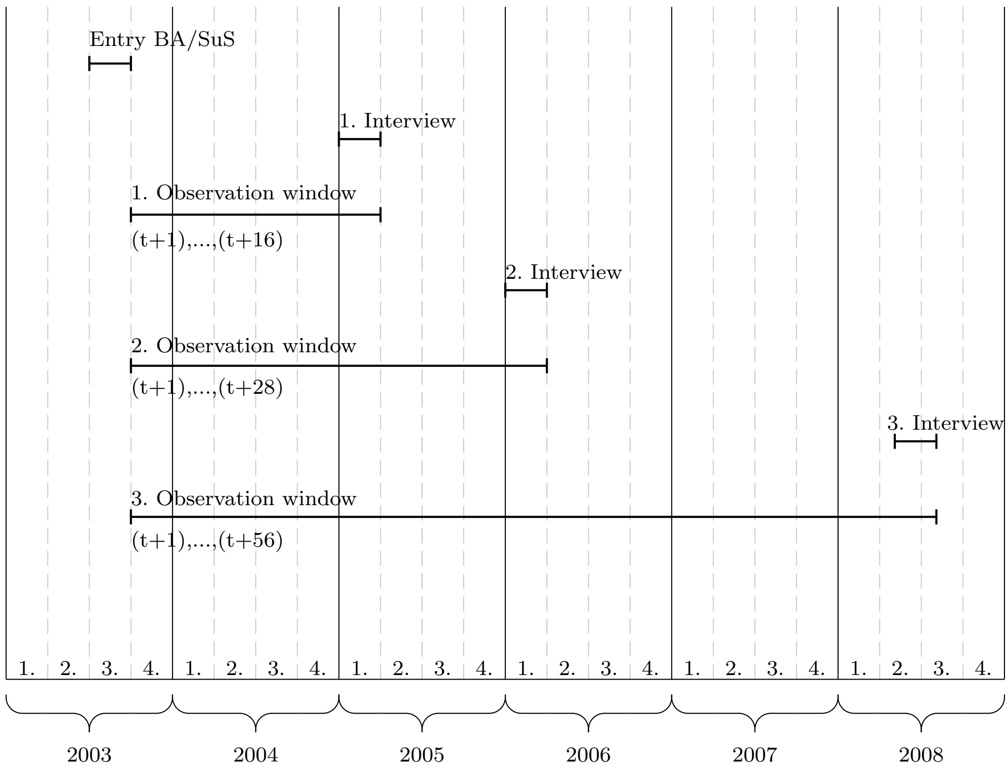
Therefore, participants in each program who became self-employed in the third

[14] See Caliendo and Kritikos (2009) for information on the features of the new program and a critical discussion of its introduction in August 2006, and Caliendo et al. (2012) for information with respect to the reform in November 2011.

[15] Having access to only one particular quarter of entrants bears the risk of a selective sample. However, comparing the distribution of certain characteristics (e.g. age and educational background) across different quarters does not show any significant differences.

[16] Therefore, we only briefly discuss the basic data construction and refer to Baumgartner and Caliendo (2008) for a more extensive discussion of the data issues.

Figure 2.1: Survey Design



quarter of 2003 are randomly drawn. The comparison group is restricted to those who were unemployed in the third quarter of 2003, eligible to participate in either of the two programs, but did not join a program in this quarter. However, controls are allowed to participate in ALMP programs afterwards.[17] Starting from the entry cohort, the third quarter of 2003, three interviews were conducted. As depicted in Figure 2.1, the first two interviews took place in January/February of 2005 and 2006 while the third and final interview was conducted in May/June of 2008. In total, the data allow us to follow individuals up to five years after start-up.

Table 2.4 provides the number of observations (realized interviews) after the final interview wave was completed. In total, we have 2,817 participants and 2,214 non-participants available for the empirical analysis. We further see that the data provide sufficient number of observations in different cells which allow us to run the empirical analysis separately for men and women in East and West Germany.

[17]The actual number of non-participants who participated in ALMP programs after the third quarter 2003 is rather low. Approximately 15% of all non-participants were assigned to programs of ALMP and only 2% participated in SUS or BA within our observation period.

Table 2.4: Number of Observation at the Third Interview

	Total	West Germany Men	West Germany Women	East Germany Men	East Germany Women
Participants	2,817	1,266	679	550	322
Start-up subsidy	1,351	486	448	231	186
Bridging allowance	1,466	780	231	319	136
Non-participants	2,214	929	591	423	271

Thereby, women in East Germany are the smallest group in our sample where we still have 186 (136) SUS (BA) participants and 271 non-participants available.

The implementation of several interviews has the advantage that the time horizon between those interviews can be minimized which makes it is easier for the respondents to remember past labor market activities. This decreases measurement error and makes longitudinal information more reliable. However, it has the disadvantage that individuals have to be contacted more often which increases the likelihood that they drop out of the survey. In our data we observe a panel attrition of 54% on average among participants and 61% among non-participants. The lower attrition among participants is due to the program link, i.e., participants received monetary support and therefore feel more obliged to the survey than non-participants. The figures appear to be high on a first view, however, one has to take into account that individuals have been contacted three times over a five year horizon. To make sure that the panel attrition does not introduce a bias in our analysis, we check the results with respect to selection due to panel attrition. We find positive selection, i.e., individuals who perform relatively well in terms of labor market outcomes are more likely to respond. Therefore, we use *sequential inverse probability weighting* to adjust for this selective attrition. Under the assumption that the selection process is due to observable characteristics, this procedure is $\sqrt{N}$-consistent (see Wooldridge, 2002). We emphasize though that the correction process is only required for descriptive results. The matching results later on, i.e., the comparison between participants and non-participants, rely on unweighted outcome variables because participants and non-participants are similarly affected by selection due to panel attrition by what the bias cancels out.

2.5 Empirical Strategy

In order to estimate causal effects, we base our analysis on the potential outcome framework, also known as the Roy (1951) - Rubin (1974) model. The two potential outcomes are Y^1 (individual receives treatment, $D = 1$) and Y^0 (individual does not receive treatment, $D = 0$). The observed outcome for any individual i can be written as: $Y_i = Y_i^1 \cdot D_i + (1 - D_i) \cdot Y_i^0$. The treatment effect for each individual i is then defined as the difference between her potential outcomes: $\tau_i = Y_i^1 - Y_i^0$. Since we can never observe both potential outcomes for the same individual at the same time, the fundamental evaluation problem arises. We will focus on the most prominent evaluation parameter, which is the average treatment effect on the treated (ATT), and is given by:

$$\tau_{ATT} = E(Y^1 \mid D = 1) - E(Y^0 \mid D = 1). \tag{2.1}$$

The last term on the right hand side of equation (2.1) describes the hypothetical outcome without treatment for those individuals who received treatment. Since the condition $E(Y^0 \mid D = 1) = E(Y^0 \mid D = 0)$ is usually not satisfied with non-experimental data, estimating ATT by the difference in sub-population means of participants $E(Y^1 \mid D = 1)$ and non-participants $E(Y^0 \mid D = 0)$ will lead to a selection bias. This bias arises because participants and non-participants are selected groups that would have different outcomes, even in the absence of the program due to observable or unobservable factors.[18] We apply propensity score matching and thus rely on the conditional independence assumption (CIA), which states that conditional on observable characteristics (W) the counterfactual outcome is independent of treatment: $Y^0 \amalg D|W$, where $\amalg$ denotes independence. In addition to the CIA, we also assume overlap: $Pr(D = 1 \mid W) < 1$, for all W. This implies that there is a positive probability for all W of not participating, i.e., that there are no perfect predictors which determine participation. These assumptions are sufficient for identification of the ATT based on matching (MAT), which can then be written as:

$$\tau_{ATT}^{MAT} = E(Y^1|W, D = 1) - E_W[E(Y^0|W, D = 0)|D = 1], \tag{2.2}$$

where the first term can be estimated from the treatment group and the second term from the mean outcomes of the matched comparison group. The outer expectation is taken over the distribution of W in the treatment group.

[18] See, for example Caliendo and Hujer (2006) for further discussion.

As direct matching on W can become hazardous when W is of high dimension ("curse of dimensionality"), Rosenbaum and Rubin (1983) suggest using balancing scores $b(W)$. These are functions of the relevant observed covariates W such that the conditional distribution of W given $b(W)$ is independent of the assignment to treatment, that is, $W \amalg D|b(W)$. The propensity score $P(W)$, i.e., the probability of participating in a program, is one possible balancing score. For participants and non-participants with the same balancing score, the distributions of the covariates W are the same, i.e., they are balanced across the groups. Hence, the identifying assumption can be re-written as $Y^0 \amalg D|P(W)$ and the new overlap condition is given by $Pr(D = 1 \mid P(W)) < 1$.

The CIA is clearly a very strong assumption and the applicability of the matching estimator depends crucially on its plausibility. Blundell et al. (2005) argue that the plausibility of such an assumption should always be discussed on a case-by-case basis. Only variables which simultaneously influence the participation decision and the outcome variable should be included in the matching procedure. Hence, economic theory, a sound knowledge of previous research, and information about the institutional setting should guide the researcher in specifying the model (see, e.g., Smith and Todd, 2005 or Sianesi, 2004). We use both administrative and survey data, which enables us to control for numerous individual information and labor market conditions. Based on this exhaustive data, we argue that the CIA holds in our application. However, we test the sensitivity of the results with respect to time-invariant unobserved differences between participants and non-participants by implementing conditional *difference-in-differences* (DID). This allows for unobservable but temporally invariant differences in outcomes between participants and non-participants, which obviously relaxes the CIA. Conditional DID was initially suggested by Heckman et al. (1998). It extends the conventional DID estimator by defining outcomes conditional on the propensity score and using semiparametric methods to construct the differences. If the parameter of interest is ATT, the conditional DID estimator is based on the following identifying assumption:

$$E[Y_t^0 - Y_{t'}^0|P(W), D = 1] = E[Y_t^0 - Y_{t'}^0|P(W), D = 0], \tag{2.3}$$

where (t) is the post-treatment and (t') the pre-treatment period. It also requires the common support condition to hold and can be written as:

$$\tau_{ATT}^{CDID} = E(Y_t^1 - Y_{t'}^0|P(W), D = 1) - E(Y_t^0 - Y_{t'}^0|P(W), D = 0). \tag{2.4}$$

For identification of causal effects, any general equilibrium effects need to be excluded, that is treatment participation of one individual can not have an impact on outcomes of other individuals. This assumption is referred to as *stable-unit-treatment-value-assumption* (SUTVA). Imbens and Wooldridge (2009) argue that the validity of such an assumption depends on the scope of the program as well as on resulting effects. They infer that for the majority of labor market programs, the SUTVA is potentially fulfilled because such programs are usually of small scope with rather limited effects on the individual level. We follow their argumentation and refer to Table 2.3, where we see that entries into SUS and BA are approximately of the same scope as other ALMP programs and in relation to the total number of entries into unemployment of 5.5 million in 2004 quite small.

2.6 Main Analysis: Long-term Evidence

After having set the stage and explained the identification strategy, we start the empirical analysis by providing first of all evidence on the general effectiveness of start-up subsidies for the unemployed. The aim is to isolate the program effect from other distorting effects such as labor supply decisions of individuals and variations in labor demand due to macroeconomic conditions. Therefore, we restrict the sample to men in West Germany only. Men (in contrast to women) are more likely to look for full-time employment and to be self-employed, and West Germany is characterized by better labor market conditions than East Germany. By this restriction we avoid several side-effects, such as labor supply decisions, macroeconomic constraints and so on. Later on we do relax this restriction and look at the particular case of unemployed women and the role of start-up subsidies (see Section 2.8).

Table 2.4 provides the number of realized interviews for men in West Germany. For the analysis we have 486 participants in SUS, 780 recipients of BA and 929 non-participants available.

2.6.1 Descriptive Evidence

Table 2.15 in the Appendix provides descriptive statistics measured at entry into program in the third quarter of 2003 separately for male participants (SUS and BA) and non-participants in West Germany. Participants in SUS are on average younger and lower educated individuals with less employment duration and lower earnings in the past. This is in line with our expectations, as the financial support in case of BA depends on previous earnings and is only paid for a short period of six months. Hence, individuals with low earnings in the past are only eligible to minor support if they choose BA. It is therefore rational for those individuals to choose SUS because the subsidy is small but it might be extended up to three years. On the other hand, individuals with higher earnings want to secure their high entitlement and, consequently, choose BA. BA participants in our sample received on average €2,056/month and 89% of the SUS participants received the subsidy for three years. Moreover, in terms of location participants seem to be equally distributed throughout West Germany. As pointed out in previous research (e.g. Dunn and Holtz-Eakin, 2000), we find that self-employment is influenced by intergenerational transmission, i.e., the fraction with parental self-employment among participants is higher than among non-participants.

In Table 2.5 we provide the labor market status of participants and non-participants after 28 and 56 months following start-up and the monthly income after 56 months. As mentioned before, all descriptive results are weighted using *sequential inverse probability weighting* to adjust for the selection process due to panel attrition (see Wooldridge, 2002). First of all, a closer look at the labor market developments of participants reveals that the fraction of self-employed individuals decreases from 71.5% to 67.9% for former BA recipients and from 67.6% to 59.7% for firms initially supported by SUS. Hence, the decline in self-employment is more than twice as high for SUS (-7.9 percentage points) than for BA (-3.6 percentage points) in the given period. This is mainly due to the fact that SUS expired between the second and third interview; whereas BA support had already stopped after six months, that was before the first interview took place. The sharp drop in self-employment rates after the end of the subsidy period may be seen as indication that some businesses were only able to survive with the help of the subsidy.

However, the main objective of ALMP is not primarily to create self-employment but to integrate unemployed individuals into the labor market. Hence, we now consider the share of individuals either in self-employment or regular employment. After 56 months since start-up, we find about 81% of SUS and 89% of former BA participants well integrated in the labor market. For non-participants, only 63% are either self-employed or regular employed. Hence, we observe a raw difference of employment rates of about 20% between participants and non-participants. These are descriptives only and the gap is potentially caused by differences in key variables.

Table 2.5: Descriptive Evidence on Labor Market Status and Income

	Start-up Subsidy	Bridging Allowance	Non-Participants
Labor market status			
2nd interview (January/February 2006)			
Self-employed	67.6	71.5	12.7
Regular employed	11.7	14.0	35.9
Unemployed or in ALMP	15.2	11.1	35.9
Others	5.6	3.4	15.5
3rd interview (May/June 2008)			
Self-employed	59.7	67.9	14.1
Regularly employed	20.9	21.1	49.1
Unemployed or in ALMP	11.7	6.7	19.9
Others	7.6	4.3	16.9
Income[a)] at 3rd interview (May/June 2008)			
Total income	1,672.0	2,336.0	1,581.1
	(1,720.4)	(1,962.9)	(1,601.6)
	[1,276.3]	[1,942.3]	[1,338.0]
Working income	1,498.5	2,167.4	1,302.8
	(1,780.2)	(2,006.3)	(1,662.5)
	[1,145.3]	[1,815.2]	[1,190.1]
Household members	1.6	1.8	1.6
Equivalent income[b)]	1,678.2	2,020.6	1,458.4
	(1,907.8)	(1,809.4)	(1,560.4)
	[1,236.7]	[1,602.6]	[1,135.6]

Note: Men in West Germany. Numbers are percentages unless otherwise stated.
[a)] Income is measured as average monthly net income in euros; standard deviation and median are provided in parentheses and square brackets respectively.
[b)] The equivalent income is calculated by adjusting the household income by the number of household members. The household income is divided by the weighted number of household members. Following the actual OECD equivalence scale, the household head achieves a weight of one, all children below the age of 15 are weighted with 0.3 and everybody else with 0.5 (see Whiteford and Adema, 2007). Since we only observe the total number of household members, every household member beside the household head receives a weight of 0.4.

Therefore, we need an identification strategy to estimate causal effects. We apply propensity score matching that relies on the conditional independence assumption as discussed in Section 2.5. The results of the causal analysis are finally presented in Section 2.6.3.

With respect to another objective of ALMP, the achievement of certain income levels for participants, we also provide in Table 2.5 net incomes (measured 56 months after start-up). Next to working income, the total income captures transfer payments such as unemployment benefit, pension, or child benefit and the equivalent income takes the number of household members into account.[19] We can see that former BA recipients have higher income in terms of working, total and equivalent income compared to SUS participants. This is not surprising because of the aforementioned selection into BA of highly educated individuals with high earnings in the past. It is also noticeable that non-participants earn on average less than participants; however considering the median of the income distribution, the difference to SUS participants almost vanishes.

Table 2.6: Comparison to Previous Dependent Employment

	Start-up Subsidy	Bridging Allowance
Type of activity	0.6	0.5
Income	0.2	0.2
Promotion prospects	0.5	0.5
Workload	-0.1	-0.1
Working time	-0.2	-0.3
Social security	-0.2	-0.3

Note: Men in West Germany. Only self-employed individuals after 56 months since start-up. Scale: Improved (1), Unchanged (0), Declined (-1).

Finally, to answer the question whether participants are more satisfied with their employment status compared to previous dependent employment, Table 2.6 provides some evidence on job satisfaction among participants who are self-employed at the third interview. The respondents were asked to compare their self-employment with the previous employment spell with respect to different aspects. Thereby, positive values indicate an overall improvement while negative values depict a decline. For participants in both programs, the situation improved in terms of type of activity, income and promotion prospects but declined for measures such as workload, working time and social security. However, the improvement among the first three measures is obviously more valued by individuals than the decrease in the latter because of higher absolute values.

[19] The equivalent income is calculated by adjusting the household income by the number of household members. According to the actual OECD equivalence scale, the household head achieves a weight of one, all children below the age of 15 are weighted by 0.3 and everybody else with 0.5 (see Whiteford and Adema, 2007). Since we are only able to observe the total number of household members, we assign a weight of 0.4 to every household member beside the household head.

2.6.2 Estimation Procedure

After having presented descriptive evidence, we proceed with the estimation of causal effects. As described in Section 2.5, we apply propensity score matching for which we have to estimate the propensity scores for participation in the respective program versus non-participation in a first step. Therefore, we use *probit*-estimation. We test different specifications following economic theory and previous empirical findings as discussed above. But we also check econometric indicators such as significance of parameters or *pseudo*-R^2 to find the final specification.[20] The results of the *probit*-estimation can be found in Table 2.16 in the Appendix. Let us briefly discuss the main components that influence the selection into treatment. In particular, variables such as age, duration of previous unemployment, regional cluster, information with respect to previous earnings and the intergenerational transmission turn out to be most important for the selection into SUS. In the case of "BA vs. NP", the duration of previous unemployment, indicators for the labor market history and also parental self-employment have a significant impact. This actually confirms our expectation that individuals with higher previous earnings are more likely to choose BA. In addition, we also provide the distribution of the estimated propensity scores in the upper part of Figure 2.8 in the Appendix. As we can see, the distribution of the propensity scores are biased towards the tails, that is participants have a higher probability on overage of becoming self-employed than non-participants. Nevertheless, participant's propensity score distribution overlaps the region of the propensity scores of non-participants completely; therefore, the overlap assumption is fulfilled.

In the next step we estimate the average treatment effects on the treated as depicted in Equation 2.2. In order to increase efficiency and being able to apply bootstrapping for inference we use a *kernel* matching algorithm.[21] To assess the matching quality, that is, whether the matching procedure balances the distribution of observable variables between participants and non-participants, Table 2.17 summarizes different quality measures.[22] First of all, we provide in the upper part

[20]For a more extensive discussion on the estimation of propensity scores, we refer to Heckman et al. (1998) and Caliendo and Kopeinig (2008) among others.

[21]More specifically, we apply an *Epanechnikov Kernel* with an bandwidth of *0.06.* We run different matching algorithm and find that our results are not sensitive. Furthermore, we applied *inverse probability weighting* (IPW) as an alternative approach for estimating ATT, as suggested by Imbens (2004). This method also relies on the CIA. Using IPW, we find hardly any substantial differences for the employment effects but slightly higher income effects.

[22]For a more intensive discussion with respect to assessing the matching quality, we refer to Caliendo and Kopeinig (2008).

the number of variables which differ significantly between participants and non-participants by using a *t-test*.[23] For instance, we can see that for SUS, 28 variables have a mean that is significantly different between treated and non-treated at the 5% level before matching takes place. In the matched sample in turn, only two variables are significantly different for treated and non-treated individuals. In fact, in the case of BA after matching, we find no significant differences at all. This indicates that matching has been successful. Since using a *t-test* to assess the matching quality does not tell us anything about the bias reduction, we also provide the *mean standardized bias* (MSB) and the number of variables with a standardized bias of a certain amount. It can be seen that in case of "SUS vs. NP" ("BA vs. NP") the MSB declines from initially 14.6% to 3.5% (8.6% to 2.2%) after matching, where a MSB below 3% to 5% generally indicates a success of the matching approach (Caliendo and Kopeinig, 2008). Finally, we also re-estimate the propensity scores within the matched sample, as suggested by Sianesi (2004). The distribution of covariates should be well balanced within the matched sample and hence the resulting *pseudo*-R^2 from the propensity score estimation should be rather low. In fact, we do observe a sharp drop in *pseudo*-R^2 for both programs also suggesting a successful matching.

2.6.3 Results

The aim of the programs is to integrate unemployed individuals in the labor market and to increase income levels. Therefore, we use different outcome variables for the calculation of causal effects. We employ "not unemployed" and "self-employed or regular employed" as binary outcome variables to measure the degree of labor market integration. This is due to two reasons. First, non-participants are less likely to become self-employed than participants; and hence, comparing participants and non-participants with respect to self-employment only would bias the causal effects upwards. Second, the main objective of ALMP is to integrate individuals into the labor market which includes being regular employed as a success. We also want to highlight that being not registered as unemployed captures an upper bound estimation for the degree of labor market integration, i.e., independence of unemployment or social benefits. The binary outcome variables take on the value one if the individual is either "not unemployed" or "self-employed or regular employed"

[23]We consider the distribution of observable characteristics between participants and non-participants before and after matching based on 56 variables in total.

and zero otherwise.[24] Moreover, we examine whether program participation leads to an increase in income levels.

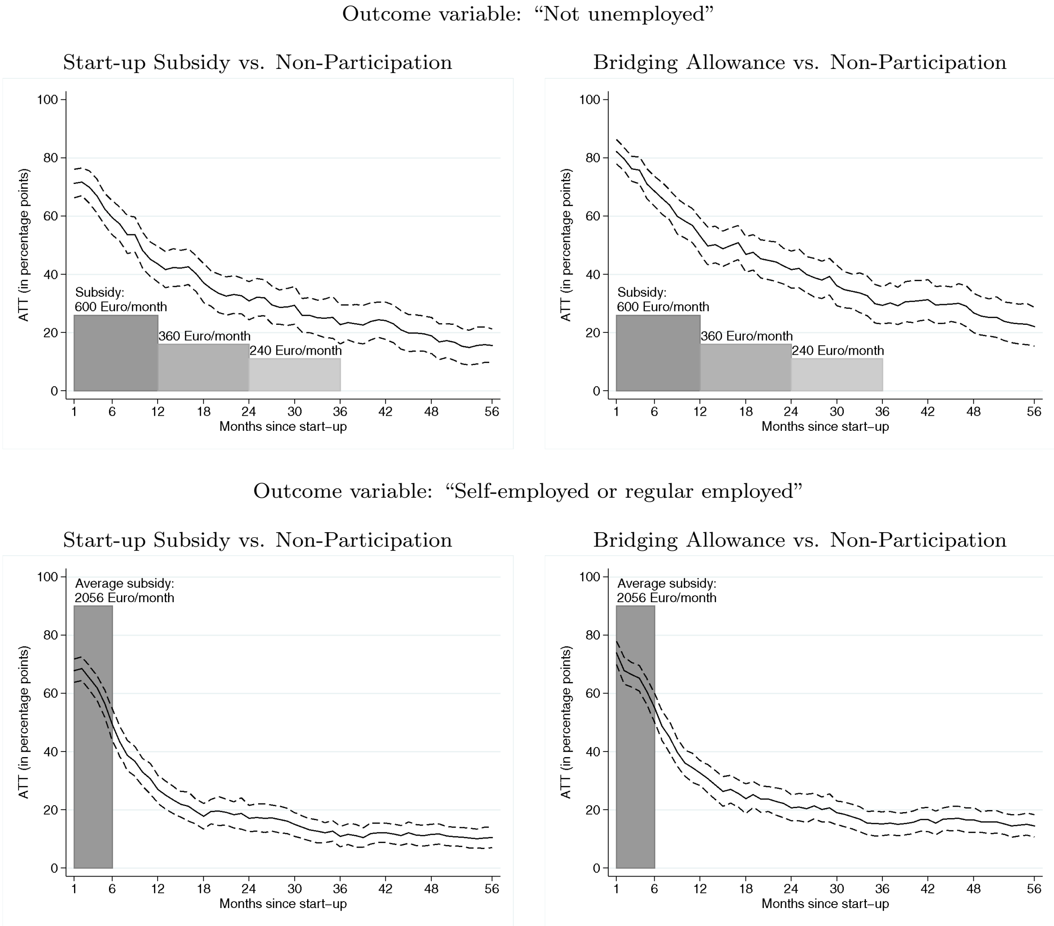
Figure 2.2 shows the average treatment effect on the treated as defined in Equation 2.2 over time and Table 2.7 provides the corresponding exact values for selected points in time. As one can see in Figure 2.2, the effects are positive and significant at all times for either outcome variable.[25] To be precise, 56 months after start-up, participants in SUS (BA) have a 15.6% (10.6%) higher probability of not being registered as unemployed compared to non-participants. Regarding integration into the labor market, that is being either self-employed or regular employed, we detect that the employment probability of participants is 22.1 percentage points higher for SUS and 14.5 percentage points for BA participants in comparison to non-participants. These strong positive long-run effects are remarkable compared to findings of evaluation studies investigating other programs of ALMP in Germany, such as vocational training or job creation schemes.

Moreover, for BA the positive effect seems to be rather stable after three years following start-up, indicating that either surviving firms or employed individuals are well integrated in the (labor) market. For individuals supported with SUS, we do not find such a convergence. We argue that due to financial support which lasted longer, the adjustment process at the market is still ongoing. Because of this and the fact that the control group for BA participants is more competitive in the labor market than the assigned control group for SUS participants, the higher effects for SUS can not be directly contrasted to the results of BA participants. In Table 2.7, we also provide the cumulated effects over time which reveal that within our observation period of 56 months, participants in SUS (BA) spent on average 23.5 (14.6) months more in self-employment or regular employment than non-participants. One may argue that cumulating the effects over the entire period will capture locking-in effects and lead to an overestimation of the effects, since participants received financial support. We take care of this by providing "partly" cumulated effects, for which we cumulate the effects only over the period after financial support ended. For the case of SUS, we find that participants are still on average 5.5 months longer self-employed or regular employed than non-participants which actually depicts 20% of the post-

[24] We define individuals who are neither registered as unemployed nor in a program of active labor market policy (except the two start-up subsidies) as being "not unemployed". Moreover, individuals who are either employed subject to social security contributions or self-employed are treated as "self-employed or regular employed".

[25] In addition, Figure 2.9 in the Appendix depicts the causal effects of both programs and the respective gross levels for participants and matched non-participants over time.

Figure 2.2: Causal Effects of Start-up Subsidy and Bridging Allowance Over Time



Note: Depicted are average treatment effects on the treated (solid line), i.e., the difference in outcome variables between male participants and non-participants in West Germany. In addition, we provide 5% confidence intervals (dashed lines), which are based on *bootstrapped* standard errors with 200 replications. The duration and the amount of financial support are indicated by shaded bars. Due to institutional settings, the start-up subsidy amounted to €600/month, €360/month and €240/month in the first, second and third year; while the average subsidy in the case of bridging allowance was €2,056 paid for six months only. Thereby, the average subsidy is calculated by taking the average monthly unemployment benefit level (€40/day times 30.5 days) plus 68.5% for social security liabilities.

program period of 20 months. For BA participants, we find a partly cumulated effect of 10.8 months, which is also 20% of the remaining period (of 50 months in this case).

To shed light on the question of income gains for participants, we provide the causal effects for income differences at the end of the observation period at the bottom of Table 2.7. We use three income-related outcome variables: The most relevant one is monthly net income from self-employment or paid employment (working income). However, since it is often argued that differences between (low) labor income and unemployment benefits are especially low in Germany, we will also look at the total personal income of individuals, that is, including transfer

Table 2.7: Causal Effects of Start-up Subsidy and Bridging Allowance

	Start-up Subsidy vs. Non-Participation	Bridging Allowance vs. Non-Participants
Outcome variable: "Not unemployed"		
Difference in percentage points		
After 6 months	59.4 (3.0)	49.3 (2.8)
After 36 months	22.9 (3.4)	10.9 (1.8)
After 56 months	15.6 (2.9)	10.6 (1.8)
Difference in months		
Total cumulated effect ($\sum_{t=1}^{56}$)	18.7 (1.3)	12.2 (0.8)
Partly cumulated effect$^{a)}$	3.9 (0.6)	8.5 (0.7)
Outcome variable: "Self-employed or regular employed"		
Difference in percentage points		
After 6 months	68.5 (2.6)	55.0 (2.5)
After 36 months	29.4 (3.3)	15.3 (2.1)
After 56 months	22.1 (3.4)	14.5 (1.9)
Difference in months		
Total cumulated effect ($\sum_{t=1}^{56}$)	23.5 (1.3)	14.6 (0.9)
Partly cumulated effect$^{a)}$	5.5 (0.6)	10.8 (0.9)
Outcome variable: "Income 56 months after start-up"		
Difference in €/month		
Working income	435 (135)	618 (110)
Total income	270 (121)	485 (110)
Equivalent income$^{b)}$	248 (151)	546 (92)

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between male participants and non-participants in West Germany. We define individuals who are neither registered as unemployed nor in a program of active labor market policy (except the two start-up subsidies) as being "not unemployed". Moreover, individuals who are either employed subject to social security contribution or self-employed are treated as "self-employed or regular employed". Standard errors are in parentheses and are based on *bootstrapping* with 200 replications.
$^{a)}$ SUS: $\sum_{t=37}^{56}$, BA: $\sum_{t=7}^{56}$
$^{b)}$ See Table 2.5 for definition of equivalent income.

payments such as unemployment and child benefits. Finally, in order to take the household size into account we additionally calculate the effects on the equivalent income. The results unambiguously show that participants earn significantly more than non-participants. Participants in SUS (BA) have on average a net working income which is €435 (€618) higher per month than the one of non-participants at the end of our observation period. If we look at the total income participants still have a higher income than non-participants (€270 for SUS and €485 for BA). Finally, looking at the equivalent income also shows that participants in SUS (BA) earn on average €248 (€546) more than non-participants.

In summary, our results suggest that supporting unemployed individuals by SUS or BA has been a success in terms of both employment prospects as well as income measures compared to non-participation. The employment effects at the end of our observation period and cumulated over time are substantial and so are the income effects. Relating the working income effects to the average monthly net working income of non-participants (compare Table 2.5) shows that these are economically very significant gains of around 28% to 39%.

2.6.4 Sensitivity Analysis

After having presented strong positive effects for both programs, we now need to check the robustness of our results with respect to deviations from the identifying assumption. If participants and non-participants differ in terms of unobserved characteristics, the CIA is violated and therefore our results would be biased. Since it is not possible to test the CIA directly with non-experimental data, we assess the sensitivity of our results in four different directions. First, we extend the set of variables in the propensity score estimation in order to see whether this has an impact on the causal estimates. Second, we allow for time-invariant unobserved differences between participants and non-participants and re-estimate the effects on employment and income. Third, we examine how strong an unobserved component would need to be in order to undermine the results from our analysis. Fourth, we estimate the effects for different sub-sets of the population where participants and non-participants are most comparable.

Extending the Set of Variables in the Propensity Score

Previous research has shown that entrepreneurs differ in various aspects from the general population. They are more likely to be male, higher educated and have self-employed parents. Clearly, this can also be true for our treatment groups and that is why we control for such characteristics in the propensity score estimation. However, there might still be personality traits which are not captured by the set of variables we control for. "Animal spirits" in the Schumpeterian sense will probably be more pronounced within the treatment group, even after controlling for observed characteristics and previous labor market experience. One often cited and used proxy for such spirits are attitudes towards risk. The influence of risk aversion on the decision to become self-employed is a much discussed topic in the entrepreneurial literature. Conventional wisdom asserts that the role model of an entrepreneur requires to make risky decisions in uncertain environments and hence that more risk-averse individuals are less likely to become an entrepreneur. Caliendo et al. (2009) use experimentally-validated measures of risk attitudes in the most recent waves of the German Socio-Economic Panel (SOEP) to examine whether the decision of starting a business is influenced by objectively measurable risk attitudes at the time when this decision is made. The authors show that in general individuals with lower risk aversion are more likely to become self-employed.

In the second interview wave (28 months after start-up) of our data risk attitudes of participants and non-participants were elicited in a similar way as in the SOEP. Respondents were asked for attitudes towards risk in general and could indicate their willingness to take risks on an eleven-point scale ranging from zero (complete unwillingness) to ten (complete willingness). Table 2.15 in the Appendix shows, that there are clear differences in the risk attitudes between participants and non-participants. Whereas participants have an average of 5.8, non-participants have an average of 5.5. Furthermore, 42% of the participants answer “7 or more” whereas this is only true for 33% of the non-participants.

Including this variable in the propensity score estimation is not without critique, since it was elicited 28 months after the decision to join the program and start a business. Hence, reverse causality might be an issue here, where the experience in the 28 months between starting the business and the interview taking place might have an influence on the attitudes towards risk. This is why we do not include risk attitudes in the final propensity score estimation in the previous section. However, most of the recent research (see, e.g., Dohmen et. al, 2007) claims that risk attitudes are stable over time such that this might be less problematic. For the sensitivity analysis we have therefore included this variable in the propensity score estimation and replicated the full analysis. The variable is highly significant in the score estimation and we present the additional matching results in Panel A of Table 2.8 (employment effects) and Table 2.9 (income effects).[26] Comparing the new results with the baseline results from before (compare Table 2.7) we see that inclusion of the new variable “risk attitudes” lowers the effects slightly. For example, the effect on the outcome variable “self-employed or regular employed” after 56 months falls from 22.1% to 21.1% for SUS participants and the total cumulated effect goes down from 23.5 months to 23.4 months. For the BA participants the change is even smaller and slightly positive. Overall, we can conclude that adding this essential new variable “risk attitudes” does not change our results.

[26]Full propensity score estimation results (and distributions) are available in Table 2.18 (and Figure 2.8) in the Appendix.

Table 2.8: Sensitivity Analysis – Causal Effects of Start-up Subsidy and Bridging Allowance: Employment Effects

	Start-up Subsidy vs. Non-Participation		Bridging Allowance Non-Participation	
	Outcome variables:			
	"Not UE"	"SE or RE"	"Not UE"	"SE or RE"
Main results (see Table 2.7)				
Effect after 56 months (in %-points)	15.6 (2.9)	22.1 (3.4)	10.6 (1.8)	14.5 (1.9)
Total cumulated effect ($\sum_{t=1}^{56}$)	18.7 (1.3)	23.5 (1.3)	12.2 (0.8)	14.6 (0.9)
Partly cumulated effect$^{a)}$	3.9 (0.6)	5.5 (0.6)	8.5 (0.7)	10.8 (0.9)
A) Alternative specification of the propensity score estimation				
Extended specification including risk attitudes				
Effect after 56 months (in %-points)	14.5 (3.2)	21.1 (3.4)	10.6 (1.8)	14.8 (2.1)
Total cumulated effect ($\sum_{t=1}^{56}$)	18.4 (1.2)	23.4 (1.3)	12.2 (0.8)	14.9 (0.9)
Partly cumulated effect$^{a)}$	3.7 (0.5)	5.3 (0.7)	8.5 (0.8)	11.0 (0.8)
B) Difference-in-Difference				
Total cumulated effect ($\sum_{t=1}^{56}$)				
DID1	16.9 (1.5)	21.7 (1.4)	11.7 (0.7)	14.1 (0.9)
DID2	17.7 (1.2)	22.6 (1.3)	12.2 (0.8)	14.6 (0.9)
DID3	17.9 (1.3)	22.7 (1.4)	11.7 (0.7)	14.1 (0.9)
Partly cumulated effect$^{a)}$				
DID1	2.1 (1.2)	3.7 (1.0)	8.0 (0.7)	10.2 (0.8)
DID2	2.9 (0.7)	4.5 (0.8)	8.5 (0.7)	10.8 (0.8)
DID3	3.1 (0.8)	4.6 (0.7)	8.0 (0.7)	10.2 (0.9)
C) Common support condition				
Thick support 1 – $0.33 < \hat{P}(W) < 0.67$				
Effect after 56 months (in %-points)	18.0 (4.0)	22.0 (4.4)	13.5 (2.1)	17.9 (2.7)
Total cumulated effect ($\sum_{t=1}^{56}$)	19.1 (1.5)	23.6 (1.6)	12.9 (0.9)	16.1 (1.1)
Partly cumulated effect$^{a)}$	4.0 (0.7)	5.5 (0.7)	9.2 (0.9)	12.2 (1.0)
Thick support 2 – $F(\hat{P}(W) > 5\%)$				
Effect after 56 months (in %-points)	17.7 (2.7)	21.3 (3.3)	13.8 (1.7)	18.4 (2.1)
Total cumulated effect ($\sum_{t=1}^{56}$)	18.9 (1.1)	22.0 (1.1)	13.4 (0.8)	16.5 (1.0)
Partly cumulated effect$^{a)}$	4.0 (0.5)	4.9 (0.6)	9.8 (0.8)	12.6 (0.9)
Optimal subpopulation				
Effect after 56 months (in %-points)	15.0 (3.1)	21.1 (3.5)	11.1 (1.6)	15.3 (1.9)
Total cumulated effect ($\sum_{t=1}^{56}$)	17.9 (1.2)	22.9 (1.4)	12.4 (0.8)	14.9 (0.9)
Partly cumulated effect$^{a)}$	3.7 (0.6)	5.2 (0.6)	8.7 (0.7)	11.1 (0.9)

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between male participants and non-participants in West Germany. Thereby the outcome variable "not unemployed" is depicted by "Not UE" and "self-employed or regular employed" by "SE or RE". All results are differences in months unless otherwise stated. Standard errors are in parentheses and are based on *bootstrapping* with 200 replications.
Alternative specification of the propensity score estimation: The extended specification contains risk attitudes in addition to the final specification.
Difference-in-Difference: The reference levels for the pre-treatment period are defined as follows: DID1: July 1998 - June 2003; DID2: January 2001 - June 2003; DID3: July 1998 - Dec. 2000.
Common support condition - Thick Support: We estimate the effects *(1)* in a region defined by $0.33 < \hat{P}(W) < 0.67$. Moreover, we divide the propensity score distribution into ten deciles and estimate the effects *(2)* only in regions where we have a density of at least 5% ($F(\hat{P}(W) > 5\%)$) in both groups (participants and non-participants) respectively. A detailed Table with the distribution of participants and non-participants along the propensity score distribution is available in the supplementary appendix.
Common support condition - Optimal subpopulation: The analysis is restricted to a subset of the original sample by dropping individuals with covariate values that are outside the optimal common support range (see Crump et al., 2009).
$^{a)}$ SUS: $\sum_{t=37}^{56}$, BA: $\sum_{t=7}^{56}$

Conditional Difference-in-Differences

As already outlined in Section 2.5 we also test the sensitivity of our results with respect to time-invariant unobserved heterogeneity by using a conditional difference-in-differences approach. Before using such an approach, one has to determine the reference level for the before/after difference (see equation 2.4). For the outcome variables "not unemployed" and "self-employed or regular employed" we choose three different time periods for the comparison. In the first approach (DID1) we use the time period from July 1998 to June 2003, that is, the five-year employment history before entering the program. For the first outcome variable, we sum the months not spent in unemployment, whereas for the second, we sum the months spent in paid employment. Additionally, we restrict the reference period to the latest 2.5 years (DID2, January 2001-June 2003) as well as the earliest 2.5 years (DID3, July 1998 to December 2000). For the DID procedure with the income variables we use two reference levels: First, the average monthly income from regular employment in 2002 for the working income comparison (DID4) and second, the average monthly income in 2002 for the total income comparison (DID5).

Panel B in Tables 2.8 and 2.9 provides the cumulated employment effects and income effects for the conditional DID estimator. As we can see the results hardly differ from the matching estimates. For instance, for the case of "BA vs. NP" we find participants being on average 14.6 months longer in employment or self-employment than non-participants using the total cumulated effect (cf. Table 2.7). Using conditional DID, the results vary from 14.1 to 14.6. The income effects are also very close to the matching results. This evidence indicates that controlling for time-invariant unobserved heterogeneity does not add essential information and consequently suggests that the CIA seems to be a reasonable assumption for our analysis.

Bounding and Simulation Analysis

Since it is not possible to test the CIA directly with non-experimental data; we now use a *bounding approach* initially suggested by Rosenbaum (2002). This approach consists of simulating an unobserved component and testing to which degree of unobserved heterogeneity results are robust. It should be clear that this approach does not answer the question whether or not the CIA is fulfilled but conveys information on the robustness of the results with respect to unobserved heterogeneity. The

Table 2.9: Sensitivity Analysis – Causal Effects of Start-up Subsidy and Bridging Allowance: Income Effects

	Start-up Subsidy vs. Non-Participation		Bridging Allowance Non-Participation	
	Working income	Total income	Working income	Total income
Main results (see Table 2.7)	435 (135)	270 (121)	618 (110)	485 (110)
A) Alternative specification of the propensity score estimation				
Extended specification including risk attitudes	385 (153)	225 (149)	595 (117)	464 (118)
B) Difference-in-Difference	475 (130)	288 (139)	656 (128)	480 (128)
C) Common support condition				
Thick support 1 – $0.33 < \hat{P}(W) < 0.67$	226 (186)	114 (188)	588 (150)	468 (127)
Thick support 2 – $F(\hat{P}(W) > 5\%)$	307 (179)	168 (151)	583 (129)	461 (123)
Optimal subpopulation	410 (137)	257 (153)	613 (118)	480 (105)

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between male participants and non-participants in West Germany. Standard errors are in parentheses and are based on *bootstrapping* with 200 replications. All results are differences in €/month, measured 56 months after start-up.
Alternative specification of the propensity score estimation: The extended specification contains risk attitudes in addition to the final specification.
Difference-in-Difference: The reference levels for the pre-treatment period are defined as follows: The working income is measured as the average monthly income from employment in 2002 and the total income as the average monthly total income in 2002.
Common support condition - Thick Support: We estimate the effects *(1)* in a region defined by $0.33 < \hat{P}(W) < 0.67$. Moreover, we divide the propensity score distribution into ten deciles and estimate the effects *(2)* only in regions where we have a density of at least 5% ($F(\hat{P}(W) > 5\%)$) in both groups (participants and non-participants) respectively. A detailed Table with the distribution of participants and non-participants along the propensity score distribution is available in the supplementary appendix.
Common support condition - Optimal subpopulation: The analysis is restricted to a subset of the original sample by dropping individuals with covariate values that are outside the optimal common support range (see Crump et al., 2009).

main idea is that in the presence of unobserved factors, identical individuals with respect to observable characteristics (W_i) have different probabilities of receiving treatment. Therefore, an artificial factor Γ is introduced to simulate an unobserved term. The underlying test statistic then tests up to which extent this unobserved factor Γ will influence the significance of the results (see Becker and Caliendo, 2007, for more details on the implementation of the test procedure and the STATA module *mhbounds.ado*).

We find strong positive effects for both programs and therefore we are only interested in the test-statistic for the upper bound under the assumption that we have overestimated the treatment effect. In other words, if unobserved factors lead to positive selection, i.e., those who participate always have a higher employment probability even in the absence of treatment, the test statistic Q^+ will become insignificant for a certain value of Γ. To ease the interpretation we also provide respective p-values (p^+).

Table 2.19 summarizes test statistics separately for the outcome variables "not unemployed" and "self-employed or regular employed" and for "SUS vs. NP" and "BA vs. NP". We consider the outcome variables after 56 months since start-up in Table 2.19.[27] Below the detailed test-statistics and respective p-values we provide the exact values of Γ at which results turn insignificant. First of all, in the case of the absence of unobserved heterogeneity, that is $\Gamma = 1.0$, we can see that the test statistic for the upper bounds are significant throughout, indicated by $p^+ < 0.05$. Starting from that point, we stepwise increase the value of Γ. As mentioned above, this actually simulates an ascending influence of unobserved factors. For the comparison "BA vs. NP" results are very robust against strong unobserved selection bias; up to $\Gamma = 3$ results remain significant. This implies that unobserved factors would need to have twice the influence (on selection and outcomes) as W_i in order to undermine the results. For the comparison "SUS vs. NP" on the other hand, results are more sensitive with critical values of 1.25/1.30 at the 1%-level and 1.40/1.45 at the 5%-level after 56 months. While this does not mean that there is unobserved heterogeneity influencing our results, this does call for a cautious interpretation of the results for SUS.

Since these critical values are rather abstract, we implement in addition a *simulation approach* as suggested by Ichino et al. (2008) to further investigate the influence of potential unobserved heterogeneity. The basic idea is to simulate an unobserved variable (or confounder) by adapting the distribution of an observable variable. Since we exactly know the influence of the observable characteristics on outcomes and selection we have a direct linkage to the potential unobserved leverage for the interpretation. The results are shown in Table 2.20 in the Appendix where we concentrate on the effects on the outcome variable "self-employed or regular employed" after 56 months since start-up.[28] The first two columns show the effect of each confounder on the untreated outcome and on the selection into treatment. Thereby, a value below (above) one indicates a negative (positive) impact. The last column shows the resulting ATT given the existence of a confounder with a certain distribution. For instance, consider the effects for "SUS vs. NP" which are presented in the upper panel. In the absence of unobserved heterogeneity the impact on outcome and selection is zero and the ATT is 22 percentage points which is our baseline estimate from Table 2.7. If now an unobserved term is introduced which has the identical distribution as the age dummy "25 - 29 years", the influence

[27] We also conducted the test for different points in time but the results hardly differ.

[28] Additional results are available on request from the authors.

on outcome (2.24) and on selection (1.52) would be positive. This means that such an unobserved term would have a positive effect on being "self-employed or regular employed" 56 months after start-up in case of no treatment and also on being treated at all. Including this simulated unobserved confounder leads to an ATT of 22 percentage points which is identical to the ATT in the absence of unobserved heterogeneity. We tested other confounders such as "upper secondary school", "duration of previous unemployment" and "parental self-employment". Even for an unobserved term associated with a strong positive effect on selection into treatment such as parental self-employment, the ATT hardly changes (to 21 percentage points). The finding that the ATT is always almost identical to the baseline effects confirms the robustness of our results with respect to unobserved heterogeneity.

Thick and Optimal Common Support

The combined evidence of the sensitivity analysis so far suggests that the results are robust, but there may still remain concerns about any lingering selection on unobservables. Black and Smith (2004) show that such a lingering selection on unobservables will have its largest effects on bias for values of the propensity score in the tails of the distribution. This can be shown analytically (based on normality assumption of the joint error terms of the selection and outcome equations) but the underlying intuition is quite simple: when the probability of being in the treatment group is high, unobservable factors on average play a larger role than for probabilities near 0.5. This might lead to considerable selection bias if matching estimators must rely on the right tail of the distribution of propensity score in the comparison group. To deal with this, Black and Smith (2004) estimate the effects in a "thick support" region defined by $0.33 < \hat{P}(W) < 0.67$. We adopt their approach; additionally we divide the propensity score distribution into ten deciles and estimate the effects only in regions where we have a density of at least 5% in both groups respectively.[29] The results of both approaches are available in Tables 2.8 and 2.9 (Panel C). Our estimates based only on the "thick support" region of propensity scores around 0.5 are only slightly larger than those constructed using the full sample. The difference is a bit more pronounced for participants in BA where, e.g., the total cumulated effect on being self-employed or regular employed rises from 14.6 to 16.1 months. This difference could arise either from genuinely larger impacts in this region or lingering

[29] A detailed Table with the distribution of participants and non-participants along the propensity score distribution is available in Table 2.21 in the Appendix.

selection on unobservables which plays a bigger role outside the thick support region than within it. However, since the differences are quite small, lingering selection on unobservables does not seem to play a major role here. Using the second approach, i.e., restricting the analysis to regions where the propensity score density is above 5% for participants and non-participants, reduces the sample to the region $0.1 < \hat{P}(W) < 0.6$ for the SUS effects and $0.2 < \hat{P}(W) < 0.7$ for the BA effects. The results are also presented in Panel B and are very similar to the ones before.

Using the concept of "thick support" in this way means to restrict the propensity score distribution either arbitrarily or following a rule of thumb. Crump et al. (2009) suggest to base the common support decision rather on an objective measure. Restricting the propensity score distribution and hence excluding observations yields two opposing consequences for the variance term: while the variance increases due to the smaller sample size, the variance also decreases as participants with covariate values outside the range of the non-participants are excluded. They argue that the optimal common support is defined by balancing these two opposing variance components. To do so, we follow their approach and estimate the *optimal subpopulation average treatment effects* (OSATE) where we restrict the analysis to a subset of the original sample and drop individuals with covariate values that are outside the optimal common support range.[30] We do not find any significant differences to our main results.

2.6.5 Interim Conclusion

Before we take a closer look at effect heterogeneity, we conclude from the main analysis that both start-up programs are effective with respect to employment probabilities and improves the income situation. Male participants in SUS (BA) spend significant amounts of time longer in employment or self-employment than non-participants. Our results also unambiguously show that participants earn significantly more than non-participants. Additionally, self-employed participants are also more satisfied with their self-employment compared to previous dependent employment. Since it has often been argued that individuals who participate in start-up programs and become self-employed have characteristics (observed and/or unobserved) which make them different from other unemployed individuals we carefully assess the sensitivity of our results with respect to deviations from the identifying

[30]Restricting the estimation sample in such a way lowers external validity of the estimate, but probably enhances internal validity (Imbens and Wooldridge, 2009).

assumption using a holistic approach. Overall, we are confident that the results are robust and not driven by any remaining unobserved heterogeneity.

2.7 Effect Heterogeneity

Starting from the very promising evidence on the long-run effects of start-up programs for the unemployed, in the following we take a closer look on effect heterogeneity and investigate for which subgroups of the labor market (with respect to individual characteristics) those programs are most beneficial and if regional economic conditions have an influence on program effectiveness. Knowing how start-up schemes work for those groups and within different labor markets will help to design and assign programs more appropriate and thereby fight unemployment.

2.7.1 Who Benefits the Most?

First of all, we consider effect heterogeneity with respect to individual characteristics of male participants and non-participants in West Germany. This is in particularly insightful when determining the type of individuals who benefit most from participation. Disadvantaged groups in the labor market, such as low educated or young individuals, are likely to face limited job offers and the opportunity of becoming self-employed depicts a chance to escape unemployment. Additionally, self-employment might also be an alternative for individuals who are potentially discriminated in dependent employment, for example if their work is not valued high enough (see Clark and Drinkwater, 2000, for some evidence regarding ethnic minorties in the UK). We also have to take into account, that more educated unemployed individuals with past working experience have a relatively high probability of finding dependent employment again. Therefore, the distance between participants and matched non-participants in terms of labor market perspectives should be rather small. Taken together, this leads us to expect that the net effects of start-up programs (when compared to non-participation) are highest for disadvantaged individuals.

To answer the question of who benefits most, we conduct the complete estimation procedure, that is propensity score estimation and *kernel*-matching, for different subgroups of our sample with respect to educational attainment, professional qualification, age and nationality. The results are summarized in Table 2.26 in the Appendix, in which the upper part depicts the effects for the whole sample.

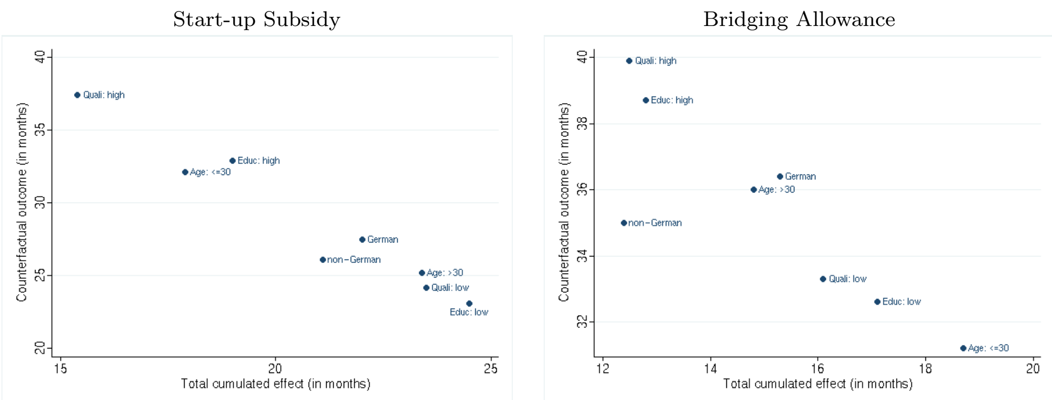
First of all, consider the results stratified by educational attainment. We split the sample into high (completed upper secondary school) and low (no degree, lower or middle secondary school) educated individuals. It can be seen that low educated participants perform better in both programs in terms of employment prospects; the total cumulated effect is about 5 months larger than for high educated individuals. This is mainly driven by the fact that the control group of the highly educated have a higher probability of being employed at all times than the respective low educated comparison group. We illustrate that in Figure 2.10 by showing the levels for the outcome variable "self-employed or regular employed" among participants and non-participants within the matched sample; the difference between the respective solid and dashed line corresponds to the ATT presented in Table 2.26. This confirms our expectation that the low educated control group performs relatively worse and consequently the effects are bigger for that group. Hence, offering individuals with bad labor market prospects the opportunity to turn unemployment into self-employment can be considered an effective strategy. The income effects in Table 2.26 do not reveal such obvious patterns. In the case of "SUS vs. NP" the low educated participants yield much higher income effects compared to non-participation than the highly educated do. For the comparison "BA vs. NP" it is the reverse, that is the highly educated are better off than their low educated counterparts. This suggests that highly educated BA recipients who survived in self-employment are also very successful in terms of income. Furthermore, we conduct a separate analysis for different levels of professional qualification. Here we define all individuals with tertiary or technical college education as highly qualified; whilst skilled or unskilled workers are low qualified. As we can see in Table 2.26 the effect pattern is very similar to the one of educational attainment (because professional qualification and educational attainment are highly correlated).

We also conduct the analysis separately for individuals aged 30 or younger as well as for individuals above the age of 30. Here, the employment effects of the two programs go in opposing directions. The results suggest that SUS tends to be more effective for participants above the age of 30; whereas BA seems to be more effective for younger participants. Figure 2.10 reveals that this is again mainly due to different labor market performance of the respective control groups. For both programs, there is hardly any difference between the program participants, that is the solid lines almost overlap. However, in the case of SUS controls, a considerable higher share of young controls is employed or self-employed and the reverse applies for BA. Probably more experienced (>30 years) BA controls are more likely to be employed or self-employed which seems reasonable given that BA attracts rather highly educated individuals with higher earnings in the past (see Section 2.4). Apparently, for these individuals experience is important in order to find a job in the labor market and therefore older BA control individuals perform better in the labor market. On the other hand, low educated individuals with bad labor market performance in the past (mainly attracted by SUS) have fewer opportunities in the labor market the older they are. The income effects are consistently higher for younger individuals. What has to be kept in mind here is that the matching quality for the younger cohorts is less satisfying and the same is true for SUS participants with high qualification (see Table 2.22 to 2.25 in the Appendix) for detailed matching quality indicators for the different subgroups). These groups are quite small making it harder to find suitable comparison individuals. Hence, the results have to be interpreted with caution.

Finally, we stratify the analysis with respect to German or non-German citizenship and find higher employment effects for natives. Figure 2.10 shows that the higher effects for natives are driven by the success of the participants. It can be seen that control groups do not really differ for both groups. This in turn suggests that SUS and BA seem to be even more effective for German participants. Additionally, natives achieve higher income effects even though they are not significant for the SUS case.

Figure 2.3: Effect Heterogeneity Conditional on Labor Market Perspectives Among Matched Non-Participants

Outcome variable: “Self-employed or regular employed”



Note: Depicted on the horizontal axis are the cumulated average treatment effects on the treated consistent to Table 2.26 for the outcome variable “self-employment or regular employment”. On the vertical axis we provide the average months spent in “self-employment or regular employment” within the observation period of 56 months for the matched non-participants.

Figure 2.3 exemplifies our findings with respect to effect heterogeneity and depicts the effects of program participation conditional on labor market perspectives without program participation. Therefore, we contrast cumulated average treatment effects for the outcome variable "self-employed or regular employed" (horizontal axis) to the average months spent in "self-employment or regular employment" among matched non-participants (vertical axis), which is supposed to reflect the labor market perspectives in case of non-participation. The scatter plot clearly indicates a negative relationship, underscoring the finding that groups with bad labor market perspectives benefit most. For instance, for individuals with high education/high qualification the estimated effects (horizontal axis) of the programs are rather small, however, for the opposite case—low education/low qualification—the effects are large. This suggests that SUS and BA are most effective for particular disadvantaged groups who face limited options in dependent employment. As previously mentioned, such groups are at high risk of becoming long-term unemployed; and therefore, these ALMP programs potentially contribute to the reduction of long-term unemployment amongst disadvantaged unemployed.

To sum up, the results suggest that both programs are especially effective for individuals who are at high risk of being excluded from the labor market and becoming long-term unemployed like low educated and low qualified individuals. Following the concept of Sen (1997), SUS and BA helped abolish labor market barriers for disadvantaged groups and sustainably integrated those into the labor market. Potentially, both programs are generally appropriate for fighting long-term unemployment, social exclusion and therefore poverty.

2.7.2 Does Effectiveness Vary with Regional Economic Conditions?

After having considered effect heterogeneity with respect to individual characteristics and shown that in particular disadvantaged groups of the labor market benefit most from start-up subsidies, we now investigate program effectiveness conditional on regional economic conditions. While it is well known that firm foundation is highly important for regional development as it has a positive impact on the structural change, innovation, job creation and hence economic growth (see Storey, 1994; Audretsch and Keilbach, 2004; Fritsch, 2008), what is unknown so far is how prevailing economic conditions influence the effectiveness of start-up subsidies for the unemployed. Existing evidence on the effectiveness of traditional ALMP programs (e.g. training, wage subsidies) with respect to economic conditions suggests that programs are generally more effective in regions with unfavorable economic conditions (see Lechner and Wunsch, 2009; Fahr and Sunde, 2009; Kluve, 2010).[31] The question remains however, if this evidence is adoptable to start-up programs as those programs do not only focus on the integration into dependent employment but also into self-employment and the survival in self-employment itself depends on prevailing economic conditions. To shed light on this issue is the contribution of this section.

Theoretical Considerations

Beside other factors such as population density, presence of small firms etc., in particular economic conditions such as aggregate demand or unemployment have been found to determine business formation (see Reynolds et al., 1994; Hamilton, 1989; Georgellis and Wall, 2000; Kangasharju, 2000, amongst others). The labor market approach provides an explanation as it states that individuals face an occupational choice and become self-employed if the expected discounted utility of being self-employed exceeds those of being in paid work (see Knight, 1921; Blanchflower and Oswald, 1998; Parker, 2009). In such a model economic conditions might push or pull individuals into self-employment as those characteristics are likely to affect the profitability of self-employment or/and the utility of paid work (Hamilton, 1986; Georgellis and Wall, 2000; Wagner and Sternberg, 2004). For instance, rising unemployment increases the risk of paid work and decreases wages which pushes individuals into self-employment as the expected utility of paid work decreases.[32] Reinforcing at the same time, the profitability of self-employment might increase due to higher availability of low-cost business takeovers (higher closure rates) or stronger business promotion by the public sector in such regions. On the other side, the pull hypothesis predicts a negative correlation between start-up and unemployment rates. Low unemployment rates indicate high aggregate demand which increases potential income from self-employment and leads to increased firm foundation. Start-up rates

[31] This is not necessarily true for subgroups of the workforce. For instance, McVicar and Podivinsky (2010) consider unemployed youths and investigate the effect of the New Deal for Young People in Britain. They find an inverse u-shaped relationship between program effectiveness and unemployment rates.

[32] In this context, Tervo (2006) shows that in particular individuals with an entrepreneurial family background are likely to be pushed into self-employment as these individuals possess latent entrepreneurial human capital.

might be further reinforced by eased capital availability and lower risk of failure in periods of favorable economic condition (Parker, 2009). However, Hamilton (1989) and Georgellis and Wall (2000) find that both the push and the pull theory apply and provide evidence that the relationship between unemployment and business formation is inverse u-shaped. This suggests that rising unemployment pushes individuals into self-employment only in areas with initially low unemployment rates but reduces start-up rates in regions with already high unemployment rates. The authors explain this observation by missing pull factors in very depressing areas.

While there is a large literature on economic variation and business foundation, much less research exits on the impact of environmental conditions on post entry firm performance. In general, it is assumed that more favorable economic conditions increase business survival due to higher product demand and lower interest rates (Parker, 2009). Although the estimated effects vary, the empirical evidence confirms this hypothesis and shows that beside firm and industry characteristics in particular macro-economic conditions (employment growth, GDP, unemployment rate) play an important role in determining post entry firm performance (see Audretsch and Mahmood, 1995; Fritsch et al., 2006; Brixy and Grotz, 2006; Falck, 2007, amongst others). Overall it seems that more favorable conditions extend firm survival, however, with particular regard to unemployment rates the effects are ambiguous. Keeble and Walker (1994) and Audretsch and Mahmood (1995) find a negative relationship between unemployment rates and business survival, while van Praag (2003) find a positive but not significant relationship. Fritsch et al. (2006) argue that unemployment rates reflect different macro-economic dimensions (economic growth, availability of workers, start-up rates out of unemployment) and depending on the individual impact of each factor the overall effect of unemployment rates on business survival in regression analysis might be positive or negative.[33] In addition, with particular regard to start-ups out of unemployment we have to take into consideration that individuals have on average higher tendency towards dependent employment. This might lead to higher exit rates out of self-employment among former start-ups out of unemployment during an economic upswing when the number of vacant job opportunities increases. This would then counteract the positive correlation between economic conditions and firm survival. However, relying on

[33] While the availability of workers to new firms predicts a clear positive impact on firm survival, the effect of economic growth and start-up rates out of unemployment is ambiguous. We refer to Fritsch et al. (2006) and Falck (2007) for a detailed discussion on how environmental factors might affect business survival.

empirical evidence it seems that more favorable economic conditions extend firm survival.

Given this evidence, on might conclude that the risk of business failure is generally higher in deprived areas which would predict higher program effectiveness in privileged areas. If this is true the question arises if subsidizing business foundation among unemployed individuals in deprived areas is a sensible strategy at all or do participants return to unemployment once the subsidy expires. Beside a scientific interest this would be of high relevance to policy makers. However, program effectiveness does not solely depend on the performance of program participants (survival in self-employment) but on their labor market performance relative to non-participants in the same area. Taking this into account brings up a reverse hypothesis, namely that start-up programs might be more effective in deprived areas as self-employment provides an alternative to dependent employment which is typically limited in such regions. Existing labor demand side restrictions in deprived areas might lead to low employment probabilities among non-participants and hence to higher program effectiveness in poor compared to privileged areas.[34] As theoretical considerations do not deliver a clear answer to which of the two opposing effects dominates, i.e., higher business survival versus higher employment probabilities among non-participants in regions with favorable economic conditions, this has to be answered empirically which is the contribution of this section.

Empirical Evidence

To estimate regional effects, we classify regional labor markets (identified by labor agency districts[35] in our sample) by the distribution of different economic indicators. From the theoretical considerations, previous empirical work and data availability, we decide to stratify regional labor markets by the level of unemployment rates, vacancy rates and GDP as those measures reflect the macro-economic conditions for paid employment (wages, labor market tightness) and self-employment (aggregate demand, productivity) which determines the decision to start a businesses, its post-

[34] This is in line with findings by Lechner and Wunsch (2009) who show that training programs in Germany lead to larger employment effects if unemployment is high (in terms of both periods and regions). The authors argue that non-participants are less likely to find a job during periods of high unemployment and if then probably worse jobs. In contrast, participants are locked into the program when unemployment is high and might face better search and economic conditions if the program elapses.

[35] In total, 141 labor agency districts exist in West Germany.

entry performance and reflects existing labor demand side restrictions. Therefore, we add those aggregate information on labor agency districts in the third quarter 2003 to our data.[36] The unemployment rates and the number of vacancies are obtained from the German Federal Labor Agency, and the gross domestic product from the German Federal Statistical Office. We adjust the vacancies by the stock of unemployed and calculate GDP per capita, i.e., adjusting GDP by population, to take district sizes into account. After having merged the aggregate information on unemployment rates, vacancy rates and GDP per capita on labor agency district level to the individual data, we define regional labor markets by dividing the distribution of each measure within our estimation sample into three equal parts.[37] For the case of unemployment rates for instance, this leads to three different types of regional labor markets, those with relatively low, medium and high unemployment rates.

Table 2.10 shows the distribution of the different aggregate measures within the full estimation sample and within each of the three stratified subsamples. It is visible that the distribution of all three measures is relatively symmetric within the full estimation sample which leads to stratified subsamples of approximately the same size in terms of number of assigned labor market districts. Moreover, we see that sufficient variation in terms of the measures exist to classify distinctive regional labor markets. For instance, areas with relatively low GDP per capita show a mean of 21,947 Euro per capita which is 14,134 Euro lower than in areas with high GDP per capita which is quite substantial.

To estimate causal effects of participation in SUS and BA on labor market outcomes, we repeat the complete estimation procedure as outlined in Section 2.6.2 including PS estimation and *kernel* matching conditional on the stratified subsamples. By doing this, we take variations in terms of the selection into treatment due to different economic conditions into account.[38] To assess the resulting matching

[36] Although business formation influences economic development on the aggregate level (see Storey, 1994; Audretsch and Keilbach, 2004; Fritsch, 2008), the prevailing regional economic conditions are assumed to be exogenous to new entries into self-employment.

[37] We additionally stratify the sample by dividing the respective distributions into four equal parts. Results are similar and lead to the same conclusion. However, lower numbers of observation in each cell result in poor matching quality why we decided to take three categories as the preferred strategy.

[38] For instance, comparing the coefficients of the PS estimations within the two subgroups stratified by low and high GDP per capita reveals that approx. 30% of the coefficients show different signs (for both programs). This indicates that regional economic conditions indeed affect the selection into treatment.

Table 2.10: Distribution of labor market indicators within the estimation sample

	Full sample	Stratified regional labor markets Low	Medium	High
Unemployment rate (in %)				
Number of labor agency districts	141	53	44	44
Mean	8.300	6.160	8.086	10.699
Standard deviation	2.078	0.832	0.562	1.291
Median	8.093	6.279	8.199	10.641
Minimum	4.083	4.083	7.305	8.975
Maximum	15.350	7.284	8.937	15.350
Vacancy rate[a] (in %)				
Number of labor agency districts	141	51	45	45
Mean	10.844	6.320	9.469	17.181
Standard deviation	5.737	1.049	1.072	6.059
Median	9.371	6.613	9.545	14.558
Minimum	3.813	3.813	7.924	11.677
Maximum	36.539	7.833	11.572	36.539
Gross Domestic Product[b] (in thousand Euro per capita)				
Number of labor agency districts	141	53	48	40
Mean	28.207	21.947	26.617	36.081
Standard deviation	7.213	1.792	1.253	6.897
Median	26.575	22.438	26.575	33.045
Minimum	18.090	18.090	24.427	28.663
Maximum	49.070	24.280	28.610	49.070

Note: Labor market indicators are measured in third quarter 2003 at the level of labor agency districts. In total, 141 labor agency districts exist in West Germany.
[a] Available vacancies as the share of the stock in unemployment.
[b] In prices of 2005.

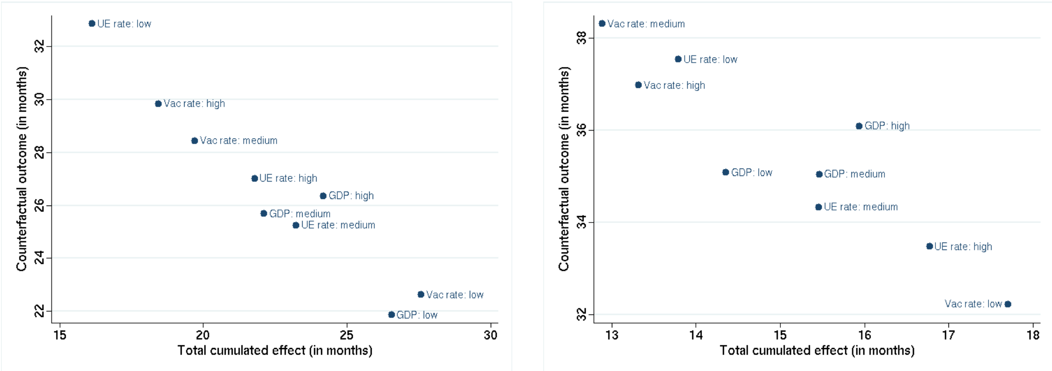
quality within each regional subgroup, Table 2.27 to 2.29 in the Appendix show respective measures (see Section 2.6.2 for a discussion of the applied indicators). While the t-test on equal means and the Pseudo R^2 indicate towards a successful matching for both programs, the mean standard bias for SUS is after matching within some cells still above the critical value of 5% as suggested by Caliendo and Kopeinig (2008). However, the remaining bias does not have a substantial influence on the selection into treatment anymore (very low Pseudo R^2). Therefore, we conclude that the PS matching procedure sufficiently created a control group within each subsample that is very similar to the respective treatment group at the point of entry into treatment.

Consistent to the previous section where we investigate the effect heterogeneity with respect to individual characteristics, Table 2.30 in the Appendix contains a summary of the estimated ATT for employment outcomes and different income measures within the different regions. With respect to employment outcomes, we

Figure 2.4: Regional Effect Heterogeneity Conditional on Labor Market Perspectives Among Matched Non-Participants

Outcome variable: "Self-employed or regular employed"

Start-up Subsidy | Bridging Allowance



Note: Depicted on the horizontal axis are the cumulated average treatment effects on the treated consistent to Table 2.30 for the outcome variable "self-employment or regular employment". On the vertical axis we provide the average months spent in "self-employment or regular employment" within the observation period of 56 months for the matched non-participants.

see that SUS and BA generate the lowest effects within labor markets characterized by low unemployment rates, high vacancy rates or high GDP per capita. For instance, the total cumulated employment effect within regions characterized by low unemployment rates is 16.1 (13.8) for SUS (BA) but amounts to 21.8 (16.8) in regions with high unemployment rates. In addition to the ATT in Table 2.30, we depict the respective employment probability levels among treated and matched control individuals in Figure 2.11 in the Appendix. We see that the positive results for disadvantaged regions is primarily attributable to the low performance among the non-participants. While the black lines (treated within different areas) almost overlap, the gray lines (matched controls within different regions) show partly substantial differences in the sense that non-participants in disadvantaged regions face lower employment probabilities than in privileged regions. It seems that SUS and BA with its integration into self-employment counteract the limited job opportunities in disadvantaged areas. Figure 2.4 illustrates the negative relationship between economic condition and program effectiveness graphically. Therefore we scatter the ATT for the total cumulated employment outcome (x-axis) against the estimated counterfactual outcome (y-axis). We clearly see for both programs that the lower the counterfactual outcome (probably due to limited job opportunities in the labor market) the higher the ATT. Finally, with respect to the income measures we do not find such a clear indication. Table 2.30 in the Appendix shows that SUS and

BA indeed increase incomes of participants in the long-run in most cases, however, we do not detect the pattern of higher effectiveness within disadvantaged regions as it is the case with respect to employment outcomes.

Finally we address the question if regional economic conditions affect business survival. Therefore, Figure 2.5 shows Kaplan-Meier estimates of survival probabilities in the first self-employment spell for program participants across the stratified subsamples. Consistent with theoretical predictions and previous findings, we see that more favorable economic conditions lead to slightly extended firm survival. However, to have an objective evaluation we additionally report the test statistic and its p-value of a Cox regression-based test on the equality of survival curves in Figure 2.5 (see Suciu et al., 2004, for an overview and discussion on such tests). This test bases on a test statistic that compares observed and expected exit probabilities in each regional subgroup. Thereby, the expected exit probabilities are calculated under the null hypothesis that the survival curves are the same across those groups. As we can see, the resulting p-values are always larger than the commonly used critical value of 0.05. Therefore, the evidence is not statistically sufficient to reject the null hypothesis of equal survival curves across the three stratified subsamples and we conclude that survival of subsidized businesses is not significantly affected by regional economic conditions.[39] This supports the hypothesis that employment effects are primarily driven by the labor market performance of non-participants under different economic conditions and less by differences in terms of firm survival.

To sum up, our results suggest that promoting self-employment among unemployed individuals is in particular effective in areas with unfavorable economic conditions. It seems that SUS and BA with its integration into self-employment counteract the limited job opportunities in disadvantaged areas as we find no significant differences in terms of business survival for privileged and disadvantaged areas. However, this does not imply that start-up programs are ineffective in privileged areas as employment effects are also strongly positive and significant for such regions.

[39] This is in line with findings by Tokila (2009) who runs a survival analysis on subsidized start-ups out of unemployment in Finland. She finds that regional characteristics have only a minor impact on the exit rate.

2.8 The Effects of Start-Up Subsidies for Unemployed Females

Finally, after having presented very promising long-term evidence on start-up programs and effect heterogeneity for men in West Germany, we now want to consider the case of unemployed women and investigate to what extent start-up programs may help unemployed women to escape unemployment. As outlined already in the literature review (Section 2.2) at the beginning of this chapter, existing evaluation studies show that participation in traditional ALMP programs leads to positive but small employment effects for women in general, however, the induced higher labor market attachment comes at the price of reduced fertility among female participants (Lechner and Wiehler, 2011; Bergemann and van den Berg, 2008). This is mainly due to higher preferences for flexible working hours among women and missing part-time opportunities, while traditional programs focus on the integration in dependent employment. The OECD highlights the problem of declining fertility rates within OECD countries and its societal consequences, e.g., securing generational replacement and aging population. To counteract this worrisome development, several OECD governments started already to implement policies in the last decades (see Sleebos, 2003, for a summary of implemeted programs and empirical evidence on their effectiveness.). Against this background, Lechner and Wiehler (2011) conclude that the traditional programs of ALMP turn ineffective for women if fertility is considered as important as employment.

Supporting self-employment among unemployed women in contrast, might be a promising solution. Unemployed women start their own business which gives them more independence and flexibility in allocating their time to work and family. Therefore, start-up programs are likely to ease the integration of unemployed women without reducing fertility at the same time. This section considers female entries in Start-up Subsidy and Bridging Allowance and provide long-term evidence of participation in start-up programs on employment and income prospects of initially unemployed women and shed light on the question if and to what extent subsidized self-employment (in contrast to traditional programs of ALMP) reduces fertility among female participants. Moreover, it presents descriptive evidence on the subsidized businesses started by unemployed women.

2.8.1 Female Unemployment and Potential Effects of ALMP

As women, in contrast to men, usually have to reconcile work and family obligations, women tend to have higher preferences for flexible working hours. However, part-time jobs are limited. In addition, women are likely to experience discrimination in the labor market. The low female labor market participation might induce statistical discrimination where employers tend to prefer men as the uncertainty about women's ability is higher (see Phelps, 1972).[40] Following the theory of subjective discrimination by Becker (1971), women might be further hindered by taste-based decisions of employers. Prejudices against women might stem from expected working interruptions due to fertility or from sexist views of men about the appropriate role of women, i.e., housework and child care against labor market activity (see Charles et al., 2009, for a discussion and empirical evidence).[41] The higher preferences for flexible working hours and potential discrimination issues make the integration of unemployed women difficult which is reflected by the structure of the unemployed workforce. The unemployed female workforce is characterized by long-term unemployment, high shares of job-returnees and single parents. Unemployed women are on average also more likely to leave the workforce with increasing unemployment duration even though they are better educated than unemployed men.[42] Given that the questions arises, whether and to what extent national ALMP take these gender differences into account. A recent comparative study by the European Commission shows that the majority of the thirty European countries made efforts to adjust their employment policies with respect to gender specific needs (see European Commission, 2008). For instance, Greek authorities provide higher subsidies to employers hiring lone parents and returnees or Spain offers social security reductions for contracting women. In Germany there are no—at least to our knowledge—gender-specific programs, such that each measure provided by the Federal Employment Agency based on the Social Act III is accessible by both un-

[40]Evidence on the existence of statistical discrimination is provided by Dickinson and Oaxaca (2009) and Altonji and Pierret (2001) amongst others.

[41]Although taste-based discrimination is extremely hard to prove, studies by Goldin and Rouse (2000) and Neumark et al. (1996) provide evidence on the existence of discrimination against women within the hiring process which are also reflected in recent initiatives to overcome sexual discrimination with the introduction of anonymous job applications. (see Krause et al., 2011; Behaghel et al., 2012).

[42]The German Federal Labor Agency reports for 2008 that among unemployed women 51% have no or only a lower secondary school degree compared to 60% among unemployed men; moreover, 19% (1%) of unemployed women (men) are single parents and 37% (30%) went from unemployment to out of the labor force.

employed men and women. However, the Social Act III that regulates the labor market policy in Germany requires gender equality, which leads to increasing female entries into ALMP and attempts to eliminate female-specific labor market barriers (see Müller and Kurtz, 2003).[43] Rubery (2002) shows that the implementation of "Gender Mainstreaming" in the German labor market policy is relatively advanced in an European comparison and in particular the access to programs of ALMP has recently been simplified for job-returnees who often are not eligible to unemployment benefits and hence face restricted access to ALMP.

However, the question remains how ALMP —given the gender differences in the composition of the unemployed workforce— is supposed to work. With a focus on unemployed women who are characterized by long-term unemployment, high shares of job-returnees, single parents and high risk of leaving the workforce, in particular two outcomes are of interest that is labor market participation in general and the integration into employment. Within a theoretical model that relies on the assumption that individuals participate at the labor market if the value of participation exceeds the value of non-participation, Johansson (2001) argues that ALMP is likely to have a positive impact on labor market participation. The value of labor market participation is higher for program participants compared to non-participants as it directly or indirectly influences labor market income due to additional earnings during the program, renewal of benefit entitlement or higher job arrival rates afterwards. Johansson (2001) confirms the theory empirically and finds a positive effect on labor force participation for the case of Sweden.

With respect to ALMP and its impact on the employment probability of participants, the theory predicts that ALMP increases the employment probability of participants by increasing the efficiency of the matching process between employers and workers due to an increase in human capital, employability or the search intensity (Kluve et al., 2007). Beside this more general view, Bergemann and van den Berg (2008) particularly focus on women and provide theoretical considerations on how ALMP might increase the employment probability of female participants. First of all, women face on average higher wage elasticities than men. This is possibly due to the fact that women need to reconcile more responsibilities when allocating their time, i.e., beside work and leisure, also child care or housework. The higher female wage elasticity induces higher reservation wages than offered by the market

[43] Since January 1, 2003 the "Job-Aqtiv-Gesetz" became law and integrated the concept of "Gender Mainstreaming" as a cross-sectional target into the German labor market policy.

which in turn decreases female labor supply. Human capital enhancing programs of ALMP might increase wage offers and (if those exceed individual reservation wages) make women accepting jobs. The fact that the unemployed female workforce is characterized by a relatively high educational level in contrast to unemployed men weakens the validity of this argument. In line with this, Müller and Kurtz (2003) show for Germany that women are over-represented in schemes such as vocational training or job creation schemes which are associated with a relatively low probability of re-integration. The main hurdle for unemployed women in Germany is hence obviously not a lag in human capital. The second aspect identified by Bergemann and van den Berg (2008) that might determine the effectiveness of ALMP to re-integrate unemployed women into employment is that it decreases labor market distance. Labor market biographies of women are likely to be interrupted by maternity leave, child care or other family related reasons. Employers have therefore less information about women's productivity compared to men which might lead them to have preferences for male workers (statistical discrimination). Programs which are directly associated with an integration in employment such as wage subsidies are most promising as they give potential employers the opportunity to learn about women's employability (which also reduces potentially existing prejudices). In addition, women start working and learn about their own opportunities in the labor market and about non-pecuniary utility of employment. Although wage subsidies are likely to reduce the labor market gap for women essentially, program assignment is (in contrast to further training or job creation schemes) not solely at caseworker's but also on employer's discretion. The assignment restriction leads therefore to an under-representation of women in those programs (see Müller and Kurtz, 2003). Start-up subsidies, in contrast, are more promising as they are associated with the positive feature of wage subsidies (reduce distance to the labor market) but do not hinge on employer's decision. Unemployed women start their own business and therefore create their own job.

2.8.2 Descriptive Evidence on Female Start-Ups out of Unemployment

To assess the effectiveness of SUS and BA for unemployed women, we use all observations on female participants and non-participants in our data (compare Table 2.4 in Section 2.4). We observe 448 (186) former female participants in SUS, 231

(136) in BA and 591 (271) female non-participants in West Germany (East Germany). Based on these observations, we first of all consider descriptive statistics and address the following three questions: Who are the female business founders out of unemployment? What kind of businesses do they found and how do they perform over time? And finally, do the programs —as part of ALMP— successfully integrate female participants into the labor market and what are the effects on fertility? Thereby, we highlight significant differences to both their male counterparts and female non-participants where appropriate. Furthermore, results are separately presented by region as East and West Germany are characterized by significant different labor market conditions. West Germany is characterized by more favorable labor market conditions compared to East Germany, i.e., lower unemployment rates, relatively more vacancies etc. Although those regional differences smoothes over time, at start-up in 2003 they were prevalent however. Note that all descriptive results are weighted using *sequential inverse probability weighting* to adjust for the selection process due to panel attrition as described in Section 2.4.

Who Are the Female Business Founders?

Table 2.11 shows descriptive statistics with respect to individual characteristics of female participants. It can be seen that both programs attract different types of individuals (as detected by Caliendo and Kritikos, 2010, already). Induced by the institutional setting both programs attract different types of individuals. As the amount of the subsidy depends on the level of unemployment benefits in the case of BA, this program attracts in particular better educated individuals as those are more likely to have higher past earnings and therefore higher benefit entitlement. Furthermore, the less restrictive eligibility criteria in the case of SUS (not only restricted to unemployment benefit recipients) provides individuals without (or elapsed) entitlement, e.g., individuals with few labor market experience or long-term unemployed, access to start-up subsidies. Therefore, simplified eligibility in case of SUS provides in particular women alternative access to the labor market as those are most likely to have less labor market experience due to family obligations and therefore only low or even no unemployment benefit entitlement. The induced higher take-up rate of SUS in this respect is confirmed by Table 2.11 which shows that 56% of female SUS participants in West Germany are married and 49% have children compared to 37% and 25% in the case of BA. For East Germany however, these shares are overall large (64-70% are married, 46% have children) and do not considerably differ between BA and SUS female participants. This might be explained by higher female labor market participation[44] in East Germany which increases the share of unemployed women with unemployment benefit entitlement and therefore eligibility to BA. In other words, less restrictive eligibility criteria for SUS in terms of unemployment benefit entitlement seems to be more important for unemployed women in West Germany.

Table 2.11: Individual Characteristics of Female Participants at Business Start-up

	Start-up Subsidy		Bridging Allowance	
	West	East	West	East
Age (in years)	39.1	40.9	38.2	40.4
Married	55.7	69.5	37.1	64.4
At least one child	49.2	46.4	24.7	46.8
Non-German	29.6	38.0	26.0	31.7
Daily unemployment benefit level (in Euro)	17.5	16.1	29.0	26.0
School leaving certificate				
No or lower secondary degree	31.0	13.9	17.3	6.1
Middle secondary degree	33.5	55.9	24.7	47.7
Specialized and upper secondary school	36.4	30.2	58.0	49.2
Intergenerational transmission				
Parents are/were self-employed	27.9	27.2	25.5	22.5
General willingness to take risk$^{a)}$ (Scale: 0=complete unwillingness; 10=complete willingness)				
Mean	5.4	5.5	5.7	5.8

Note: All numbers are percentages unless otherwise indicated. A comparison to female non-participants and male participants can be found in Table 2.32 in the Appendix.
$^{a)}$ Measured at the second interview, i.e., 28 months after start-up.

Furthermore, we want to shed light on the question if primarily women with strong family obligation choose start-up programs and to what extent female business founders differ to their male counterparts. Therefore, Table 2.32 in the Appendix shows a comparison of female participants to both female non-participants and male business founders. Thereby the first two columns present results for female SUS participants in East and West Germany (as shown in Table 2.11), while column three and four show the respective differences to female non-participants where positive numbers denote higher values for female participants. Finally, column five and six contain respective differences to male business founders.

We make two interesting observations. First, consider the differences to female non-participants. Beside the program-specific pattern, i.e., out of all non-participants BA attracts better educated individuals with higher benefit entitlement,

[44] The Federal Labor Agency reports for 2003 a female labor market participation of 63.6% in West and 71.4% in East Germany.

we see that female business founders are more risk loving compared to female non-participants which is also significant for the case of BA. This supports the hypothesis that self-employment particularly attracts women with higher risk preferences. Second, compared to men we see that SUS female participants in both East and West Germany are significantly more likely to be married and have children while the evidence is mixed for BA. In this regard, we do not find significant differences to female non-participants (except for BA in West Germany). Moreover we find find that female business founders are on average better educated than their male counterparts as indicated by positive and significant differences for the category specialized and upper secondary school (except for SUS participants in East Germany where the difference is not significant).

What Types of Businesses Do They Start?

Table 2.12 shows a comparison between female and male founders with respect to different aspects of the founding process and business evolvement. First of all, consider the characteristics of the founding process. As expected from the composition of BA female participants, i.e., better educated, higher earnings in the past and lower family ties, we see that female BA participants (compared to SUS) report more often to be motivated by being their own boss, found more capitalized businesses and consider the subsidy to be less important for the founding decision. This reinforces the hypothesis that BA female participants are similar to a general business founder and SUS participants are rather "atypical" (compare Caliendo and Kritikos, 2010). However, female participants in both programs report "termination of unemployment" as their main motive. Moreover, the comparison to male participants shows that female participants seem to have different motivations to start their own business (men report more often "being the own boss") and tend to invest less. For instance, women are approx. 10%-points more likely to cap their initial investment to a maximum of €1000. Furthermore, the decision to become self-employed hinges much more on the existence of the subsidy for women (although the difference to men is not significant). This descriptive evidence might indicate that self-employment was probably not the first choice of unemployed women but rather served as an alternative exit out of unemployment.

Given this indication that becoming self-employed was probably not the preferred strategy of female participants together with findings by Ehlers and Main (1998) who show that supporting low-income, minority women in the US fosters

Table 2.12: Comparative Statistics of Subsidized Businesses by Gender

	Start-up Subsidy				Bridging Allowance			
	Female participants		Difference[a] to male participants		Female participants		Difference[a] to male participants	
	West	East	West	East	West	East	West	East
Business related characteristics at start-up								
Motivation to become self-employed								
I always wanted to be my own boss	45.6	43.2	−9.0**	−7.4	50.1	51.6	−5.8	−3.6
Termination of unemployment	84.7	82.3	+1.3	−3.6	75.0	80.3	−1.7	+4.4
Advice from the labor agency	22.4	13.6	+6.4**	+2.8	16.0	8.6	+2.6	+1.8
Capital invested at start-up								
$<$ 1000 Euro	69.3	60.9	+13.6***	+11.6**	43.7	46.8	+9.1**	+15.6***
$\geq$ 10000 Euro	10.3	6.2	−5.3**	−5.1*	24.8	26.3	−12.9***	−4.8
Relevance of the subsidy for start-up								
Subsidy was crucial	35.6	41.7	+5.2	+7.2	23.7	27.8	−0.1	+2.9
Business development: Measured 56 months after start-up								
Share in self-employment	59.6	57.1	−0.1	−6.3	66.6	58.1	−1.3	−12.8**
Continuously self-employed for 56 months	91.1	91.6	+3.0	−4.2	86.3	86.5	+1.4	+4.8
Working time (hours/week)	33.1	37.5	−11.8***	−11.3***	38.9	44.7	−12.7***	−9.5**
Self-employed individuals only								
Personal income from self-employment (net, in Euro)								
Monthly	1061.4	811.6	−687.5***	−772.8***	1464.7	1267.5	−1012.3***	−411.8***
Hourly	8.7	7.0	−1.0	−1.8	10.0	7.3	−2.6**	−0.3
Partner with working income in household (in %)	60.8	69.5	+13.8**	+8.0	61.5	65.9	+2.4	+4.3
Partner's working income (net, in Euro/month)	2433.5	1686.9	+953.9***	+68.7	2484.3	2107.0	+810.3**	+777.0***
Equivalent income[b] (net, in Euro/month)	1820.9	1452.1	−8.1	−648.0**	2155.7	1920.0	−71.0	−68.1
Number of household member	2.7	2.7	+0.2	−0.0	2.3	2.9	−0.7***	+0.3
Job satisfaction compared to previous dependent employment (Scale: Improved (1), Unchanged (0), Declined (-1))								
Type of activity	0.6	0.5	+0.0	−0.2**	0.6	0.5	+0.1	+0.0
Working time	0.0	−0.2	+0.2**	+0.0	−0.1	−0.2	+0.2***	+0.2
Employee structure								
Share with at least one employee	17.0	20.1	−4.0	−1.5	30.8	29.6	−11.1*	−5.6
Number of employees	2.3	1.7	−0.2	−0.7	5.1	2.8	+0.5	−0.9

Note: All numbers are percentages unless otherwise indicated.
[a] Positive numbers denote higher values for female participants. Differences are statistically significant at the * 10%, ** 5%, *** 1% level.
[b] The equivalent income is calculated by adjusting the household income by the number of household members. The household income is divided by the weighted number of household members. Following the actual OECD equivalence scale, the household head achieves a weight of one, all children below the age of 15 are weighted with 0.3 and everybody else with 0.5 (see Whiteford and Adema, 2007). Since we only observe the total number of household members, every household member beside the household head receives a weight of 0.4.

labor market segregation of those women, it is very important to consider business evolvement in the long-run. Therefore, the lower part of Table 2.12 provides information measured 56 months after start-up and focusses on the individual income situation and job satisfaction of still self-employed former female participants to assess the long-term living situation. Furthermore, it includes the employee structure established within the former subsidized businesses to shed light on the supposed double dividend (further job creation) associated with start-up subsidies. Before starting the discussion though, we emphasize that results presented in Table 2.12 are purely descriptive and differences between programs or gender do not allow for a causal interpretation as structural differences in terms of participants exist (compare Table 2.32).

First of all, five years after start-up the majority of SUS and BA female participants are still self-employed. In fact, we find about 58% of former SUS female participants as self-employed whereof 91% were continuously self-employed within the entire observation period of 56 months. In case of BA, we find 67% in West and 58% in East Germany as self-employed whereof 86% survived continuously. Compared to men, we do not see significant differences in terms of both self-employment and survival rates (except for BA in East Germany). This descriptive evidence indicates a high and persistent integration of former subsidy recipients in self-employment. In addition, we find supportive evidence that women use self-employment to reconcile work and family. First, they work significantly less hours than self-employed men and second, in particular female SUS participants who are characterized by higher shares of being married and having children (see Table 2.11) work also less hours than BA female participants.

In the following we consider the income situation of still self-employed individuals to figure out to which extent women's earnings from self-employment contribute to assure household's livelihood as this is an important indication if women use self-employment to maximize income or take advantage of the independence to combine work and family obligations. Table 2.12 shows that SUS female participants earn on average €1,061 (€812) per months from self-employment 56 months after start-up in West (East) Germany. The monthly income for self-employed BA female participants is higher and amounts to €1,465 (€1,268). First of all, it can be seen that higher monthly earnings among men are attributable to higher working hours and the gender gap disappears in terms of hourly earnings (except for BA participants in West Germany).

The Federal Statistical Office reports net hourly wages of €12 and €10 in West and East Germany for women in dependent employment in Germany in 2010.[45] A comparison to hourly earnings of self-employed women shows that former female participants earn less in self-employment. Furthermore, the majority of female participants lives together with a partner with further income from self- or dependent employment. Table 2.12 shows that partner's average working income is much higher than income from self-employment by female participants; in case of SUS even more than twice as big. This indicates that women's income from self-employment is on average lower than wages in dependent employment and it is most likely not essential to assure households livelihood. We take this as supportive evidence that women instead of maximizing income primarily choose self-employment to take advantage of the independence to combine work and family obligation. In line with this, female participants also report an improved satisfaction in terms of type of activity compared to previous dependent employment; it seems that they enjoy being self-employed.

In terms of further job creation (double dividend), Table 2.12 shows that women tend to operate primarily as solopreneurs as only 20% (30%) of female SUS (BA) participants have at least one employee 56 months after start-up. Conditional on having at least one employee, SUS female participants employ on average two employees while BA participants have three to five employees. Compared to men, women tend to have smaller businesses but the differences in terms of both share with employees and absolute number of employees are almost never significant. Therefore, the double dividend argument associated with start-up subsidies is also true for female subsidy recipients but the scope of job creation is limited.

What Are the Long-term Labor Market Outcomes?

ALMP aims to improve labor market prospects of unemployed individuals. Therefore, the question remains if the promotion of self-employment is a sensible strategy in this regard. Table 2.13 provides information on long-run labor market outcomes of female participants and non-participants measured 56 months after program start. Moreover, we provide information on fertility between both groups.

Beside high shares in self-employment among female participants 56 months after start-up (as depicted in Table 2.12), we find an even higher integration in em-

[45] The Federal Statistical Office only reports gross hourly wages of €18 and €15 in West and East Germany. We calculate net hourly wages by assuming a tax rate of 34%.

Table 2.13: Labor Market Outcomes of Female Participants and Non-Participants 56 months After Start-up

	Female participants		Difference[a)] to female non-participants	
	West	East	West	East
		Start-up Subsidy		
Labor market status				
Employed[b)]	76.3	76.1	+28.4***	+30.5***
Others[c)]	16.7	13.5	−24.4***	−13.7***
Income situation (net, in Euro/month)				
Working income	908.2	841.3	+66.5	+232.2***
Equivalent income[d)]	1629.5	1424.1	+135.1	+267.6***
Number of household member	2.7	2.8	+0.0	+0.2
Fertility				
Share in maternity or parental leave	1.9	5.4	−4.0**	−3.7**
		Bridging Allowance		
Labor market status				
Employed[b)]	90.1	81.9	+42.2***	+36.4***
Others[c)]	6.8	10.9	−34.3***	−16.3***
Income situation (net, in Euro/month)				
Working income	1393.8	1058.9	+552.1***	+449.8***
Equivalent income[d)]	1961.3	1655.5	+466.9***	+499.1***
Number of household member	2.2	2.8	−0.4***	+0.2
Fertility				
Share in maternity or parental leave	3.0	0.8	−2.9**	−1.0**

Note: All numbers are percentages unless otherwise indicated.
[a)] Positive numbers denote higher values for female participants. Differences are statistically significant at the * 10%, ** 5%, *** 1% level.
[b)] Being self-employed or regular employed.
[c)] Includes marginal employment, education and periods out of the labor force.
[d)] The equivalent income is calculated by adjusting the household income by the number of household members. The household income is divided by the weighted number of household members. Following the actual OECD equivalence scale, the household head achieves a weight of one, all children below the age of 15 are weighted with 0.3 and everybody else with 0.5 (see Whiteford and Adema, 2007). Since we only observe the total number of household members, every household member beside the household head receives a weight of 0.4.

ployment as a whole, i.e., being in self- or regular employment. Taking together self- and regular employment rates, the overall labor market integration amounts to 76% in case of SUS and 90% (82%) for BA in West (East) Germany. It seem that participation in SUS and BA —even in case of business failure— affect the probability of finding regular employment positively, e.g., due to labor market networks (contact to business partners) or an increase in employability and human capital. The unconditional comparison to non-participants shows that lower shares in employment but higher shares in the category “others” that captures marginal employment, education, out of the labor force and maternity or parental leave. This reflects the vulnerability of female labor market attachment, e.g., due to limited flexible working schemes in dependent employment. Table 2.13 further shows that female participants experience higher working and equivalent incomes than non-participants 56 months after start-up. With respect to fertility outcomes, we see that higher shares of non-participants are in maternity or parental leave indicating reduced fertility among female participants. However, in order to finally conclude if the promotion of self-employment is a sensible strategy to improve labor market outcomes without reducing fertility among female participants, it requires causal evidence, i.e., comparing participants and non-participants by controlling for structural differences between both groups, which is the objective of the next section.

2.8.3 Details on the Estimation of Causal Effects

As described in Section 2.5, we apply propensity score matching for which we have to estimate the propensity scores for participation in the respective program versus non-participation in a first step. To estimate the propensity scores of program participation versus non-participation for unemployed women we apply a non-linear *probit*-estimation. Results of the *probit*-estimations are depicted in Table 2.31 and the resulting distribution of the estimated propensity scores is depicted in Figure 2.12 in the Appendix. We see participant's propensity score distribution overlaps the region of the propensity scores of non-participants completely; therefore, the overlap assumption is fulfilled.

In a next step, we estimate the average treatment effects on the treated as depicted in Equation 2.2 by applying a *kernel* matching algorithm[46] and using bootstrapping to draw inference. Table 2.33 in the Appendix provides different statistics to asses the resulting matching quality, i.e., whether the matching procedure sufficiently balances the distribution of observable variables between participants and non-participants. We apply a simple comparison of means (*t-test*), the *mean standardized bias* (MSB) and the Pseudo-R^2 of the *probit*-estimation in the matched and unmatched sample respectively. A discussion on these measured can be found in Section 2.6.2. Overall, we conclude that the applied PS matching procedure yields a control group that is very similar to the treatment group with respect to their observable characteristics at point of entry into treatment.

[46] More specifically, we apply an *Epanechnikov Kernel* with an bandwidth of *0.06*. For sensitivity checks with respect to the choice of the estimation method see Table 2.34.

2.8.4 Results

To answer the two remaining research questions, i.e., long-term evidence of participation in start-up programs on employment and income prospects, and second, if and to what extent start-up programs reduce fertility among female participants, we define different outcome variables. To assess the employment prospects, we employ "self-employed or regular employed" as a binary outcome variable which is one for individuals who are either employed subject to social security contribution or self-employed and zero otherwise. We use this due to two reasons: First, non-participants are less likely to become self-employed than participants; and hence, comparing participants and non-participants with respect to self-employment only would bias the causal effects upwards. Second, the main objective of ALMP is to integrate individuals into the labor market which includes being regular employed as a success. Furthermore, to assess the impact on income prospects, we choose to consider individual working income and equivalent income which reflects the income situation of the household. As non-working women have zero working income and employment status differs between participants and non-participants, we additionally conduct a conditional analysis where we consider working income of full- or part-time employed ($\geq$ 15 hours/week) participants and non-participants only. Finally, to address the question if start-up programs increase labor market attachment by reducing fertility among female participants (as found for other programs of ALMP) we consider periods of out of the labor force and periods specifically linked to fertility by employing two binary outcome variables: "out of the workforce" such as being a houseman/-wife, long-term illness or rehabilitation and periods of "maternity or parental leave".

Employment and Income Prospects

Table 2.14 presents the estimated ATT, i.e, the difference in outcome variables between female participants and matched non-participants, with respect to employment and income prospects. With respect to the probability to be "self-employed or regular employed", the positive and significant results in Table 2.14 show that both programs successfully integrate former unemployed women in the labor market in the long-run. We emphasize though that the particular high effects in the beginning of the observation period (after 6 months) are likely to be due to program locking-in effects, i.e., participants received funding during the first six months in case of BA and up to three years in case of SUS which makes participants more likely to be self-employed. However, at the end of our observation window (56 months after start-up) when the last subsidy payment was at least two years ago, SUS female participants have nevertheless a 25.5% (37.8%) points higher employment probability compared to non-participants in West (East) Germany; 23.2% (33.1%) for the case of BA. Comparing these estimated employment effects to those for traditional ALMP programs underlines the success of SUS and BA and further supports the hypothesis that self-employment allows women to reconcile work and family. For instance, (Biewen et al., 2007) report employment effects of 5-10% (5%) for training programs 30 months after program start and Caliendo et al. (2008) find -1% (5%) for job creation schemes in West (East) Germany 36 months after program start.

Finally, we cumulate the monthly employment effects over the entire observation window which shows that female SUS participants in West (East) Germany spent on average 26.9 (29.8) months more in self-employment or regular employment compared to female non-participants. These effects are quite large taking into account that the observation window consists of 56 months in total. Again, due to a shorter period of funding (up to three years for SUS compared to six months for BA) and therefore smaller locking-in at the beginning of the observation window, cumulated effects for BA participants are slightly smaller and amount to 20.6 (25.9) months in West (East) Germany. Comparing the results for women to those for men in Section 2.6 (West Germany only), we find that the estimated employment effects of SUS and BA are larger for women than for men which is consistent with findings of other studies on traditional programs of ALMP (compare Section 2.2).

To answer the question if higher employment probabilities also translate into higher incomes for participants, Table 2.14 shows the ATT with respect to different income variables measured 56 months after start-up. We choose a holistical approach to investigate the program impact on participant's income and consider both individual working and equivalent household income. As mentioned above, due to higher employment probabilities for participants 56 months after start-up and therefore higher shares of non-participants with zero working income, we additionally provide the ATT with respect to working income for full- or part-time employed participants and non-participants only. Although this restricts the sample to women working 15 hours per week or more, we further correct for differences in working hours by calculating hourly earnings in addition to monthly income. Beside long-term evidence on employment prospects, this detailed income analysis is one

Table 2.14: Employment and Income Effects of Start-up Subsidy and Bridging Allowance for Female Participants

	Start-up Subsidy		Bridging Allowance	
	West	East	West	East
Number of observation				
Treated	413	173	225	128
Controls	525	250	518	250
Outcome variable: Self-employed or regular employed				
After 6 months	74.5***	74.0***	63.6***	67.5***
After 36 months	39.0***	48.3***	31.3***	41.6***
After 56 months	25.5***	37.8***	23.2***	33.1***
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	26.9***	29.8***	20.6***	25.9***
Outcome variable: Income measures				
Monthly working income	138	348***	225	334***
Monthly equivalent income$^{a)}$	193**	354**	385***	357***
Conditional analysis: Only full- or part-time employed individuals ($\geq$ 15 hours/week)				
Monthly working income	-106	74	21	78
Hourly working income	1.5	-9.5	1.4	-22.7

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between female participants and non-participants. Standard errors are based on *bootstrapping* with 200 replications. Significance levels are denoted by * 10%, ** 5%, *** 1%.
Employment outcome: Results are differences in %-points unless otherwise stated.
Income measures: Results are differences in € (net) measured 56 months after start-up and rely on a reduced sample size due to missing observation in income variables. To calculate hourly wages of individuals in dependent employment we consider actual (not contractual) working hours.

of our main contribution to the existing literature as evaluation studies on start-up programs mostly focusses on employment outcomes but due to data restriction often ignore the impact on income.

Regarding monthly working income the estimated effects for all participants are significantly positive in East Germany (€348 for SUS and €334 for BA) but insignificant in West Germany. Although female participants have higher employment probabilities 56 months after start-up, participation does not lead to a clear increase in working income. Conditional on being full- or part-time employed, any significant effect on monthly working income disappears. The effects on hourly earnings are positive for female participants in West and negative in East Germany but not significant in statistical terms. The rather disappointing evidence on working income might be due to two reasons: First, women opt for self-employment not to maximize working income but due to limited employment prospects in the regular labor market. This is reinforced by the impact on hourly earnings for female participants. Although the effects on hourly earnings are not statistically significant, they are at least in East Germany quite large and might be significant in economic terms. The results suggest that former SUS (BA) female participants earn on average €10 (€23) less per hour than working non-participants in East Germany. Therefore, the overall disappointing evidence on working income for female participants might be interpreted as a kind of compensation for being employed. Second, the large observation window of 56 months might still be too short and additional human capital accumulation among female participants (strong positive employment effects) takes more time to translate into an income gain.

The effects with respect to equivalent household income are positive and (in contrast to working income) throughout statistically significant for female participants. This indicates that within female participant's households additional income exists and hence income of female participants is not necessarily important to assure household's livelihood. This hypothesis is in line with descriptive evidence in Section 2.8.2 where we find that partner's average working income is much higher than income from self-employment by female participants.

Impact of Higher Labor Market Attachment on Fertility

Existing evaluation studies show that participation in programs such as training, job search assistance, job creation schemes or wage subsidies improve employment prospects for women, however, the induced higher labor market attachment reduces fertility among female participants. Lechner and Wiehler (2011) show that traditional programs of ALMP turn ineffective for women if fertility is considered as important as employment. As self-employment (in contrast to dependent employment) is likely to give women more independence to reconcile work and family obligations, the question remains whether the high and persistent employment effects in case of start-up programs do also reduce fertility among female participants. To shed light on this question, we follow female participants and non-participants over time and compare them by using two additional outcome variables: First, the binary variable "out of the workforce" delivers evidence on the program impact on the general labor market attachment and is one if the individual is not employed, not actively looking for a job and not in education, and zero otherwise. Second, to measure fertility we use the binary outcome variable "maternity or parental leave" which is one for respective spells and zero otherwise. Figure 2.6 and 2.7 depict the ATT with respect to these outcome variables and Figure 2.13 and 2.14 show additionally respective probability levels for SUS and BA participants and matched non-participants.

As expected from the large employment effects from above, we find that female participants have a lower probability to leave the workforce compared to non-participants in the short- to medium-term which is most likely driven by locking-in effects. While in East Germany the difference is with 3-4%-points quite small and disappears in the long-run, in West Germany the difference is larger (5-10%-points), persistent over time and most often statistically significant. This indicates that program participation increases labor market attachment of female participants only in West Germany beyond the locking-in period.

Taking this regional disparity into account, we now compare the results on labor market attachment to the results on fertility, which are depicted on the right side of Figure 2.6 and 2.7. The increased labor market attachment of female participants in West Germany does not or only slightly reduce fertility among participants. For BA female participants the higher labor market attachment does not lead to a significant reduction in fertility as indicated by the dashed lines (confidence interval) overlapping the null. In case of SUS, for female participants in West Germany the difference in terms of fertility is statistically significant different from zero within the first five months after start-up and not afterwards. In East Germany however, we find multiple negative and statistically significant effects with respect to fertility up to 18 months after start-up. Regarding the probability levels in Figure 2.13 and 2.14, it is clearly visible for East Germany that an increase in the probability to be in maternity or parental leave among female participants coincides with an increase in the probability to leave the workforce. It seems that women in East Germany do not use self-employment as flexible as in West Germany to reconcile work and family. Table 2.12 shows that self-employed women in East Germany face lower average hourly earnings. One explanation therefore is that women in East Germany need to work more hours per week in order to reach a comparative income level to women in West Germany. This higher intensive margin reduces fertility among female participants in East Germany.

Given these results, we conclude that in general participation in start-up programs increases labor market attachment of female participants with —in contrast to traditional programs of ALMP— less detrimental impacts on fertility. It seems that self-employment —in contrast to dependent employment— gives women more independence to reconcile work and family obligations. Specifically, we find that women in East Germany do not use self-employment as flexible as in West Germany which is likely due to lower hourly earnings which induce higher working hours.

Sensitivity Analysis

To check the robustness of our results with respect to deviations from the identifying assumption, we apply an identical sensitivity analysis as outlined and extensively discussed in Section 2.6.4. Therefore, we do not discuss it here in detail again but present respective results in Table 2.34 and 2.35 in the Appendix and conclude that results on women turn out to be as robust as those for men in Section 2.6.

2.8.5 Interim Conclusion

This section considers the case of unemployed women and investigate to what extent start-up programs may help unemployed women to escape unemployment. The descriptive analysis reveals that 57-67% of female participants are self-employed 56 months after start-up from which on average 90% were continuously self-employed. This indicates a high and persistent integration into self-employment. Among those who failed, a significant share is employed subject to social security contribution so that we observe a total labor market integration of 76-90%. Moreover, we find supportive evidence that female participants indeed use self-employment to reconcile work and family as they work significantly less hours than self-employed men and are characterized by higher shares of being married and having children (except BA female participants in West Germany). The results with respect to further job creation are rather disappointing as the majority still operates without employees. The causal analysis, i.e., comparison to non-participation, shows large and significant employment effects for female participants which are three to four times as large as estimated employment effects for traditional ALMP programs such as training or job creation schemes. This underlines the success of SUS and BA which is most likely due to better compatibility of work and family in self-employment. However, the large employment effects do not lead to a clear increase in working income 56 months after start-up. Therefore, it might be that women primarily opt for self-employment due to limited employment prospects in the regular labor market and not to maximize working income. Moreover, additional human capital accumulation due to more employment experience of female participants might take more time to translate also into a working income gain and the period of 56 months is too short. With respect to fertility, we find that start-up programs have in general less detrimental effects on fertility compared to traditional programs of ALMP. It seems therefore that self-employment in contrast to dependent employment gives women more independence and flexibility in allocating their time to work and family which in turn increases employment chances.

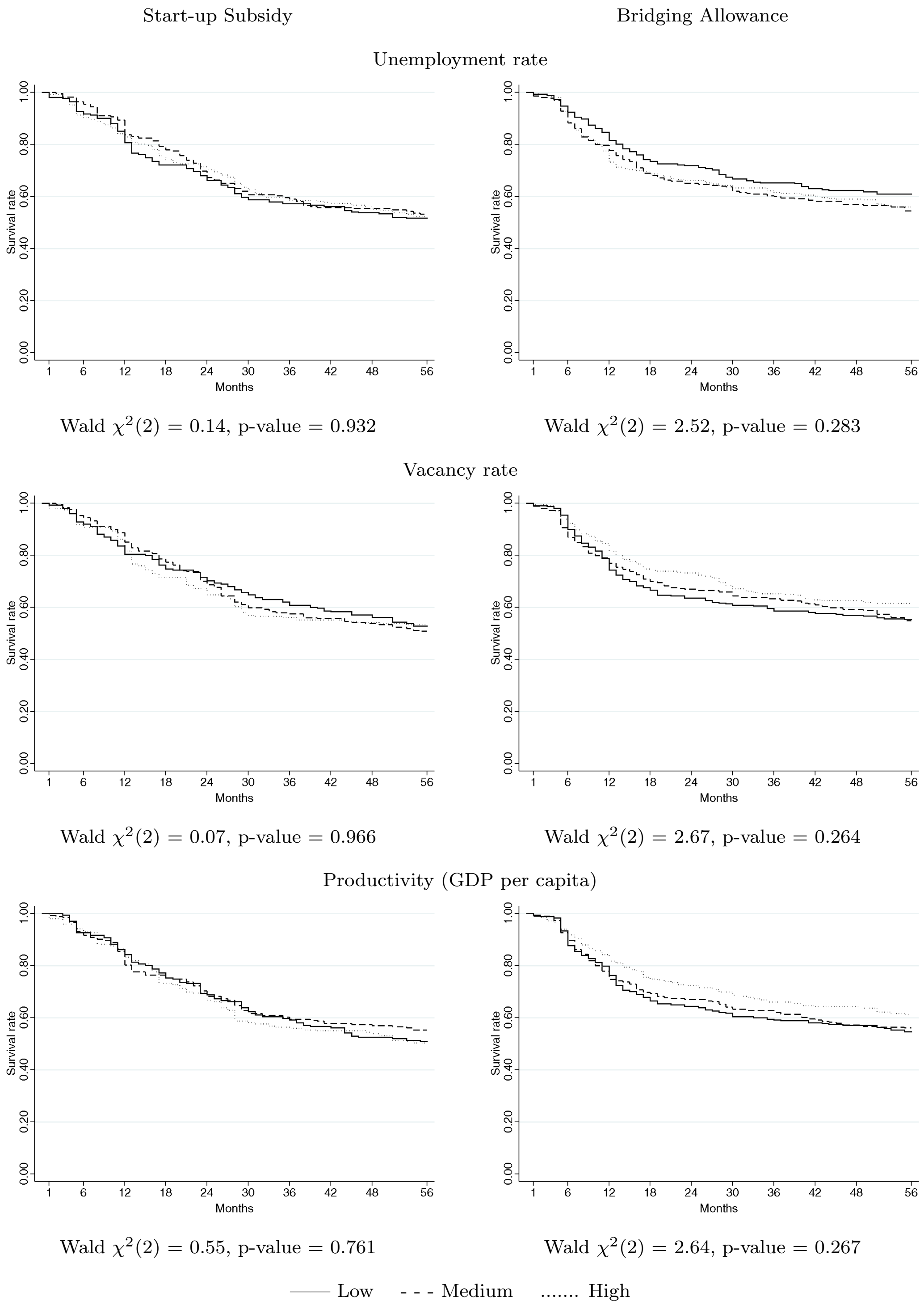
2.9 Conclusion

In this chapter, we analyze the effects of two distinct programs designed to turn unemployment into self-employment. The programs differ in their design and attract different types of persons. Individuals participating in the bridging allowance are more educated and have higher earnings in the past; whereas SUS participants are on average less educated and have a relatively poor previous labor market performance. Using an unique data set consisting of administrative and survey data, we are able to add three substantial aspects to previous literature: First of all, we observe individuals for nearly five years following start-up, such that we are able to provide first evidence on the long-term effects of these programs (especially for industrialized countries). Second, we carefully consider effect heterogeneity in order to determine for which groups and in which regions programs work best. Third, we provide empirical evidence on effectiveness for unemployed women.

We base our analysis on propensity score matching methods to assess the effectiveness of SUS and BA against non-participation. The identifying assumption is that conditional on the very informative data at hand selection into the programs can be assumed to be random such that outcome differences between participants and non-participants can be interpreted as causal effects. Since it has often been argued that individuals who participate in start-up programs and become self-employed have characteristics (observed and/or unobserved) which make them different from other unemployed individuals, we carefully assess the sensitivity of our results with respect to deviations from the identifying assumption. Overall, this makes us confident that the results are robust and not driven by any remaining unobserved heterogeneity.

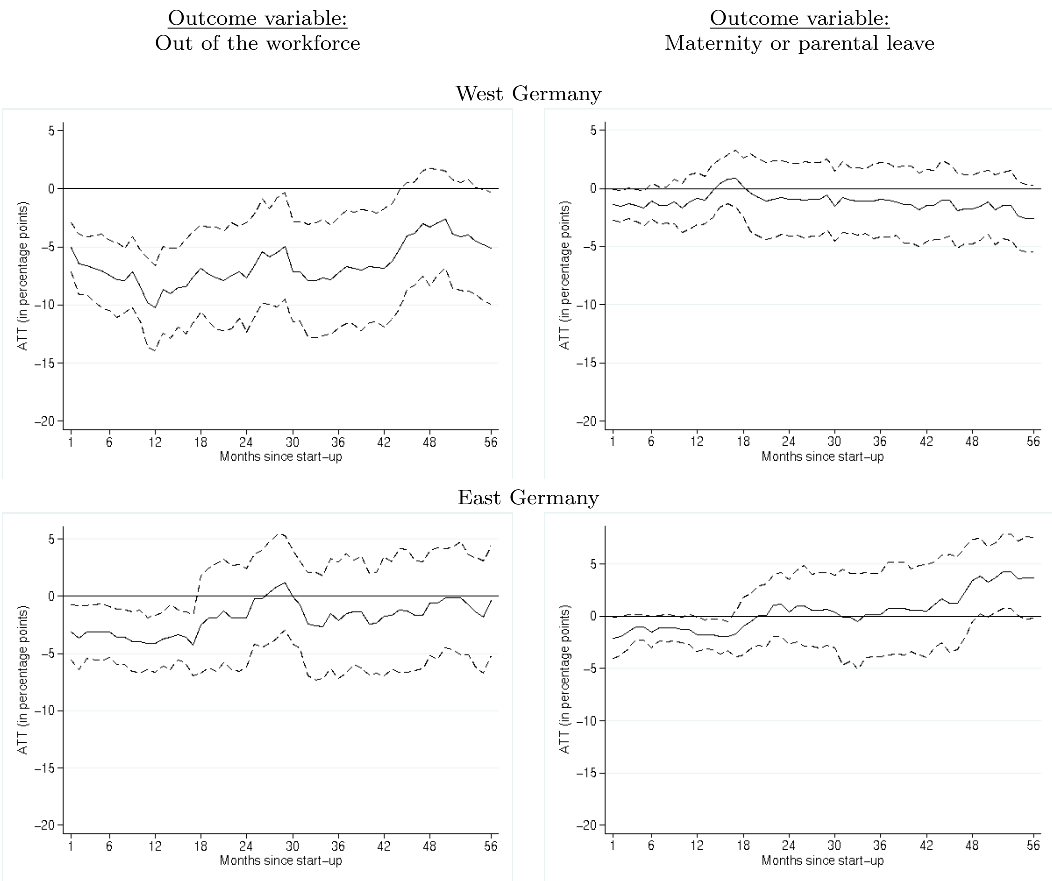
With respect to long-term effects of start-up programs, we find persistent positive long-run effects of SUS and BA on the employment situation of former unemployed individuals. In particular, we use the probability of being employed (either self-employed or as an employee) and personal income as outcome variables. The results show that both programs are effective with respect to employment probabilities. Participants in SUS (BA) spend significant amounts of time longer in employment or self-employment than non-participants. Our results also unambiguously show that male participants earn significantly more than non-participants. Additionally, self-employed participants are also more satisfied with their self-employment compared to previous dependent employment. Regarding effect heterogeneity, we estimate causal effects for different subgroups stratified by educational attainment, professional qualification, age and nationality, and for different regions stratified by local unemployment rates, vacancy rates and GDP per capita. The results suggest that both programs are especially effective for disadvantaged individuals such as low educated and low qualified individuals who are at high risk of being excluded from the labor market and becoming long-term unemployed. Moreover, programs seem to be more effective in regions with unfavorable economic conditions. Given the results on unemployed women we find that participation in start-up programs increases labor market attachment of female participants with —in contrast to traditional programs of ALMP— less detrimental impacts on fertility. It seems that self-employment —in contrast to dependent employment— gives women more independence to reconcile work and family obligations. Following the concept of Sen (1997), we conclude that SUS and BA helped abolishing labor market barriers for disadvantaged groups and sustainably integrated those into the labor market.

Figure 2.5: Survival in Self-employment Conditional on Regional Economic Condition



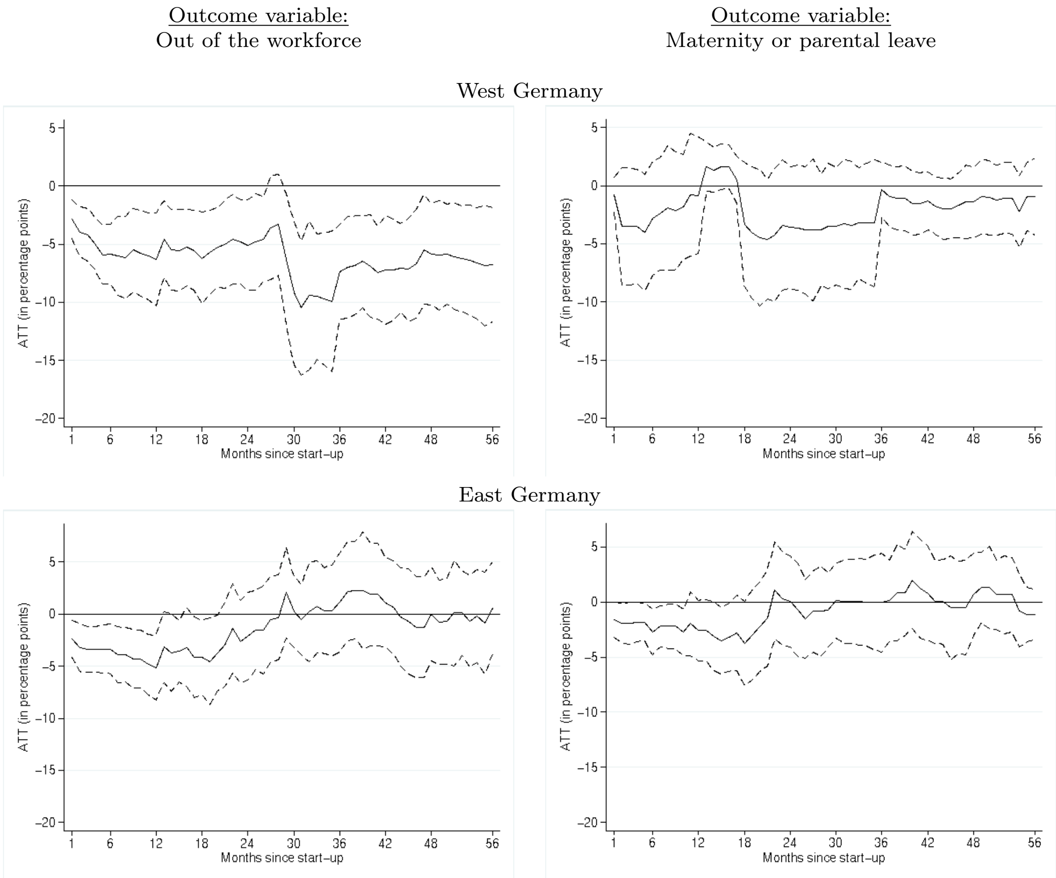
Note: Depicted are Kaplan-Meier estimates of the survival probability in the first self-employment spell for male program participants in West Germany conditional on the regional economic conditions at start-up. Below the graphs, we report the test statistic and p-value based on a Cox regression-based test on the equality of the depicted survival curves whereby the underlying null hypothesis states that the survival functions are the same.

Figure 2.6: Causal Effects on Labor Market Attachment of Female Participants Over Time - Start-up Subsidy



Note: Depicted are average treatment effects on the treated (solid line) as the difference in outcome variables between female participants and non-participants; the 5% confidence intervals (dashed lines) are based on bootstrapped standard errors with 200 replications. The binary outcome variable "out of the workforce" (housewife, illness, parental leave, retirement etc.) is one if the individual is not employed, not actively looking for a job and not in education, and zero otherwise. The binary outcome variable "maternity or parental leave" is one for respective spells and zero otherwise.

Figure 2.7: Causal Effects on Labor Market Attachment of Female Participants Over Time - Bridging Allowance



Note: Depicted are average treatment effects on the treated (solid line) as the difference in outcome variables between female participants and non-participants; the 5% confidence intervals (dashed lines) are based on bootstrapped standard errors with 200 replications. The binary outcome variable “out of the workforce” (housewife, illness, parental leave, retirement etc.) is one if the individual is not employed, not actively looking for a job and not in education, and zero otherwise. The binary outcome variable “maternity or parental leave” is one for respective spells and zero otherwise.

2.10 Appendix

2.10.1 Appendix to 2.6

Table 2.15: Selected Descriptive Statistics

	Start-up Subsidy	Bridging Allowance	Non-Participants
Number of observations[a)]	472	756	853
Age (in years)	38.86	40.17	39.75
	(9.78)	(8.66)	(8.88)
Age bracket			
18 to 24 years	0.068	0.026	0.049
25 to 29 years	0.131	0.095	0.095
30 to 34 years	0.174	0.126	0.130
35 to 39 years	0.153	0.242	0.212
40 to 44 years	0.176	0.200	0.210
45 to 49 years	0.127	0.160	0.165
50 to 64 years	0.172	0.151	0.138
Marital status (Ref.: Single)			
Married	0.472	0.648	0.594
Number of children in household			
No children	0.708	0.595	0.639
1 child	0.144	0.155	0.145
2 or more children	0.148	0.250	0.216
Health restriction that affect job placement (Ref.: No)			
Yes	0.078	0.033	0.057
Nationality (Ref.: German)			
Non-German	0.328	0.265	0.249
Desired working time (Ref.: Part-time)			
Full-time	0.977	0.992	0.984
School achievement			
None	0.028	0.007	0.014
Lower secondary school	0.405	0.290	0.370
Middle secondary school	0.250	0.233	0.223
Specialized upper secondary school	0.104	0.193	0.150
Upper secondary school	0.214	0.278	0.243
Occupational group			
Manufacturing	0.040	0.011	0.018
Agriculture	0.333	0.233	0.277
Technical occupations	0.038	0.160	0.108
Services	0.517	0.565	0.539
Others	0.072	0.032	0.059
Professional qualification			
Workers with tertiary education	0.123	0.259	0.200
Workers with technical college education	0.068	0.112	0.106
Skilled workers	0.559	0.515	0.549
Unskilled workers	0.250	0.114	0.145
Duration of previous unemployment			
$<$ 1 month	0.133	0.074	0.014
$\geq$ 1 month - $<$ 3 months	0.150	0.222	0.223
$\geq$ 3 months - $<$ 6 months	0.212	0.249	0.251
$\geq$ 6 months - $<$ 1 year	0.288	0.316	0.339
$\geq$ 1 year - $<$ 2 years	0.155	0.124	0.150
$\geq$ 2 years	0.061	0.015	0.023

Table to be continued.

Table 2.15 continued.

	Start-up Subsidy	Bridging Allowance	Non-Participation
Professional experience (Ref.: Without professional experience)			
With professional experience	0.824	0.860	0.877
Duration of last employment (in months)	32.394	54.041	41.963
	(40.987)	(54.358)	(49.076)
Number of placement offers	5.367	3.758	5.181
	(8.563)	(6.921)	(7.664)
Daily income from regular employment in the first half of 2003 (in Euros)	9.969	25.783	20.700
	(21.571)	(41.503)	(34.970)
Unemployment benefit level (in Euros)	24.363	40.405	33.167
	(11.436)	(15.275)	(14.322)
Remaining unemployment benefit entitlement (in months)	4.752	7.317	7.054
	(5.759)	(6.380)	(6.397)
Employment status before job-seeking			
Employment	0.591	0.782	0.769
Self-employed	0.053	0.024	0.036
School attendance/never employed before/apprenticeship	0.123	0.073	0.063
Unemployable	0.083	0.042	0.059
Other, but employed at least once before	0.131	0.070	0.066
Other	0.019	0.009	0.007
Regional cluster[b)]			
II a	0.013	0.024	0.028
II b	0.153	0.159	0.135
III a	0.127	0.071	0.088
III b	0.083	0.091	0.110
III c	0.222	0.237	0.244
IV	0.127	0.144	0.117
V a	0.036	0.042	0.038
V b	0.165	0.148	0.176
V c	0.074	0.083	0.064
Intergenerational transmission			
Parents are/were self-employed	0.284	0.284	0.155
General willingness to take risk[c)] (Scale: 0 = complete unwillingness; 10 = complete willingness)			
Mean	5.816	5.847	5.490
	(2.177)	(2.071)	(2.011)
Share with risk attitude ≥ 7	0.419	0.427	0.329

Note: Men in West Germany. Numbers are percentages unless otherwise stated. Measured in the third quarter 2003; standard deviation in parentheses.

[a)] Differences to realized interviews in Table 2.4 are due to missing information in the administrative data for some individuals.

[b)] The regional clusters categorize German labor office districts with comparable local labor market characteristics (see Blien et al., 2004). For instance, the category IIa contains urban districts with relatively high unemployment rates, IIIc primarily rural areas with below-average unemployment rates and few dynamic, while the category Vc captures districts characterized by favorable labor market conditions and high dynamic.

[c)] Measured at the second interview, i.e., 28 months after start-up.

Table 2.16: Propensity Score Estimation

	Start-up Subsidy vs. Non-Participation	Bridging Allowance vs. Non-Participation
Age bracket (Ref.: 18 to 24 years)		
25 to 29 years	0.430**	0.354*
30 to 34 years	0.508**	0.254
35 to 39 years	0.266	0.291
40 to 44 years	0.361*	0.119
45 to 49 years	0.433**	0.196
50 to 64 years	0.863***	0.316
Marital status (Ref.: Single)		
Married	−0.098	0.009
Number of children in household (Ref.: No children)		
One child	0.184	−0.105
Two or more children	0.089	−0.160
Health restriction that affects job placement (Ref.: No)		
Yes	−0.129	−0.090
Nationality (Ref.: German)		
Non-German	0.095	0.164**
Desired working time (Ref.: Part-time)		
Full-time	−0.037	0.135
School achievement (Ref.: None)		
Lower secondary school	−0.081	0.228
Middle secondary school	0.069	0.293
Specialized upper secondary school	−0.063	0.333
Upper secondary school	0.038	0.288
Occupational group (Ref.: Manufacturing)		
Agriculture	−0.250	0.100
Technical occupations	−0.705**	0.261
Services	−0.395	0.089
Others	−0.597**	−0.342
Professional qualification (Ref.: Workers with tertiary education)		
Workers with technical college education	0.126	−0.038
Skilled workers	0.071	0.042
Unskilled workers	0.198	0.066
Duration of previous unemployment (Ref.: $<$ 1 month)		
$\geq$ 1 month - $<$ 3 months	−1.634***	−0.893***
$\geq$ 3 months - $<$ 6 months	−1.488***	−0.943***
$\geq$ 6 months - $<$ 1 year	−1.639***	−1.069***
$\geq$ 1 year - $<$ 2 years	−1.765***	−1.118***
$\geq$ 2 years	−1.316***	−1.145***
Professional experience (Ref.: Without professional experience)		
With professional experience	−0.123	−0.251**
Duration of last employment (in months)	0.001	0.002**
Number of placement offers	−0.006	−0.010**
Remaining unemployment benefit entitlement (in months)	−0.028***	−0.024***
Unemployment benefit level (in Euros)	−0.029***	0.024***
Daily income from regular employment in the first half of 2003 (in Euros)	−0.002	−0.002*
Employment status before job-seeking (Ref.: Employment)		
Self-employed	0.290	−0.373*
School attendance/never employed before/apprenticeship	0.362**	0.225
Unemployable	0.197	−0.072
Other, but employed at least once before	0.458***	0.246*
Other	0.352	0.456

Table to be continued.

Table 2.16 continued.

	Start-up Subsidy vs. Non-Participation	Bridging Allowance vs. Non-Participation
Regional cluster (Ref.: II a)		
II b	0.730**	0.224
III a	0.744**	0.043
III b	0.545*	0.157
III c	0.609*	0.118
IV	0.911***	0.183
V a	0.636*	0.415
V b	0.707**	−0.041
V c	0.782**	0.262
Intergenerational transmission		
Parents are/were self-employed	0.476***	0.453***
Constant	1.240**	−0.607
Number of observations		
Participants	472	756
Non-Participants	853	853
Hit-Rate (%)	70.79	65.26
Pseudo R^2	0.196	0.105
Log-likelihood	−693.612	−995.964

Note: Men in West Germany. * 10%, ** 5%, *** 1% significance level.

Table 2.17: Matching Quality

	Start-up Subsidy Before matching	Start-up Subsidy After matching	Bridging Allowance Before matching	Bridging Allowance After matching
t-test of equal means[a)]				
1%-level	19	0	9	0
5%-level	28	0	15	0
10%-level	33	0	17	0
Standardized bias				
Mean standardized bias	14.550	3.539	8.565	2.194
Number of variables with standardized bias of a certain amount				
$< 1\%$	2	12	3	22
1% until $< 3\%$	4	14	11	18
3% until $< 5\%$	4	14	6	7
5% until $< 10\%$	15	15	21	9
$\geq 10\%$	31	1	15	0
Pseudo-R^2	0.196	0.013	0.105	0.007

Note: Men in West Germany.
[a)] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 56 observable variables in total.

Table 2.18: Propensity Score Estimation: Extended Specification

	Start-up Subsidy vs. Non-Participation	Bridging Allowance vs. Non-Participation
Age bracket (Ref.: 18 to 24 years)		
25 to 29 years	0.459**	0.407*
30 to 34 years	0.537***	0.3
35 to 39 years	0.291	0.344*
40 to 44 years	0.381*	0.166
45 to 49 years	0.46**	0.241
50 to 64 years	0.898***	0.381*
Marital status (Ref.: Single)		
Married	−.103	−.005
Number of children in household (Ref.: No children)		
One child	0.182	−.092
Two or more children	0.083	−.152
Health restriction that affects job placement (Ref.: No)		
Yes	−.106	−.071
Nationality (Ref.: German)		
Non-German	0.088	0.161**
Desired working time (Ref.: Part-time)		
Full-time	−.080	0.092
School achievement (Ref.: None)		
Lower secondary school	−.104	0.159
Middle secondary school	0.044	0.216
Specialized upper secondary school	−.093	0.242
Upper secondary school	0.014	0.214
Occupational group (Ref.: Manufacturing)		
Agriculture	−.249	0.112
Technical occupations	−.721**	0.282
Services	−.412*	0.083
Others	−.621**	−.340
Professional qualification (Ref.: Workers with tertiary education)		
Workers with technical college education	0.101	−.038
Skilled workers	0.054	0.046
Unskilled workers	0.184	0.059
Duration of previous unemployment (Ref.: $<$ 1 month)		
$\geq$ 1 month - 3 months	−1.633***	−.908***
$\geq$ 3 months - $<$ 6 months	−1.470***	−.950***
$\geq$ 6 months - $<$ 1 year	−1.621***	−1.080***
$\geq$ 1 year - $<$ 2 years	−1.752***	−1.123***
$\geq$ 2 years	−1.312***	−1.183***
Professional experience (Ref.: Without professional experience)		
With professional experience	−.129	−.261**
Duration of last employment (in months)	0.001	0.002***
Number of placement offers	−.006	−.010**
Remaining unemployment benefit entitlement (in months)	−.028***	−.023***
Unemployment benefit level (in Euros)	−.029***	0.024***
Daily income from regular employment in first half of 2003 (in Euros)	−.002	−.002
Employment status before job-seeking (Ref.: Employment)		
Self-employed	0.295	−.393*
School attendance/never employed before/apprenticeship	0.373**	0.244*
Unemployable	0.202	−.079
Other, but at least once employed before	0.458***	0.246*
Other	0.307	0.445

Table 2.18 to be continued.

Table 2.18 continued.

	Start-up Subsidy vs. Non-Participation	Bridging Allowance vs. Non-Participation
Regional cluster (Ref.: II a)		
II b	0.71**	0.198
III a	0.726**	0.021
III b	0.543*	0.14
III c	0.58*	0.086
IV	0.885***	0.156
V a	0.63*	0.39
V b	0.693**	−.077
V c	0.762**	0.224
Intergenerational transmission		
Parents are/were self-employed	0.462***	0.44***
Willing to take risk: Risk attitude $\geq$ 7 (Ref.: Unwilling to take risk)	0.211**	0.236***
Constant	1.248**	−.578
Number of observations		
Participants	472	756
Non-Participants	853	853
Hit-Rate (share of correct predictions in %)	67.00	63.08
Pseudo R^2	0.200	0.110
Log-likelihood	−690.334	−990.198

Note: Men in West Germany. * 10%, ** 5%, *** 1% significance level.

Table 2.19: Sensitivity to Unobserved Heterogeneity – Bounding Approach

Γ	Outcome variable: Not unemployed				Outcome variable: Self-employed or regular employed			
	SUS vs. NP		BA vs. NP		SUS vs. NP		BA vs. NP	
	Q^+	p^+	Q^+	p^+	Q^+	p^+	Q^+	p^+
After 56 months since start-up								
1.00	3.721	0.000	8.596	0.000	4.473	0.000	10.332	0.000
1.25	2.355	0.009	7.163	0.000	2.862	0.002	8.616	0.000
1.50	1.252	0.105	6.034	0.000	1.558	0.060	7.254	0.000
1.75	0.325	0.372	5.107	0.000	0.460	0.323	6.128	0.000
2.00	0.307	0.379	4.321	0.000	0.346	0.364	5.169	0.000
Critical values								
1%	1.25 - 1.30		2.80 - 2.85		1.30 - 1.35		3.00 - 3.05	
5%	1.40 - 1.45		3.20 - 3.25		1.45 - 1.50		3.30 - 3.35	
10%	1.45 - 1.50		3.35 - 3.40		1.55 - 1.60		3.45 - 3.50	

Note: Men in West Germany. Reported results are achieved by using *mhbounds.ado* (see Becker and Caliendo, 2007). Critical values are related to the exact values of Γ at which results turn insignificant. BA - Bridging Allowance, SUS - Start-up Subsidy, NP - Non-Participation.

Table 2.20: Sensitivity to Unobserved Heterogeneity – Simulation Approach

Confounder	Influence of unobserved confounder on: Outcome	Influence of unobserved confounder on: Selection	ATT (S.E.)
Start-up Subsidy vs. Non-Participation			
No unobserved heterogeneity (see Table 2.7)	0.00	0.00	0.22 (0.04)
Confounder with an influence like (see Table 2.16)			
Age bracket (25 - 29 years)	2.24	1.52	0.22 (0.01)
Upper secondary school	2.28	0.76	0.23 (0.01)
Duration of previous unemployment (1 month - < 3 months)	1.50	0.65	0.22 (0.01)
Parents are/were self-employed	1.66	2.19	0.21 (0.01)
Bridging Allowance vs. Non-Participation			
No unobserved heterogeneity (see Table 2.7)	0.00	0.00	0.14 (0.02)
Confounder with an influence like (see Table 2.16)			
Age bracket (25 - 29 years)	2.18	1.01	0.15 (0.00)
Upper or upper secondary school	2.34	1.37	0.14 (0.00)
Duration of previous unemployment (1 month - < 3 months)	1.45	1.02	0.14 (0.00)
Parents are/were self-employed	1.63	2.19	0.14 (0.01)

Note: Men in West Germany. Reported results are achieved by using *sensatt.ado* (see Nannicini, 2007) and are related to the binary outcome variable "self-employment or regular employment" measured 56 months after start-up. The first two columns show the effect of an unobserved confounder distributed like particular observable confounders on the untreated outcome and on the selection into treatment. Thereby, a value below (above) one indicates a negative (positive) impact. In case of no unobserved heterogeneity, the unobserved term is excluded and both impacts are zero.

Table 2.21: Distribution of Participants and Non-Participants Along the Propensity Score Distribution

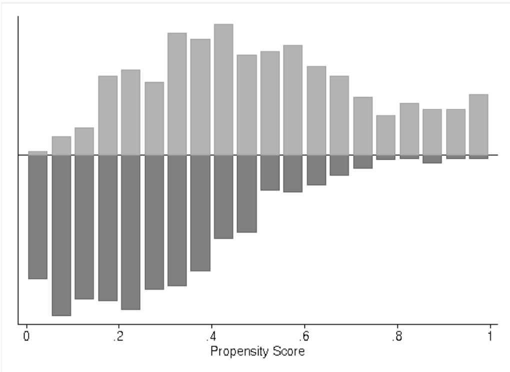
	Start-up Subsidy vs. Non-Participation: Participants	Start-up Subsidy vs. Non-Participation: Non-Participants	Bridging Allowance vs. Non-Participation: Participants	Bridging Allowance vs. Non-Participation: Non-Participants
Propensity scores				
< 0.1	1.48	19.93	0.13	0.59
0.1 until < 0.2	7.41	20.28	2.12	7.74
0.2 until < 0.3	11.02	20.16	6.48	20.16
0.3 until < 0.4	16.53	17.23	14.15	24.03
0.4 until < 0.5	16.10	11.25	21.56	20.87
0.5 until < 0.6	14.83	5.04	16.80	14.77
0.6 until < 0.7	11.65	3.52	18.52	8.09
0.7 until < 0.8	6.78	1.29	11.38	2.46
0.8 until < 0.9	6.78	0.82	5.69	1.06
0.9 until 1	7.42	0.47	3.17	0.23

Note: All results in percentages. Propensity scores are estimated using the final specification as presented in Table 2.16. For instance, 1.48% of Start-up Subsidy participants have estimated propensity scores below 0.1.

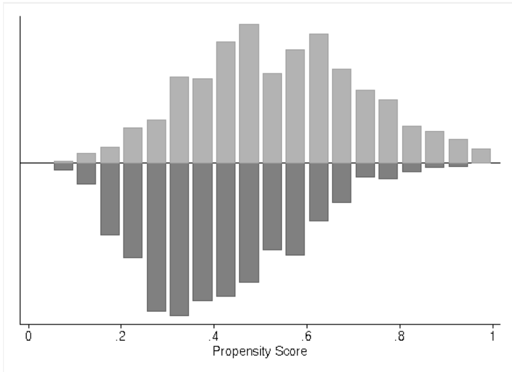
Figure 2.8: Propensity Score Distributions

Estimated using the final specification as depicted in Table 2.16

Start-up Subsidy vs. Non-Participation

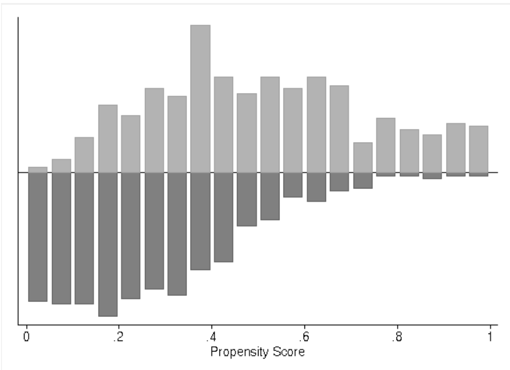


Bridging Allowance vs. Non-Participation

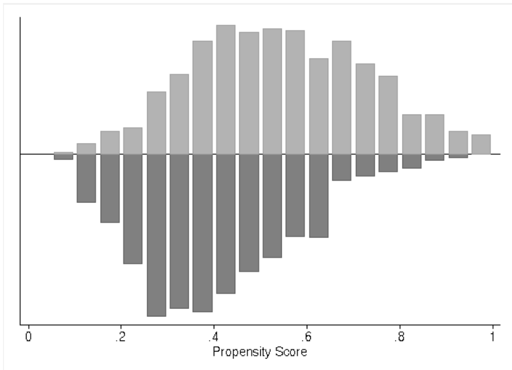


Estimated using the extended specification as depicted in Table 2.18

Start-up Subsidy vs. Non-Participation



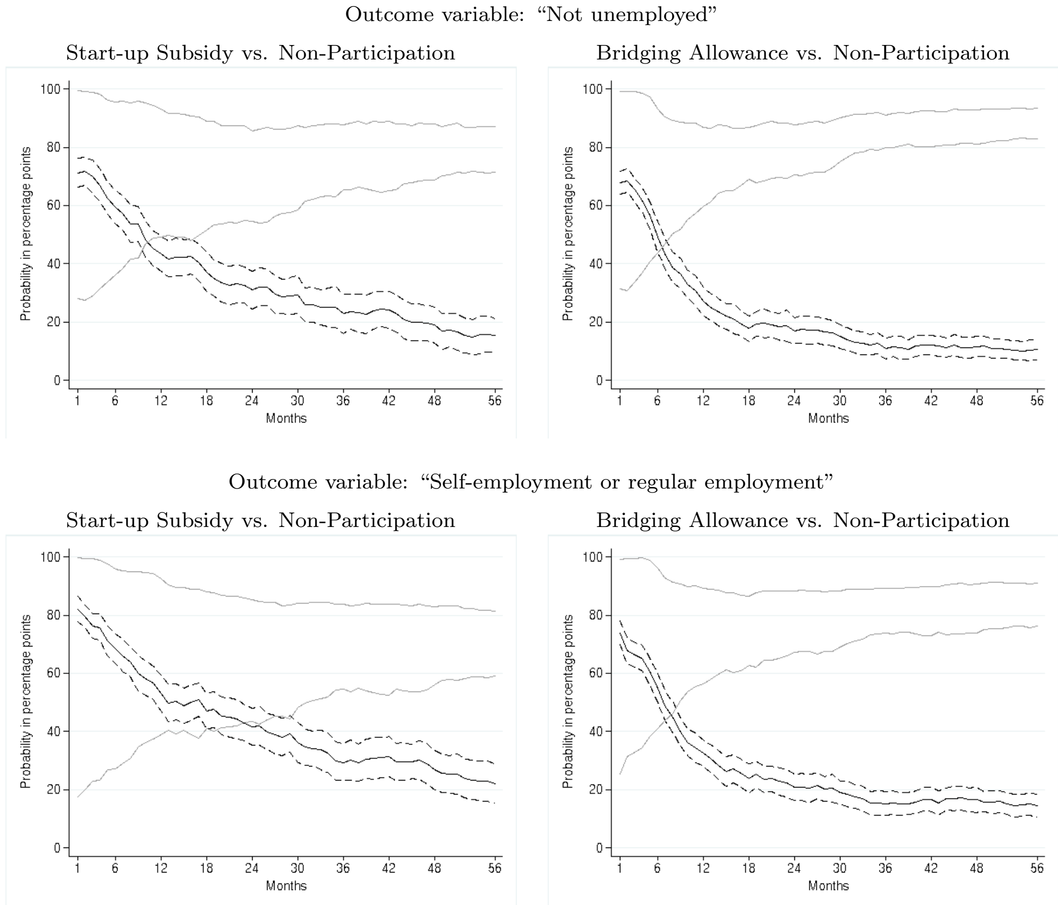
Bridging Allowance vs. Non-Participation



Participants Non-Participants

Note: Depicted are propensity score distributions for male participants and non-participants in West Germany.

Figure 2.9: Causal Effects and Gross Levels of Start-up Subsidy and Bridging Allowance Over Time



Note: Depicted are average treatment effects on the treated (solid line), i.e., the difference in outcome variables between male participants and non-participants in West Germany. We provide 5% confidence intervals for the ATT (dashed lines), which are based on *bootstrapped* standard errors with 200 replications. Moreover, the solid gray lines indicate gross levels of the ATT, i.e., due to persistent positive ATT, the upper (lower) gray lines indicate the gross probability of participants (matched non-participants).

2.10.2 Appendix to 2.7

Table 2.22: Matching Quality Across Subgroups: Educational level

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
	Low educated			
t-test of equal means[a)]				
1%-level	13	0	9	1
5%-level	21	1	13	1
10%-level	26	1	19	1
Mean standardized bias	12.987	3.753	10.833	2.244
Number of variables with standardized bias of a certain amount				
$< 1\%$	2	12	7	12
1% until $< 3\%$	8	9	4	26
3% until $< 5\%$	8	16	3	12
5% until $< 10\%$	7	15	14	4
$\geq 10\%$	29	2	26	0
Pseudo-R^2	0.169	0.015	0.136	0.007
	Highly educated			
t-test of equal means[a)]				
1%-level	9	0	3	0
5%-level	15	1	6	1
10%-level	24	2	8	1
Mean standardized bias	17.737	7.393	6.861	3.375
Number of variables with standardized bias of a certain amount				
$< 1\%$	3	2	5	11
1% until $< 3\%$	1	8	16	18
3% until $< 5\%$	2	12	9	11
5% until $< 10\%$	10	18	13	11
$\geq 10\%$	36	12	10	2
Pseudo-R^2	0.301	0.059	0.112	0.013

Note: Men in West Germany.
[a)] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 54 observable variables in total.

Table 2.23: Matching Quality Across Subgroups: Professional Qualification

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
Low qualified				
t-test of equal means$^{a)}$				
1%-level	14	0	8	1
5%-level	19	1	12	1
10%-level	24	1	16	1
Mean standardized bias	12.615	4.145	10.007	2.822
Number of variables with standardized bias of a certain amount				
$< 1\%$	3	9	3	15
1% until $< 3\%$	1	12	3	17
3% until $< 5\%$	12	14	9	11
5% until $< 10\%$	13	19	22	11
$\geq 10\%$	25	0	17	0
Pseudo-R^2	0.177	0.019	0.126	0.008
Highly qualified				
t-test of equal means$^{a)}$				
1%-level	9	2	3	0
5%-level	12	4	5	1
10%-level	18	4	7	1
Mean standardized bias	19.008	14.048	9.002	4.166
Number of variables with standardized bias of a certain amount				
$< 1\%$	2	1	2	8
1% until $< 3\%$	2	4	5	15
3% until $< 5\%$	4	5	8	11
5% until $< 10\%$	9	15	21	15
$\geq 10\%$	34	26	16	3
Pseudo-R^2	0.082	0.000	0.128	0.020

Note: Men in West Germany.
$^{a)}$ Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 54 observable variables in total.

Table 2.24: Matching Quality Across Subgroups: Age

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
		≤ 30		
t-test of equal means[a)]				
1%-level	3	2	2	5
5%-level	4	6	8	7
10%-level	7	5	11	8
Mean standardized bias	12.457	9.968	14.709	14.308
Number of variables with standardized bias of a certain amount				
$< 1\%$	2	5	2	1
1% until $< 3\%$	6	9	5	4
3% until $< 5\%$	8	9	4	3
5% until $< 10\%$	10	12	9	11
$\geq 10\%$	25	16	30	31
Pseudo-R^2	0.006	0.000	0.007	0.000
		> 30		
t-test of equal means[a)]				
1%-level	16	0	9	1
5%-level	29	1	12	1
10%-level	32	1	18	1
Mean standardized bias	15.779	3.74	8.765	2.492
Number of variables with standardized bias of a certain amount				
$< 1\%$	0	8	5	15
1% until $< 3\%$	4	20	6	23
3% until $< 5\%$	3	11	10	7
5% until $< 10\%$	14	13	20	9
$\geq 10\%$	33	2	13	0
Pseudo-R^2	0.197	0.017	0.109	0.008

Note: Men in West Germany.
[a)] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 51 observable variables in total.

Table 2.25: Matching Quality Across Subgroups: Nationality

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
Native				
t-test of equal means[a)]				
1%-level	19	0	7	1
5%-level	23	1	11	1
10%-level	27	1	19	1
Mean standardized bias	15.296	3.424	8.753	2.197
Number of variables with standardized bias of a certain amount				
$< 1\%$	3	11	3	16
1% until $< 3\%$	0	20	6	26
3% until $< 5\%$	3	8	14	6
5% until $< 10\%$	16	14	15	7
$\geq 10\%$	33	2	17	0
Pseudo-R^2	0.209	0.016	0.110	0.007
Non-German				
t-test of equal means[a)]				
1%-level	8	2	5	0
5%-level	13	6	13	1
10%-level	19	8	16	1
Mean standardized bias	14.696	11.871	12.263	5.202
Number of variables with standardized bias of a certain amount				
$< 1\%$	3	4	7	5
1% until $< 3\%$	4	10	3	16
3% until $< 5\%$	7	3	5	10
5% until $< 10\%$	10	14	14	16
$\geq 10\%$	31	24	26	8
Pseudo-R^2	0.096	0.000	0.188	0.032

Note: Men in West Germany.
[a)] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 55 observable variables in total.

Table 2.26: Effect Heterogeneity: Causal Effects of Start-up Subsidy and Bridging Allowance

	Start-up Subsidy vs. Non-Participation		Bridging Allowance vs. Non-Participants	
	Main results			
# Treated	472		756	
# Controls	853		853	
Outcome variable: "Self-employed or regular employed"				
After 36 months (in %-points)	29.4		15.3	
After 56 months (in %-points)	22.1		14.5	
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	23.5		14.6	
Outcome variable: "Income 56 months after start-up" (in €/month)				
Working income	435		618	
Equivalent income[a)]	(248)		546	
	Educational level			
	Low	High	Low	High
# Treated	322	150	400	356
# Controls	518	335	518	335
Outcome variable: "Self-employed or regular employed"				
After 36 months (in %-points)	29.6	25.5	20.0	10.6
After 56 months (in %-points)	23.7	17.6	19.2	11.7
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	24.5	19.0	17.1	12.8
Outcome variable: "Income 56 months after start-up" (in €/month)				
Working income	616	(-100)	416	768
Equivalent income[a)]	(328)	(-23)	286	732
	Professional qualification			
	Low	High	Low	High
# Treated	382	90	475	281
# Controls	592	261	592	261
"Outcome variable: Self-employed or regular employed"				
After 36 months (in %-points)	27.3	16.3	15.8	12.7
After 56 months (in %-points)	20.5	11.5	17.1	12.4
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	23.5	15.4	16.1	12.5
Outcome variable: "Income 56 months after start-up" (in €/month)				
Working income	628	-464	486	865
Equivalent income[a)]	353	(-189)	439	725
	Age			
	≤ 30	> 30	≤ 30	> 30
# Treated	112	360	110	646
# Controls	141	712	141	712
Outcome variable: "Self-employed or regular employed"				
After 36 months (in %-points)	21.9	27.0	20.1	15.7
After 56 months (in %-points)	(8.7)	21.3	10.5	16.2
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	17.9	23.4	18.7	14.8
Outcome variable: "Income 56 months after start-up" (in €/month)				
Working income	543	374	914	573
Equivalent income[a)]	506	(242)	761	525

Table to be continued.

Table 2.26 continued.

	Start-up Subsidy vs. Non-Participation		Bridging Allowance vs. Non-Participants	
	Nationality			
	Native	Non-German	Native	Non-German
# Treated	317	155	556	200
# Controls	641	212	641	261
Outcome variable: "Self-employed or regular employed"				
After 36 months (in %-points)	27.3	20.6	15.9	10.6
After 56 months (in %-points)	20.0	15.7	14.2	14.5
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	22.0	21.1	15.3	12.4
Outcome variable: "Income 56 months after start-up" (in €/month)				
Working income	(305)	(249)	612	587
Equivalent income[a)]	(147)	(339)	547	543

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between male participants and non-participants in West Germany. The educational level is decomposed into "high" education, capturing individuals who have successfully completed upper secondary school, and "low" education, including individuals who have either not completed school or have completed lower or middle secondary school. With respect to professional qualifications we define individuals with tertiary or technical college education as "highly" qualified, while skilled or unskilled workers are categorized as "low" qualified. Effects which are not significant different from zero at the 5%-level are in parentheses; standard errors are based on *bootstrapping* with 200 replications.

[a)] See Table 2.5 for definition of equivalent income.

Table 2.27: Matching Quality Across Regional Subgroups: Unemployment rate

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
	Low unemployment rate			
t-test of equal means[a)]				
1%-level	7	1	7	0
5%-level	14	2	11	1
10%-level	18	4	12	1
Mean standardized bias	16.129	9.309	11.481	3.374
Number of variables with standardized bias of a certain amount				
$< 1\%$	1	2	4	13
1% until $< 3\%$	1	6	6	16
3% until $< 5\%$	5	3	6	10
5% until $< 10\%$	12	24	12	14
$\geq 10\%$	35	19	26	1
Pseudo-R^2	0.269	0.059	0.177	0.015
	Medium unemployment rate			
t-test of equal means[a)]				
1%-level	13	0	3	0
5%-level	19	1	5	1
10%-level	22	1	7	1
Mean standardized bias	16.784	7.339	8.876	4.478
Number of variables with standardized bias of a certain amount				
$< 1\%$	1	3	4	10
1% until $< 3\%$	5	8	7	15
3% until $< 5\%$	4	9	9	6
5% until $< 10\%$	12	20	18	20
$\geq 10\%$	32	14	16	3
Pseudo-R^2	0.241	0.037	0.129	0.020
	High unemployment rate			
t-test of equal means[a)]				
1%-level	8	0	5	0
5%-level	20	1	13	1
10%-level	24	2	20	1
Mean standardized bias	15.489	5.024	12.037	4.806
Number of variables with standardized bias of a certain amount				
$< 1\%$	4	2	1	7
1% until $< 3\%$	5	18	10	11
3% until $< 5\%$	1	13	5	14
5% until $< 10\%$	12	19	12	19
$\geq 10\%$	33	3	27	4
Pseudo-R^2	0.247	0.032	0.159	0.029

Note: Men in West Germany.
[a)] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 54 to 55 observable variables (depending on PS specification) in total.

Table 2.28: Matching Quality Across Regional Subgroups: Vacancy rate

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
	Low vacancy rate			
t-test of equal means[a]				
1%-level	9	0	5	0
5%-level	14	1	11	1
10%-level	23	1	18	1
Mean standardized bias	14.975	4.130	11.756	4.109
Number of variables with standardized bias of a certain amount				
< 1%	1	12	2	10
1% until < 3%	9	15	5	20
3% until < 5%	3	12	7	6
5% until < 10%	9	12	18	19
≥ 10%	35	6	25	2
Pseudo-R^2	0.231	0.024	0.147	0.026
	Medium vacancy rate			
t-test of equal means[a]				
1%-level	10	1	4	0
5%-level	18	1	8	1
10%-level	22	1	13	1
Mean standardized bias	15.065	5.188	9.770	3.568
Number of variables with standardized bias of a certain amount				
< 1%	3	5	2	13
1% until < 3%	7	12	9	18
3% until < 5%	4	15	7	11
5% until < 10%	12	20	20	10
≥ 10%	31	5	18	4
Pseudo-R^2	0.278	0.008	0.151	0.015
	High vacancy rate			
t-test of equal means[a]				
1%-level	9	1	6	0
5%-level	19	3	11	1
10%-level	24	5	11	1
Mean standardized bias	16.894	8.490	9.719	2.779
Number of variables with standardized bias of a certain amount				
< 1%	3	6	7	14
1% until < 3%	3	12	8	21
3% until < 5%	7	4	4	14
5% until < 10%	9	15	17	6
≥ 10%	34	19	20	1
Pseudo-R^2	0.276	0.041	0.174	0.015

Note: Men in West Germany.
[a] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 56 to 57 observable variables (depending on PS specification) in total.

Table 2.29: Matching Quality Across Regional Subgroups: GDP per capita

	Start-up Subsidy		Bridging Allowance	
	Before matching	After matching	Before matching	After matching
	Low GDP per capita			
t-test of equal means[a]				
1%-level	16	0	5	0
5%-level	19	1	10	2
10%-level	22	3	14	2
Mean standardized bias	16.758	6.575	9.615	5.088
Number of variables with standardized bias of a certain amount				
$< 1\%$	4	7	4	3
1% until $< 3\%$	6	8	7	17
3% until $< 5\%$	5	16	12	13
5% until $< 10\%$	12	12	13	20
$\geq 10\%$	29	13	21	4
Pseudo-R^2	0.319	0.040	0.135	0.027
	Medium GDP per capita			
t-test of equal means[a]				
1%-level	11	0	6	0
5%-level	16	3	9	1
10%-level	21	6	15	1
Mean standardized bias	15.915	7.309	10.450	3.273
Number of variables with standardized bias of a certain amount				
$< 1\%$	2	6	6	8
1% until $< 3\%$	3	11	7	21
3% until $< 5\%$	5	14	6	18
5% until $< 10\%$	14	12	16	11
$\geq 10\%$	34	15	23	0
Pseudo-R^2	0.249	0.052	0.158	0.015
	High GDP per capita			
t-test of equal means[a]				
1%-level	8	0	5	0
5%-level	14	1	13	1
10%-level	17	1	17	1
Mean standardized bias	13.631	4.945	10.867	3.683
Number of variables with standardized bias of a certain amount				
$< 1\%$	2	7	4	11
1% until $< 3\%$	3	12	8	18
3% until $< 5\%$	6	13	11	15
5% until $< 10\%$	20	18	9	9
$\geq 10\%$	25	6	24	3
Pseudo-R^2	0.250	0.034	0.164	0.022

Note: Men in West Germany.

[a] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means. There are 56 to 58 observable variables (depending on PS specification) in total.

Table 2.30: Regional Effect Heterogeneity: Causal Effects of Start-up Subsidy and Bridging Allowance

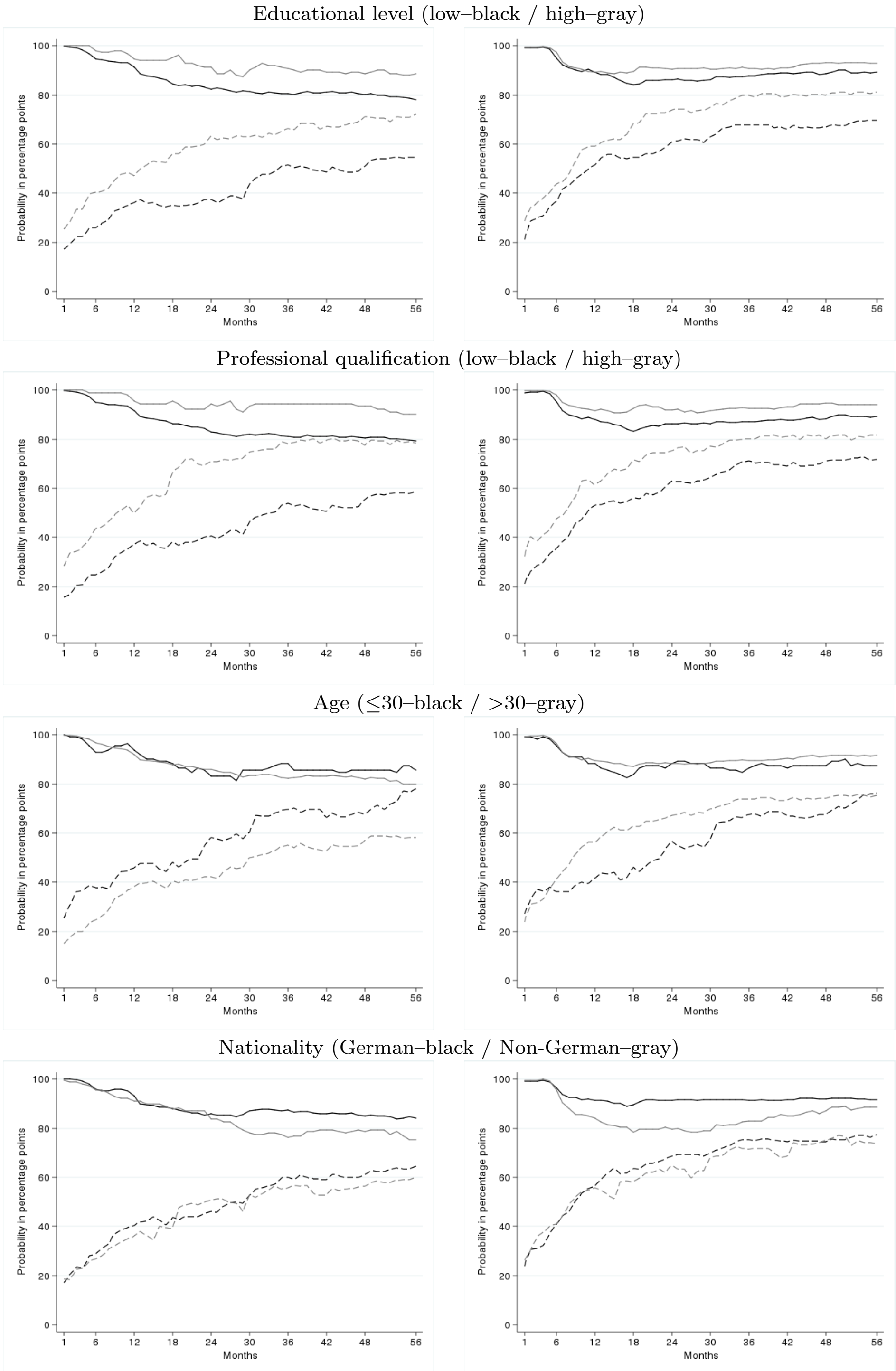
	Start-up Subsidy vs. Non-Participation			Bridging Allowance vs. Non-Participants		
	Main results					
# Treated	472			756		
# Controls	853			853		
Outcome variable: "Self-employed or regular employed"						
After 36 months (in %-points)	29.4			15.3		
After 56 months (in %-points)	22.1			14.5		
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	23.5			14.6		
Outcome variable: "Income 56 months after start-up" (in €/month)						
Working income	435			618		
Equivalent income[a)]	(248)			546		
	Unemployment rate					
	Low	Medium	High	Low	Medium	High
# Treated	155	152	165	269	254	233
# Controls	272	300	281	272	300	281
Outcome variable: "Self-employed or regular employed"						
After 36 months (in %-points)	18.1	31.9	26.4	10.9	17.0	18.8
After 56 months (in %-points)	(9.6)	19.6	22.1	10.3	15.8	18.3
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	16.1	23.2	21.8	13.8	15.5	16.8
Outcome variable: "Income 56 months after start-up" (in €/month)						
Working income	(-253)	531	600	489	828	524
Equivalent income[a)]	(-178)	642	(156)	485	662	436
	Vacancy rate					
	Low	Medium	High	Low	Medium	High
# Treated	169	162	141	254	247	255
# Controls	293	295	265	293	295	265
Outcome variable: "Self-employed or regular employed"						
After 36 months (in %-points)	43.3	20.6	21.7	23.4	12.5	10.6
After 56 months (in %-points)	27.5	19.4	16.3	19.5	11.8	11.6
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	27.6	19.7	18.4	17.7	12.9	13.3
Outcome variable: "Income 56 months after start-up" (in €/month)						
Working income	(544)	514	(110)	568	817	(444)
Equivalent income[a)]	512	309	(102)	558	626	384
	Productivity (GDP per capita)					
	Low	Medium	High	Low	Medium	High
# Treated	141	175	156	266	242	248
# Controls	286	286	281	286	286	281
Outcome variable: "Self-employed or regular employed"						
After 36 months (in %-points)	34.9	23.8	33.9	16.3	17.2	15.6
After 56 months (in %-points)	30.1	17.5	20.7	16.5	16.6	13.8
Total cumulated effect ($\sum_{t=1}^{56}$, in months)	26.5	22.1	24.2	14.4	15.5	15.9
Outcome variable: "Income 56 months after start-up" (in €/month)						
Working income	(377)	638	271	615	467	792
Equivalent income[a)]	(225)	(382)	301	439	413	714

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between male participants and non-participants in West Germany. Effects which are not significant different from zero at the 5%-level are in parentheses; standard errors are based on *bootstrapping* with 200 replications.

[a)] See Table 2.5 for definition of equivalent income.

Figure 2.10: Effect Heterogeneity: Probability Levels Among Participants and Matched Non-Participants

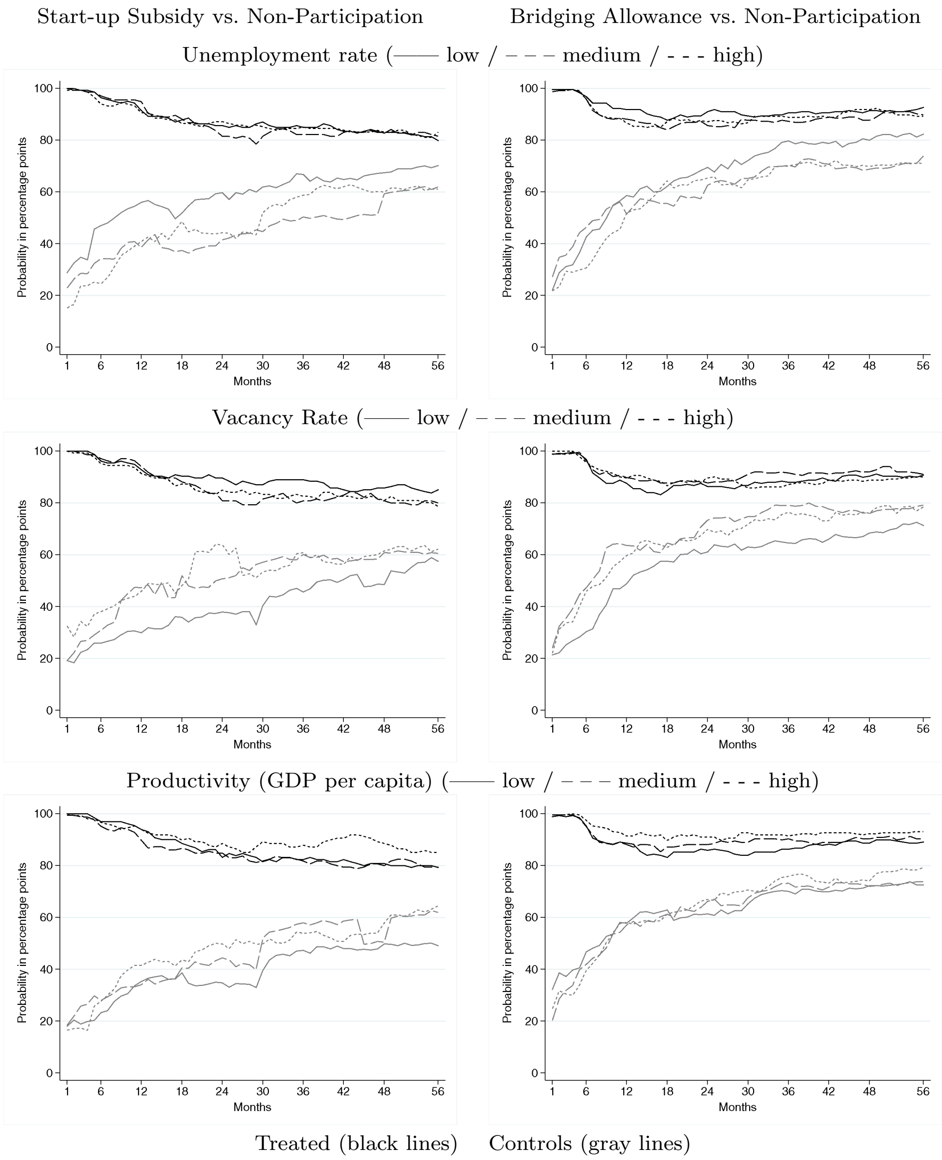
Start-up Subsidy vs. Non-Participation | Bridging Allowance vs. Non-Participation



—— Treated Controls - - -

Note: Depicted are probability levels for the outcome variable "self-employment or regular employment" among male participants and non-participants in West Germany within the matched sample, i.e., the difference between the solid and dashed line is the average treatment effect on the treated. For instance, consider the case of start-up subsidy vs. non-participation on the left panel. After 56 months 88.7% (72.1%) of the highly educated participants (matched non-participants) are in self-employment or regular employment; while only 78.0% (54.5%) of the low educated participants (matched non-participants) are either self-employed or regular employed.

Figure 2.11: Regional Effect Heterogeneity: Probability Levels Among Participants and Matched Non-Participants



Note: Depicted are probability levels for the outcome variable "self-employment or regular employment" among male participants and non-participants in West Germany within the matched sample, i.e., the difference between the solid and dashed line is the average treatment effect on the treated. For instance, consider the case of start-up subsidy vs. non-participation on the left panel. 79.9% (70.2%) of participants (matched non-participants) who were located in an area with low unemployment rates in the 3rd quarter 2003 are in self-employment or regular employment 56 months after start-up; this applies to 83.1% (61.0%) of participants (matched non-participants) who were located in areas with high unemployment rates.

2.10.3 Appendix to 2.8

Table 2.31: Propensity Score Estimation: Female Participants vs. Non-Participation

	Start-up Subsidy		Bridging Allowance	
	West	East	West	East
Age bracket (Ref.: 18 to 24 years)				
25 to 29 years	0.677**	−.358	0.117	−.213
30 to 34 years	0.466	−.318	0.387	−.142
35 to 39 years	0.484	−.019	0.449	−.083
40 to 44 years	0.671**	−.215	0.623*	−.130
45 to 49 years	0.631*	0.038	0.538	−.132
50 to 64 years	0.803**	0.008	0.794**	−.340
Marital status (Ref.: Single)				
Married	0.014	0.132	−.155	−.001
Number of children in household (Ref.: No children)				
one child	−.020	−.118	0.063	−.099
Two or more children	−.158	−.118	−.130	−.010
Health restriction that affect job placement (Ref.: No)				
Yes	−.026	−1.099**	0.363	0.046
Nationality (Ref.: German)				
Non-German	−.043	0.087	−.171	0.059
Desired working time (Ref.: Part-time)				
Full-time	−.036	0.084	0.499***	0.129
School achievement (Ref.: None)				
Lower secondary school	0.304		−.454	
Middle secondary school	0.399	−.255	−.159	0.052
Specialized upper secondary school	0.239	−.045	−.158	0.091
Upper secondary school	0.388	−.029	−.329	0.135
Occupational group (Ref.: Manufacturing)				
Agriculture	−.153	−.407	0.671	0.025
Technical occupations	0.115	0.614	0.654	0.466
Services	−.238	0.140	0.703	0.232
Others	−.443	0.150	0.376	0.145
Professional qualification (Ref.: Workers with tertiary education)				
Workers with technical college education	−.102	0.146	0.171	0.028
Skilled workers	−.197	0.407	−.138	0.097
Unskilled workers	−.019	0.532	−.252	0.125
Duration of previous unemployment (Ref.: $<$ 1 month)				
$\geq$ 1 month - 3 months	−1.427***	−2.089***	−1.577***	−.560
$\geq$ 3 months - $<$ 6 months	−1.904***	−1.822***	−1.812***	−.526
$\geq$ 6 months - $<$ 1 year	−1.554***	−1.647***	−1.768***	−.418
$\geq$ 1 year - $<$ 2 years	−1.682***	−1.814***	−1.897***	−.407
$\geq$ 2 years	−1.456***	−1.176**	−2.159***	−.490
Professional experience (Ref.: Without professional experience)				
with professional experience	0.049	0.060	−.145	0.032
Duration of last employment (in months)	0.0002	0.003	0.002	0.003*
Number of placement offers	−.010	−.024*	−.014	−.006
Remaining unemployment benefit entitlement (in months)	−.022**	−.022	−.016	−.010
Daily unemployment benefit level (in Euros)	−.020***	−.022**	0.023***	−.003
Daily income from regular employment in the first half of 2003 (in Euros)	−.005*	−.002	−.004	0.001
Employment status before job-seeking (Ref.: Employment)				
Self-employed	0.212	0.821**	−.748*	−.254
School attendance/never employed before/ apprenticeship	0.19	−.100	−.023	−.032
Unemployable	0.237	0.757***	0.07	0.097
Other, but at least once employed before	0.344**	0.011	0.415**	−.176
Other	−.343	0.371		0.055

Table to be continued.

Table 2.31 continued.

	Start-up Subsidy		Bridging Allowance	
	West	East	West	East
Regional cluster (Ref.: II a)				
I a		0.030		−.296
I b		−.223		−.119
I c		−.033		−.030
II b	0.453		−.472	
III a	0.299		−.721**	
III b	0.495		−.416	
III c	0.408		−.889***	
IV	0.46		−.390	
V a	−.002		−.981**	
V b	0.56		−.627*	
V c	0.647		−.759**	
Intergenerational transmission				
Parents are/were self-employed	0.187*	0.550***	0.103	−.044
Constant	0.902	1.566**	0.415	−.454***
Number of observations				
Participants	438	183	228	135
Non-participants	525	250	518	250
Hit-Rate (share of correct predictions in %)	64.07	68.59	68.13	68.31
Pseudo R^2	0.121	0.187	0.181	0.041
Log-likelihood	−583.361	−239.643	−376.267	−239.205

Note: * 10%, ** 5%, *** 1% significance level.

Table 2.32: Individual Characteristics of Female Participants at Business Start-up in Comparison to Female Non-Participants and Male Participants

	Female Participants		Difference[a)] to female non-participants[a)]		Difference[a)] to male participants	
	West	East	West	East	West	East
Start-up Subsidy						
Age (in years)	39.1	40.9	−0.0	+0.4	+1.8*	+1.3
Married	55.7	69.5	+2.0	+6.0	+11.4***	+10.1**
At least one child	49.2	46.4	+4.0	−2.9	+23.7***	+10.5**
Non-German	29.6	38.0	+0.5	+0.4	−6.1	+9.8*
Daily unemployment benefit level (in Euro)	17.5	16.1	−4.0***	−3.6***	−6.1***	−5.3***
School leaving certificate						
No or lower secondary degree	31.0	13.9	+3.2	+5.6*	−18.8***	−2.6
Middle secondary degree	33.5	55.9	+0.8	−5.7	+9.8***	−3.5
Specialized and upper secondary school	36.4	30.2	−4.8	−1.4	+10.0***	+5.2
Intergenerational transmission						
Parents are/were self-employed	27.9	27.2	+4.9	+13.4***	−1.6	+8.1*
General willingness to take risk[b)] (Scale: 0=complete unwillingness; 10=complete willingness						
Mean	5.4	5.5	+0.3	+0.2	−0.4*	−0.3
Bridging Allowance						
Age (in years)	38.2	40.4	−0.9	−0.0	−0.5	+2.1
Married	37.1	64.4	−16.6***	+0.8	−23.2***	+5.7
At least one child	24.7	46.8	−20.5***	−2.5	−12.9***	+12.1**
Non-German	26.0	31.7	−3.1	−5.8	−3.2	+12.1***
Daily unemployment benefit level (in Euro)	29.0	26.0	+7.5***	+6.3***	−9.4***	−3.4**
School leaving certificate						
No or lower secondary degree	17.3	6.1	−10.6**	−2.2	−17.8***	−5.3
Middle secondary degree	24.7	47.7	−8.0**	−13.9**	+1.0	−10.7*
Specialized and upper secondary school	58.0	49.2	+16.9***	+17.6***	+16.7***	+18.1***
Intergenerational transmission						
Parents are/were self-employed	25.5	22.5	+2.5	+8.6**	−3.3	−3.5
General willingness to take risk[b)] (Scale: 0=complete unwillingness; 10=complete willingness						
Mean	5.7	5.8	+0.6**	+0.5*	−0.2	+0.2

Note: All numbers are percentages unless otherwise indicated.
[a)] Positive numbers denote higher values for female participants. Differences are statistically significant at the * 10%, ** 5%, *** 1% level.
[b)] Measured at the second interview, i.e., 28 months after start-up.

Table 2.33: Matching quality

		Start-up Subsidy		Bridging Allowance	
		West Germany	East Germany	West Germany	East Germany
Number of variables		56	50	55	50
t-test of equal means[a]					
1%-level	unmatch	6	8	9	4
	match	1	0	0	2
5%-level	unmatch	11	13	17	14
	match	1	1	1	4
10%-level	unmatch	16	13	21	18
	match	1	1	1	6
Standardized bias					
Mean standardized bias	unmatch	9.354	11.816	13.606	13.928
	match	2.694	6.549	3.738	8.289
Number of variables with standardized bias of a certain amount					
$< 1\%$	unmatch	5	4	2	3
	match	18	2	11	3
1% until $< 3\%$	unmatch	9	6	7	3
	match	20	5	14	11
3% until $< 5\%$	unmatch	4	6	8	6
	match	8	13	15	6
5% until $< 10\%$	unmatch	20	13	9	13
	match	9	21	14	16
$\geq 10\%$	unmatch	18	21	29	25
	match	1	9	1	14
Pseudo R^2	unmatch	0.120	0.187	0.181	0.021
	match	0.009	0.034	0.019	0.001

Note: Women in West and East Germany. Depicted are different statistics to assess the quality of the matching process, i.e., whether the distribution of observable characteristics between female participants and non-participants is sufficiently balanced. Deviant values in terms of Pseudo R^2 compared to Table 2.31 are due to implemented common support conditions, i.e., due to excluded observations.
[a] Depicted is the number of variables which differ significantly between treated and controls. The decision is based on a simple *t-test* of equal means.

Table 2.34: Sensitivity to Estimation Methods

	Women			
	SUS vs. NP		BA vs. NP	
	West Germany	East Germany	West Germany	East Germany
Main results (compare Table 2.14)				
SE or RE ($\sum_{t=1}^{56}$)	26.9 (1.4)	29.8 (2.7)	20.6 (1.7)	25.9 (2.1)
Working income^a)	138 (84)	348 (105)	225 (137)	334 (100)
A) Alternative matching procedure				
Radius-matching with caliper of 0.1				
SE or RE ($\sum_{t=1}^{56}$)	27.0 (1.3)	29.4 (2.2)	21.2 (1.7)	26.0 (1.9)
Working income^a)	137 (84)	339 (118)	235 (133)	356 (93)
B) Common support condition				
Thick support 1 - $0.33 < \hat{P}(W) < 0.67$				
SE or RE ($\sum_{t=1}^{56}$)	27.8 (1.3)	32.6 (2.5)	20.9 (2.7)	23.7 (7.0)
Working income^a)	88 (118)	334 (123)	204 (199)	-191 (393)
Thick support 2 - F($\hat{P}$(W)>5%)				
SE or RE ($\sum_{t=1}^{56}$)	26.7 (1.6)	31.2 (2.3)	22.0 (2.0)	25.9 (2.0)
Working income^a)	54 (129)	335 (116)	94 (183)	334 (104)
Optimal subpopulation				
SE or RE ($\sum_{t=1}^{56}$)	27.0 (1.4)	28.4 (1.8)	21.5 (1.8)	25.9 (2.2)
Working income^a)	135 (86)	351 (114)	208 (144)	334 (108)
C) Conditional difference-in-difference				
CDID1: SE or RE ($\sum_{t=1}^{56}$)	26.3 (1.4)	29.2 (2.3)	21.2 (1.6)	23.9 (1.8)
CDID2: SE or RE ($\sum_{t=1}^{56}$)	26.1 (1.6)	29.1 (2.5)	20.8 (1.6)	23.7 (2.2)
CDID3: SE or RE ($\sum_{t=1}^{56}$)	25.5 (1.7)	28.5 (2.6)	21.4 (1.6)	21.7 (2.1)
CDID4: Working income^a)	119 (97)	427 (128)	369 (165)	-19 (130)

Note: Depicted are average treatment effects on the treated as the difference in outcome variables between female participants and non-participants. Thereby, the outcome variable "self-employment or regular employment" is depicted by "SE or RE". Standard errors are in parentheses and are based on *bootstrapping* with 200 replications. SUS - Start-up subsidy, BA - Bridging allowance, NP - Non-participation.
Common support condition: Thick support: We estimate the effects 1) in a region defined by $0.33 < \hat{P}(W) < 0.67$ and 2) we divide the propensity score distribution into ten deciles and estimate the effects only in regions where we have a density of at least 5% in both groups (participants and non-participants) respectively. *Optimal subpopulation*: The analysis is restricted to a subset of the original sample by keeping individuals with propensity scores inside an optimal common support range ($\alpha < \hat{P}(W) < (1-\alpha)$). The optimal cut-off point α is calculated by using optselect.ado which basically balances two opposing impacts on the variance of the estimated effect (see Crump et al., 2009).
Conditional difference-in-difference: The reference levels for the pre-treatment period are defined as follows: CDID1: July 1998-June 2003; CDID2: January 2001-June 2003; CDID3: July 1998-Dec. 2000; CDID4: Average monthly total income in 2002.
^{a}) In €/month, $t = 56$.

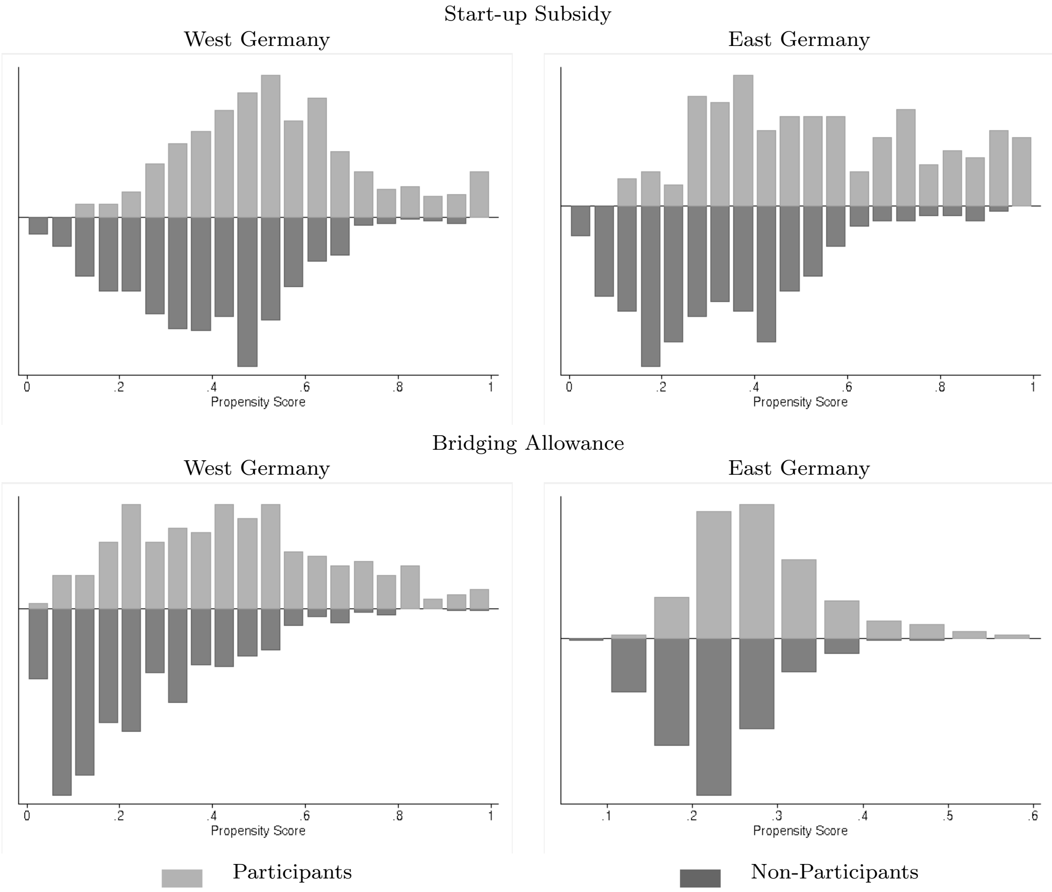
Table 2.35: Sensitivity to Unobserved Heterogeneity

	Women			
	SUS vs. NP		BA vs. NP	
	West Germany	East Germany	West Germany	East Germany
No unobserved heterogeneity (compare Table 2.14)	0.26 (0.04)	0.38 (0.06)	0.23 (0.04)	0.33 (0.05)
Bounding approach				
Exact values of Γ at which results turn insignificant at the 5%-level	1.90 - 1.95	2.15 - 2.20	3.95 - 4.00	3.30 - 3.35
Simulation approach				
Confounder with an influence like (compare Table 2.31)				
Duration of prev. unemployment ($\geq$ 1 month - < 3 months)	0.26 (0.00)	0.38 (0.02)	0.23 (0.01)	0.22 (0.01)
Parents are/were self-employed	0.26 (0.00)	0.36 (0.02)	0.23 (0.00)	0.21 (0.02)

Note: The outcome variable "self-employment or employment" 56 months after start-up is considered. SUS - Start-up subsidy, BA - Bridging allowance, NP - Non-participation.
Bounding approach: Results are achieved by using mhbounds.ado (see Becker and Caliendo, 2007).
Simulation approach: Results are achieved by using sensatt.ado (see Nannicini, 2007).

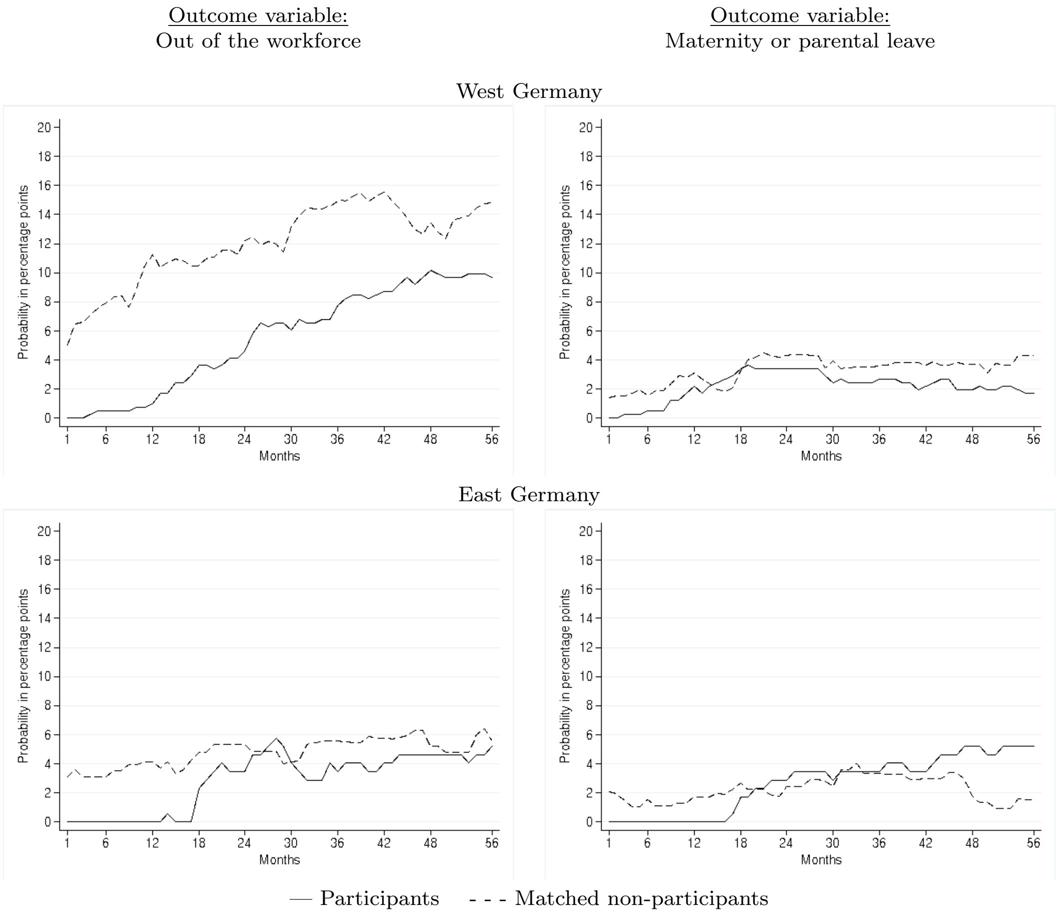
Figure 2.12: Propensity Score Distribution: Female Participants vs. Non-Participants

Estimated using the final specification as depicted in Table 2.31



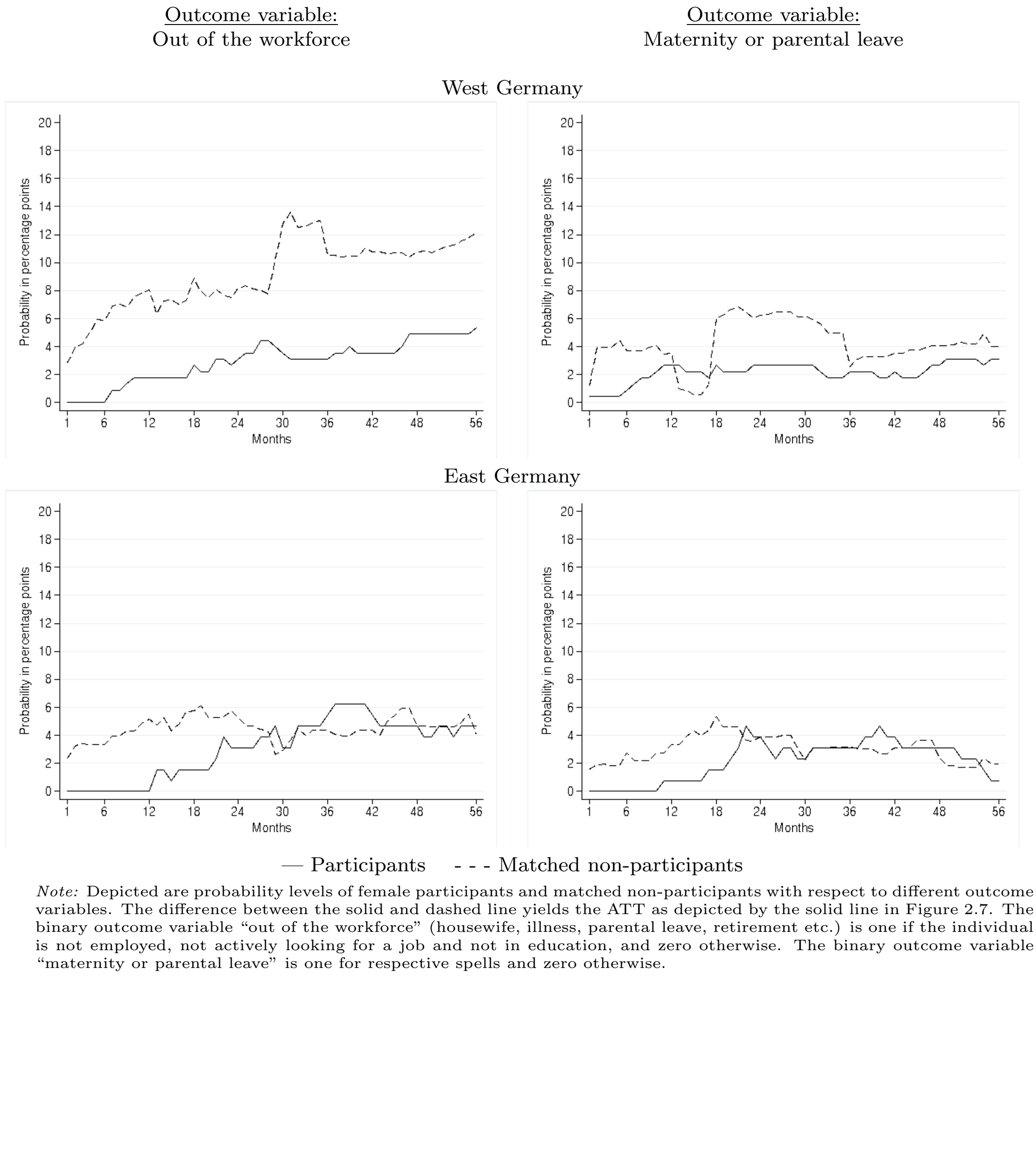
Note: Depicted are propensity score distributions for female participants and non-participants in West and East Germany.

Figure 2.13: Probability Levels of Female Participants and Matched Non-Participants Over Time - Start-up Subsidy



Note: Depicted are probability levels of female participants and matched non-participants with respect to different outcome variables. The difference between the solid and dashed line yields the ATT as depicted by the solid line in Figure 2.6. The binary outcome variable "out of the workforce" (housewife, illness, parental leave, retirement etc.) is one if the individual is not employed, not actively looking for a job and not in education, and zero otherwise. The binary outcome variable "maternity or parental leave" is one for respective spells and zero otherwise.

Figure 2.14: Probability Levels of Female Participants and Matched Non-Participants Over Time - Bridging Allowance



Note: Depicted are probability levels of female participants and matched non-participants with respect to different outcome variables. The difference between the solid and dashed line yields the ATT as depicted by the solid line in Figure 2.7. The binary outcome variable "out of the workforce" (housewife, illness, parental leave, retirement etc.) is one if the individual is not employed, not actively looking for a job and not in education, and zero otherwise. The binary outcome variable "maternity or parental leave" is one for respective spells and zero otherwise.

3 Marginal Employment and the Impact for the Unemployed

In some countries including Germany unemployed workers can increase their income during job search by taking up "marginal employment" up to a threshold without any deduction from their benefits. Marginal employment can be considered as a wage subsidy as it lowers labor costs for firms owing to reduced social security contributions, and increases work incentives due to higher net earnings. Additional earnings during unemployment might lead to higher reservation wages prolonging the duration of unemployment, yet also giving unemployed individuals more time to search for better and more stable jobs. Furthermore, marginal employment might lower human capital deterioration and raise the job arrival rate due to network effects. To evaluate the impact of marginal employment on unemployment duration and subsequent job quality, we consider a sample of fresh entries into unemployment. Our results suggest that marginal employment leads to more stable post-unemployment jobs, has no impact on wages, and increases the job-finding probability if it is related to previous sectoral experience of the unemployed worker. We find evidence for time-varying treatment effects: whilst there is no significant impact during the first twelve months of unemployment, job finding probabilities increase after one year and the impact on job stability is stronger if the jobs are taken up later within the unemployment spell.[47]

[47] This chapter is based on joint work with Marco Caliendo and Arne Uhlendorff (Caliendo et al., 2012).

3.1 Introduction

Unemployment insurance (UI) systems provide benefit payments for unemployed job seekers. The amount of benefits usually depends on previous earnings and declines in accordance with the elapsed unemployment duration. Many studies have shown that more generous benefit schemes correspond with longer unemployment durations, while the empirical evidence of benefit generosity on job match quality is rather mixed and only some studies find positive impacts on post unemployment outcomes.[48] In general, UI systems have to strike a balance between the insurance component and the aim of providing the opportunity to search for suitable job matches on the one hand and disincentive effects and moral hazard on the other hand.

Besides a decreasing profile of benefit payments, different strategies exist to increase the outflow probability from unemployment to employment, and to avoid long spells of unemployment. Such strategies comprise active labor market policies (ALMP) including training programs, wage subsidies, public employment measures, job search assistance and monitoring schemes (see Card et al., 2010; Kluve, 2010, for recent overviews of the effectiveness of these program types). In some countries such as Germany and Austria, the UI system is characterized by an additional feature: unemployed workers are allowed to work for some hours during their job search by taking up "marginal employment". This is defined as employment below a certain income threshold with reduced social security contributions (SSC). The main objective of marginal employment (known as "mini-job" in Germany) is to stimulate labor market flexibility in the low wage sector by increasing the attractiveness of those employment schemes to both firms and employees. Reduced social security contributions lead to lower labor costs for firms and higher work incentives resulting from the higher net income for individuals with low earnings. Although marginal employment does not legally belong within active labor market policy programs in Germany, from an economic perspective it is comparable to a wage subsidy. Marginal employment is used by a wide range of labor market groups. This includes individuals with high labor supply elasticities such as women, employed individuals who use it as a secondary job and unemployed individuals. Taking up marginal employment is attractive for unemployed individuals because they are allowed to

[48] For example Belzil (2001), Tatsiramos (2009) and Caliendo et al. (2012) find evidence for positive impacts while van Ours and Vodopivec (2008) and Card et al. (2007) find no impact of the generosity of unemployment benefits on job quality.

keep a certain amount of additional earnings without any benefit reduction and might interact with participation probabilities in other measures of ALMP.

The expected effects of such a policy are ambiguous. On the one hand, marginal employment might increase the probability of taking up a regular job because it may lower human capital deterioration. Moreover, it may be used as a positive screening device or probation period by potential employers before offering a regular job and may increase the probability of receiving job offers due to network effects. However, on the other hand, the additional income should increase the reservation wage for taking up a regular job, which should prolong the unemployment duration. These effects may have an impact on both, the unemployment duration and the job match quality. For example, the increased income due to marginal employment may allow the unemployed to wait for a better and more stable job, which could decrease the risk of re-entering unemployment. Hence, the overall impact of entering marginal employment on subsequent employment outcomes is theoretically ambiguous. It is the aim of this chapter to empirically assess the overall impact of entering marginal employment on the unemployment duration and subsequent job quality of unemployed individuals.

Comparable to the German setting, unemployment insurance systems in Finland and Denmark allow the unemployed workers to take up a part-time job whilst in receipt of unemployment benefits, if they still search for a full-time job. Kyyrä (2010) applies a "timing of events" approach and finds evidence for positive effects on the transition rate to regular jobs for Finland, while Kyyrä et al. (2009) find heterogeneous effects on the expected unemployment duration for Denmark. Both studies do not take post unemployment outcomes into account. However, to evaluate the effectiveness of this kind of policy it is important to know whether taking up a part-time job or marginal employment during unemployment has an impact on the subsequent job quality and whether for example this reduces the probability of re-entering unemployment.

In this chapter we take into account the dynamic selection of unemployed job seekers into marginal employment by applying the "timing of events approach" following Abbring and van den Berg (2003). This approach allows to control for selection into treatment based on both observed and unobserved characteristics. One central assumption of this approach is the no-anticipation assumption, which implies that individuals do not know exactly when a treatment – in this case entering marginal employment – will take place.[49] Since the unemployed workers have to search for a mini-job, with the job finding probability dependent on the job offer arrival rate and the probability that the characteristics of the mini-job are acceptable, it seems very plausible that the event of entering the treatment is – similar to the transition to a regular job – not deterministic. We additionally evaluate the treatment effect on the job match quality, i.e. we extend the model by estimating the duration of subsequent employment spells and a wage equation for the initial wage.[50] This framework furthermore allows to analyze effect heterogeneity with respect to observed characteristics such as age, skill level and the previous working sector, and to investigate whether the treatment effect varies with the elapsed unemployment duration.

The analysis is based on an inflow sample of male workers into unemployment in West Germany in 2001. We observe labor market states of individuals for three years after entering unemployment, and our dataset includes daily wages of employed workers, detailed sectoral information about marginal and regular employment and a firm identifier which allows to investigate at least at a descriptive level whether individuals find a regular job in the same firm in which they have a mini-job.

Our results suggest that having a mini-job does not have any effect on the probability of finding a regular job within the first twelve months of unemployment. However, we find a significantly positive impact on the outflow probability for long-term unemployed workers. Moreover, the jobs taken up by job seekers who entered a mini-job during their unemployment spell are more stable compared with jobs found by the non-treated individuals. These effects are stronger if the jobs are taken up later in the unemployment spell. We do not find any time-varying effects of taking up marginal employment on wages, but find some evidence for effect heterogeneity with respect to observable characteristics: more skilled individuals and individuals who are not working in the construction sector appear to have slightly lower wages if they have taken up a mini-job during unemployment, while a higher local unemployment rate correlates with lower wages for these workers. We find a significantly positive impact on the transition probability to regular employment if the mini-job is in the same sector as the previous regular job.

The chapter is organized as follows: Section 3.2 describes the institutional

[49] It is important to note that this does not imply that the individuals do not know the probability distribution of future events conditional on observable and unobservable characteristics.

[50] For similar approaches in the context of sanction effects see Arni et al. (2012) and van den Berg and Vikström (2009).

background and surveys relevant previous research. Section 3.3 presents the data and descriptive statistics. Section 3.4 describes the econometric approach. The results of the empirical analysis are presented in Section 3.5, and Section 3.6 provides a interim summary of this chapter.

3.2 Institutional Background and Related Literature

3.2.1 Institutional Settings

Marginal employment in Germany is defined as employment below a certain income level or as temporary employment for a fixed period, and is subject to reduced social security contributions. For 2010 the Federal Employment Agency reports about 7.3 million “marginal jobs”, where around two-thirds of these jobs are held by individuals who do not have a regular regular job (including unemployed workers). The idea of marginal employment was primarily developed in the 1960’s – a period in which labor demand exceeded its supply – as an attempt to increase work incentives for groups with traditionally low labor force participation, including students and housewives/-men, etc. At this time the German social security funds were well balanced, so policy makers decided to exempt low-income jobs from SSC to increase the attractiveness of such jobs (cf. Rudolph, 1999).

In the subsequent period marginal employment has been subject to several reforms; however we restrict the discussion to the parts which are relevant for our observation period of 2001 to 2004. The first main reform took place in April 1999, with the total exemption from SSC abolished as a response to firms substituting regular employment with marginal employment to avoid higher SSC in the late 1990s. Since then marginal employment was restricted to a maximum of €325 per month, combined with a working time restriction of 15 hours per week, and temporary employment contracts were restricted to a maximum of two months or 50 working days per year. While employees have been exempted from social security contributions, employers paid only a fixed rate of 22%.

With the reform in April 2003 – known as the “mini-job” reform – the attractiveness of marginal employment was renewed in order to increase labor market flexibility within the low wage sector. Therefore, the income threshold increased from €325 to €400 per month, the working time restriction of 15 hours per week was abolished, and the SSC paid by the employer increased slightly to 23%. While marginal employment as a secondary job was fully subject to SSC and taxes before April 2003, the reform exempted one secondary mini-job from both SSC and taxes for the employee.

Given our focus on the effect of taking up a mini-job during unemployment, we present a brief overview of the German unemployment insurance system. During our observation period from 2001 to 2004 the unemployment insurance system was characterized by two pillars: the unemployment benefits ("Arbeitslosengeld") and means-tested unemployment assistance ("Arbeitslosenhilfe"). Individuals were eligible for unemployment benefits if they were regularly employed subject to social security contributions for at least 12 months within the last three years. The benefit level relates to previous average earnings with a replacement rate of 60% (67% with children living in the household) of net earnings whereby earnings are capped by the social security contribution assessment ceiling.[51] After the unemployment benefit entitlement expired – which ranges in that period from six to 32 months depending on age and the time spent in employment in the previous seven years – individuals become eligible for means-tested unemployment assistance given they are still searching for a job, with a decreased replacement rate of 53% (57% with children). For a detailed overview of the unemployment insurance system in Germany see e.g. Konle-Seidl et al. (2010). In addition to these transfer payments, the unemployed in Germany are allowed to earn additional income through employment. This possibility is intended to encourage the unemployed to take up marginal employment in order to stay attached to the labor market. Therefore, recipients of unemployment benefit are allowed to keep €165/month of additional earnings without suffering a reduction in unemployment benefits as long as their working time does not exceed 15 hours per week. Earnings above this threshold are fully withdrawn.

It is important to note that the mini-job reform in 2003 had no impact on the situation of unemployed workers. The conditions for additional earnings during the receipt of unemployment benefits, i.e., the exemption rate of €165 and working time restrictions of 15 hours per week, remained unchanged across the reform in 2003. Caliendo and Wrohlich (2010) show that only marginal employment as a secondary job and the labor supply of students increased significantly due to the 2003 reform. They do not find any evidence for a significant impact on the unemployed, which is plausible since the incentive for the unemployed to take up marginal employment

[51] The social security contribution assessment ceiling is the maximum amount of earnings which is eligible to social security contribution. In 2001 it amounted to gross earnings of 4,450 €/month and in 2004 to 5,150 €/month in West Germany.

did not change within this reform. Conversely, incentives for individuals in regular employment increased remarkably because income from one single mini-job is totally exempted from social security contributions and taxation. However, this is not part of our analysis and does not influence our results.

3.2.2 Related Literature

There exists a number of empirical studies investigating "stepping stone effects" of different employment types to enter regular jobs. For example, Cockx and Picchio (2011) analyze the impact of short-term jobs on subsequent employment outcomes in Belgium based on a multivariate duration model and find evidence for short-term jobs representing a spring-board to long-term jobs. An earlier example for a multivariate duration model in the stepping stone literature is van den Berg et al. (2002), who find that a job as a medical assistant increases the probability of becoming a medical specialist in the Netherlands. Zijl et al. (2011) employ a similar approach and find that temporary jobs shorten the unemployment duration in the Netherlands but do not lead to a higher proportion of unemployed workers having regular jobs. In Finland, unemployed workers are allowed to take up a part-time or a short full-time job whilst receiving unemployment benefit if they continue searching for a full-time job. Kyyrä (2010) applies a timing of events approach and his results suggest that this might have positive effects on the transition rate to regular jobs. He finds evidence for an increasing impact of taking up a short full-time job over the unemployment duration, i.e., for those who take up a short full-time job shortly after entering unemployment the treatment effect does not differ significantly from zero, but it becomes stronger with the elapsed unemployment duration. For part-time jobs he does not find evidence for effect heterogeneity with respect to the elapsed unemployment duration. Within a similar institutional setting in Denmark Kyyrä et al. (2009) find heterogenous effects of taking up a part-time job during job search on the expected unemployment duration, for example with respect to age, sex and marital status. Neither of the two studies take post-unemployment outcomes into consideration.

There exist two studies investigating the effects of marginal employment on subsequent employment outcomes. Freier and Steiner (2008) analyze the effect of marginal employment as a stepping stone to regular employment in Germany. They find that marginal employment leads to a reduction in future unemployment and slightly increases cumulated earnings. However, they do not find positive effects in terms of time spent in regular employment. In a study for Austria, Böheim and Weber (2011) find that marginally employed workers experience less frequent regular employment, more unemployment and lower wages compared to non-participants. Both studies apply a static propensity score matching approach and rely on the conditional independence assumption which implies that conditional on observable characteristics entering a mini-job is not correlated with unobserved characteristics which have an impact on later outcomes.

3.3 Data and Descriptive Statistics

3.3.1 Dataset and Sample Definition

Our analysis is based on data from the administrative part of the *IZA Evaluation Dataset*[52]. This dataset is based on the *Integrated Employment Biographies* (IEB) by the Institute for Employment Research (IAB) and consists of a random draw of unemployment entries between 2001 and 2008. The IEB consists of different sources, e.g., employment history, benefit recipient history, training participant history and job search history and therefore contains detailed information on employment subject to social security contributions, unemployment and participation in active labor market policy including wages and transfer payments. The data additionally include a broad range of socio-economic characteristic including education, family status and health restrictions. The data do not contain information about the working hours and periods in self-employment, working as a civil servant, or spent in inactivity. From this data we draw a random sample of inflows into unemployment in 2001. The unemployment spell must last at least two weeks and prior to this unemployment entry the individuals have to be employed subject to social security contributions for a minimum duration of three months to ensure that we have a "real" inflow sample into unemployment. Moreover, we exclude individuals who had a mini-job during the three months before entering unemployment because we want to model the inflow into the treatment. We restrict our observation period from 2001 to 2004, since in a major reform of the German UI system was introduced in 2005.

Our sample is based on male individuals in West Germany. We focus on males because nearly all men work full-time if they have a regular job. In contrast, part-time work is much more common among females (see e.g. Haan, 2010). This

[52]For a detailed description of the *IZA Evaluation Dataset* see Caliendo et al. (2011).

implies that in some cases it is difficult to distinguish between preferred part-time jobs and mini-jobs for women during job searches in our dataset. Furthermore, the high share of part-timers among women renders an evaluation of wages in the first job after leaving unemployment difficult as we do not observe working hours. Since East and West Germany still differ substantially in terms of economic and labor market indicators during our observation period, we exclude East Germany from the analysis. As we are interested in the transition to regular employment and subsequent job stability, the adverse labor market conditions in East Germany might have distorting effects, making results difficult to interpret and transfer to other countries. Moreover, the share of unemployed individuals entering public employment programs is clearly higher in East than West Germany. Therefore, focusing on men in West German leads to a relatively homogeneous estimation sample. Nevertheless, analyzing differences between East and West Germany would be an interesting avenue for further research. We further restrict our sample to men aged between 25 and 55. The lower age restriction is motivated by the educational system, and the upper by the retirement schemes in Germany. Our final sample thus consists of 24,131 individuals out of which we randomly draw an estimation sample of 10,000 individuals to reduce the computational burden. We follow each individual for 36 months from entry into unemployment onwards. As in Germany most of employment spells start at the beginning of a month (and unemployment spells typically last until the end of a month), we construct discrete time spell data in which one month corresponds to one time unit.

In our dataset we define two mutually exclusive labor market states: unemployment and regular employment. Individuals who are either registered as unemployed at the Federal Employment Office (with or without benefit receipt) or participants of programs of the Active Labor Market Policy are defined as being unemployed. During unemployment individuals might take up a mini-job. Periods in which individuals take up marginal employment without having a parallel unemployment spell are not included in our sample and individuals with a mini-job as a secondary job are defined as being regularly employed, i.e. the secondary job is ignored. Regular employment is defined as employment subject to social security contributions.[53] We exclude any periods without information for more than one month which allows us to attribute a spell to unemployment or employment and treat the corresponding spells

[53]To exclude low-income jobs, we determine a minimum income of 600€/month and corresponding employment spells with an income below that threshold are right censored.

as right-censored.[54] This might be due to self-employment, employment as a civil servant, or not being available to the labor market. A further reason might be that individuals de-register as unemployment benefits elapse or are too low (compared to the administrative burden) yet still continue looking for a job. As our sample consists of prime-age men only, it is likely that individuals who are neither self-employed nor civil servants continue seeking a job independent of being registered as unemployed. Therefore, we examine the sensitivity of our results to this aspect in Section 3.5.3 and redefine uncovered periods as unemployment. This largely leads to longer unemployment spells and more individuals who take up a mini-job during our observation period.

3.3.2 Descriptive Statistics of Transition Processes

Table 3.1 provides the number of spells per individual spent in unemployment, in unemployment with a transition to a mini-job, and employment within our observation window.[55]

Table 3.1: Spells per Person

Number of Spells	Unemployment	Mini-Job (while being UE)	Employment
0	–	8,493	2,919
1	5,516	1,337	3,931
2	2,415	137	1,595
3	1,574	33[a)]	1,362
4	413	⋆	149
≥5	82	⋆	44

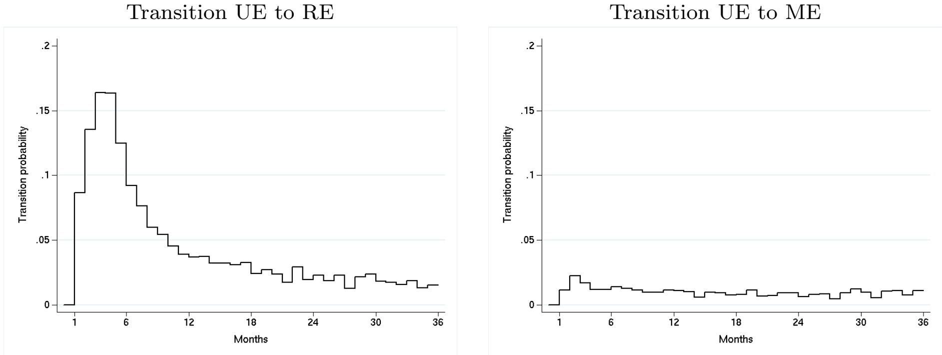
Note: Depicted are the number of spells per person. For instance, 5,516 individuals have only one single unemployment spell while 82 individuals have five or more. Each column sums up to the total number of individuals (N=10,000).
a) Contains the number of individuals with three or more mini-job spells.
⋆ To secure data anonymity cells with less than 20 observations are not shown.

Due to the construction of our sample (inflows into unemployment) every individual has at least one unemployment spell. Almost half of all individuals have repeated unemployment spells and only a minority have five or more spells; in fact, 5,516 individuals have only a single unemployment spell, while 82 individuals have five or more. Around 8,500 individuals never take up a mini-job during unemployment, and for around 2,900 individuals we do not observe a transition to regular employment.

[54] In our sample 29.7% of individuals face right censored spells due to missing information.

[55] A spell is defined as a continuous period of time within the same state without an interruption, i.e., no transitions to other states.

Figure 3.1: Hazard Functions



Note: Depicted are unconditional transitions probabilities. UE - Unemployment, ME - Marginal employment, RE - Regular employment.

Figure 3.1 depicts the hazard rates for the transition from unemployment to regular employment, and the take-up rate of mini-jobs during unemployment. The probability of leaving unemployment for a regular job is first increasing and – after around five months – decreases with the elapsed unemployment duration. Compared to the transition from unemployment to employment the probability of entering a mini-job is rather low, and does not vary strongly according to the elapsed unemployment duration.

3.3.3 Transitions to ALMP

Starting from unemployment entry, different strategies exist to increase the outflow probability from unemployment. Besides a decreasing profile of benefit payments, one main strategy consist of assigning unemployed individuals to programs of active labor market policies, e.g., training programs, wage subsidies, public employment measures, job search assistance and monitoring schemes. In addition to those measures, unemployed individuals in Germany face one additional feature: marginal employment. To assess the meaning of the different strategies for the unemployed, we follow individuals from unemployment entry onwards and consider the first transition to programs of ALMP or mini-jobs. Table 3.10 shows respective shares within the estimation sample. Overall, it is visible that marginal employment is as important for unemployed individuals as programs of ALMP (see upper panel of Table 3.10). For instance, 7.2% take up a mini-job while 4.9% and 9.3% are assigned to vocational and short-term training measures.

Table 3.10 further shows in the lower panel that individuals with a mini-job face a higher probability to participate in ALMP compared to unemployed individuals without a mini-job. Although higher participation might be explained by longer unemployment spells among treated individuals and the negative selection into mini-jobs, i.e., unemployed individuals who take up a mini-job are on average lower educated and located in regions with poor labor market conditions (see Table 3.2 below), the positive correlation between having a mini-job and entering ALMP suggests that the effectiveness of marginal employment and programs of ALMP might interact.

To shed light on this issue, it is first of all required to know if marginal employment indeed has a significant impact on labor market outcomes. In this chapter, we address this question and evaluate the impact of marginal employment on unemployment duration and subsequent job quality. Based on this evidence, future research should then shed some light on the interaction of marginal employment and measures like job search and training programs for unemployed workers.

3.3.4 Differences in Observable Characteristics

Table 3.2 provides descriptive statistics measured at the initial entry into unemployment in 2001. Results are depicted for the full and estimation sample, and in addition are separated by treatment status, i.e., those who take up a mini-job during the 36 months and those who do not.

First of all, the sample reduction (due to computational reasons) introduces no selection bias as observable characteristics are almost equally distributed between the full and the estimation sample. Of the 10,000 drawn individuals, 1,507 take up a mini-job during unemployment within our observation window. Comparing both subgroups in column three and four suggests that the group of individuals who take up marginal employment are on average lower educated in terms of both schooling and professional training. For example, around 13.5% among the treated individuals have no schooling degree, while this share is only around 9% for the non-treated. More than 40% of the unemployed workers who take up a mini-job do not have any occupational degree. The corresponding share among the comparison group is less than 30%. The sectoral distribution, the mean age and the family status is rather similar between treated and non-treated individuals, while individuals in regions with higher local unemployment rates and lower GDP per capita are more likely

Table 3.2: Descriptive Statistics of Observed Characteristics

	Full sample	Estimation sample All	Having a Mini-Job No	Having a Mini-Job Yes
Number of individuals	24,131	10,000	8,493	1,507
Age (in years)	37.4	37.4	37.5	37.2
	(8.1)	(8.2)	(8.1)	(8.2)
Married	52.9	52.5	52.2	54.2
Children	33.6	33.2	32.9	35.0
Children $\leq$ 10 years	22.0	21.5	21.2	23.2
Non-German	15.7	15.7	14.6	21.6
Severely handicapped	2.0	2.1	2.2	1.5
Health restrictions	12.5	12.4	12.0	14.8
School leaving certificate				
No degree	9.4	9.5	8.8	13.5
Lower secondary school	59.5	59.6	58.9	63.7
Middle secondary school	15.5	15.7	15.7	15.3
(Specialized) Upper secondary school	15.6	15.2	16.6	7.5
Professional training				
Unskilled	30.3	30.1	28.1	41.5
Apprenticeship or technical college degree	63.2	63.6	65.0	55.6
University degree	6.5	6.3	6.9	2.9
Sector of last job				
Construction	25.4	25.8	26.3	23.1
Production	21.7	21.3	21.3	21.3
Wholesale/Retail	13.1	13.0	13.0	12.9
Private sector services	26.6	26.6	25.9	30.8
Others (public sector, agriculture)	13.2	13.3	13.5	11.9
Local macroeconomic conditions				
Unemployment rate (in %)	7.6	7.6	7.5	8.0
	(2.4)	(2.4)	(2.4)	(2.3)
Real GDP per capita[a)] (in thousand €)	28.6	28.6	28.7	28.2
	(11.5)	(11.5)	(11.7)	(10.6)

Note: All statistics are percentages (if not differently indicated) and measured at entry into unemployment; standard deviations in parenthesis.
[a)] Normalized to prices in 2005.

to enter a mini-job during unemployment. Local unemployment is measured on a quarterly basis, while the local GDP per capita is measured on a yearly basis.[56]

3.3.5 Characteristics of Mini-job Spells

To establish the extent mini-jobs serve as a stepping stone it is important to have more information on the mini-jobs themselves.[57]. In our data we have information about the sector in which individuals have regular jobs and mini-jobs. Table 3.3 displays the sectoral distribution of mini-jobs in our sample. Mini-jobs are primarily provided by the service and the construction sectors and this is similar among skilled and unskilled workers, although the share of unskilled workers taking up a mini-job

[56]Both the unemployment rate and the GDP are measured on an employment agency district level. In total, there are 178 employment agency districts in Germany.

[57]Mini-jobs in our sample have a mean (median) duration of 4.7 (3) months

in the service sector is larger (50.4%) than the corresponding share among skilled individuals (41.4%).

Table 3.3: Sectoral Distribution of Mini-Jobs

	All	Professional training background Unskilled	Skilled
Number of spells	1,713	698	1,015
Construction	21.3	21.3	21.3
Production	8.3	7.3	9.1
Wholesale/Retail	14.0	11.6	15.7
Private sector services	45.1	50.4	41.4
Others (public sector, agriculture)	11.3	9.3	12.6

Note: All statistics are percentages (if not differently indicated). Individuals who have no professional degree at entry into unemployment are categorized as "unskilled" and as "skilled" otherwise.

More interestingly, Table 3.4 and 3.5 depict a sectoral comparison of the mini-job with the previous and subsequent regular job, respectively. For instance, Table 3.4 shows that among all unemployed who take up a mini-job and previously worked in the construction sector, 75.2% have a mini-job in the same sector while the rest are marginally employed in a different sector. We observe two patterns in Table 3.4. First, we see that many individuals take up a mini-job in the same sector in which they worked before entering unemployment. Second, if workers change the sector, they usually take up mini-jobs in the service sector. Table 3.5 suggests a strong correlation between sectors for the mini-job and the subsequent regular job. For example, 82.4% of the individuals with a mini-job in the construction sector and for whom we observe a transition into a regular job find employment in the construction sector. These numbers indicate that the mini-jobs are related to the sectoral experience and skills of the unemployed workers, which suggests that they might be relevant for the job-finding probability, for example by lowering human capital deterioration, as a screening device for potential employers or by increasing the probability of getting job offers due to network effects.

Further to the finding that unemployed with a mini-job are likely to find regular employment in the same sector, in Table 3.6 we present the shares of treated individuals who find a regular job in the same firm in which they have been marginally employed. In the upper panel we consider all transitions to regular employment with a mini-job at any time before. In the lower panel we only take into account spells in which the unemployed worker was still marginally employed in the month of the exit from unemployment to employment, i.e. the individual has not left the mini-job before finding a new regular job. A large share of marginal employed individuals find a regular job within the same firm (45%), which suggests that mini-jobs

Table 3.4: Sectoral Transition Matrix: From Previous Job to Mini-Job

Sector of previous job	Sector of mini-job				
	Constr.	Prod.	Retail	Services	Others
Construction	75.2	⋆	⋆	13.7	⋆
Production	18.3	28.1	17.0	31.4	⋆
Wholesale/Retail	⋆	⋆	35.6	42.3	⋆
Private sector services	⋆	⋆	9.8	76.1	⋆
Others (public sector, agriculture)	⋆	⋆	⋆	25.3	54.5

Note: Depicted is the sectoral distribution of mini-jobs during unemployment conditional on the sector of the previous jobs; all statistics are in percentages (if not differently indicated). In total, we observe 911 mini-jobs. For instance, among all treated individuals who previously worked in the construction sector, 75.2% also take up a mini-job in the same sector.
⋆ To secure data anonymity cells with less than 20 observations are not shown.

Table 3.5: Sectoral Transition Matrix: From Mini-Job to Subsequent Job

Sector of mini-job	Sector of subsequent job				
	Constr.	Prod.	Retail	Services	Others
Construction	82.4	8.0	⋆	⋆	⋆
Production	⋆	62.8	⋆	⋆	⋆
Wholesale/Retail	⋆	⋆	42.5	34.9	⋆
Private sector services	8.1	9.4	8.4	68.7	5.4
Others (public sector, agriculture)	⋆	⋆	⋆	26.3	58.9

Note: Depicted is the sectoral distribution of subsequent jobs conditional on the sector of the mini-job during unemployment; all statistics are in percentages (if not differently indicated). In total, we observe 911 transitions. For instance, out of all unemployed individuals who have a mini-job in the construction sector, 82.4% also find regular employment in the same sector.
⋆ To secure data anonymity cells with less than 20 observations are not shown.

are in some cases utilized as a probation period. The share of transitions within the same firm is with 52% higher in the first 12 months of unemployment than the corresponding share after one year of unemployment (30.3%). Within the group of individuals who are holding a mini-job in the month that they find a new job, the corresponding shares are slightly higher (60.0% and 39.3%, respectively).

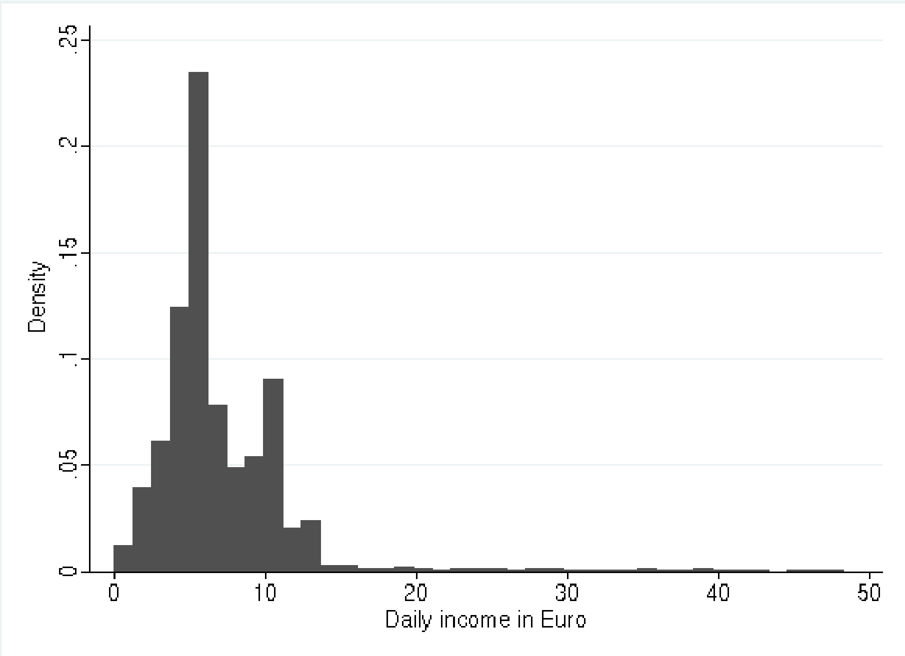
Table 3.6: Transition from UE (with Mini-Job) to RE within Same Firm

	All	Timing of transition to employment	
		$\leq$ 12 months	$>$ 12 months
All transition to RE	911	617	294
Within same firm (in %)	45.0	52.0	30.3
Direct transition to RE	484	395	89
Within same firm (in %)	56.2	60.0	39.3

Note: Depicted is the share of treated transitions from unemployment to employment which take place within the same firm, i.e., the mini-job during unemployment and the subsequent regular job are within the same firm. UE - Unemployment, RE - Regular employment.

As discussed in the section on the institutional background, unemployed workers are allowed to earn up to 165€/month without suffering a reduction in transfer payments. This implies that the average treated individual in our sample can increase his income during unemployment by around 23%. Figure 3.2 depicts the income distribution of mini-jobs during unemployment and it can be seen that indeed 50% earn 164€/month or less. However, there is still a large fraction of job seekers who earn more than threshold amount. These higher earnings might be explained by labor demand side restriction, i.e., the offered jobs do not always have the exact number of working hours which would result in 165€/month. This supports the idea that there exist search frictions in the segment of the mini-jobs.

Figure 3.2: Income Distribution of Mini-Jobs During Unemployment



Note: The mean and median of the income distribution amount to 6.9 and 5.4 Euro respectively; whereby these daily incomes correspond to 207 and 164 Euro per months.

3.4 Empirical Model

We are interested in the causal impact of taking up a mini-job on two outcomes, the unemployment duration and the job match quality. Individuals are defined to be treated if they enter a mini-job in month t of the unemployment spell from the corresponding month t onwards. This implies that individuals who have a mini-job for some time during unemployment and leave this marginal employment before they find a regular job are still defined to be "treated".

In this section we start with the presentation of a bivariate duration model for the duration until leaving unemployment for a job and the duration until the treatment, which is entering marginal employment, following the "timing of events" approach (Abbring and van den Berg, 2003).[58] In a next step we extend this by incorporating the job match quality similar to van den Berg and Vikström (2009).

As depicted in Table 3.1, our dataset contains multiple observations for some individuals, which facilitates the identification and estimation of the joint distribution of the unobserved heterogeneity variables (see e.g. Honore, 1993). Moreover, our dataset includes time-varying variables such as the local unemployment rate. Eberwein et al. (1997) and Gaure et al. (2007) emphasize that time-varying covariates provide exclusion restrictions because past values affect current transition probabilities only through the selection process, i.e. time-varying covariates provide a more robust source of identification than time-invariant covariates. These features of the dataset imply that identification does not solely rely on the functional form of the model.

3.4.1 Durations Until Employment and Until Treatment

As we observe an inflow sample into unemployment, we do not have to take the initial condition problem into account (Heckman, 1981), because every individual is initially unemployed. We observe labour market states in discrete time and assume that all individual differences in the probability of leaving unemployment for a job in period t can be characterized by observed characteristics x, unobserved characteristics V_u, and a treatment effect if a mini-job has been taken up before or at the discrete period t. Similarly, we assume that all individual differences in the probability of being treated in period t can be characterized by observable characteristics x and unobserved characteristics V_m. Given these assumptions the probability of leaving unemployment for a job $\theta_u(t)$ and the probability of taking up marginal employment $\theta_m(t)$ can be expressed by complementary log log specifications:

$$\theta_u(t|x, V_u, t_m) = 1 - \exp(-\exp(\lambda_{tu} + x_t'\beta_u + I(t \geq t_m)\delta_u + V_u)) \tag{3.1}$$

$$\theta_m(t|x, V_m) = 1 - \exp(-\exp(\lambda_{tm} + x_t'\beta_m + V_m)) \tag{3.2}$$

[58] We estimate a discrete time duration model. Abbring and van den Berg (2003) provide a proof for continuous time models. For identification in dynamic discrete models see Heckman and Navarro (2007).

$I(\cdot)$ takes on the value one if $t \geq t_m$ and δ_u is the effect of being treated on the probability of finding a job. We assume that the treatment does not affect the probability of leaving unemployment for a job before the moment of accepting the job. This assumption is referred to as the no-anticipation assumption and is very likely to hold in our application. The unemployed workers have to search for a mini-job and – similar to the transition to a regular job – the job-finding probability depends on the job offer arrival rate and the probability that the job characteristics are acceptable. This implies that the job finding process is stochastic. We assume that the unemployed workers do not know the exact timing of the treatment. However, they are allowed to know the probability distribution of future events conditional on observable and unobservable characteristics. Moreover, we assume that the unobserved heterogeneity components V_u and V_m are constant over time, i.e. across repeated spells of unemployed individuals, and that V_u and V_m are uncorrelated with observed characteristics x.

3.4.2 Post-Unemployment Outcomes

We measure the job match quality by the monthly wage and the probability of reentering unemployment. We allow both outcomes to depend on unobserved characteristics which might be correlated with the unobserved factors V_u and V_m. In order to identify the causal impact of mini-jobs on realized wages, we assume that the unobserved heterogeneity and the causal effect have an additive impact on the mean log wage. We specify the following equation for the wage at the beginning of the new employment spell:

$$\ln w = x_t'\beta_w + I(t_m \leq t_u)\delta_w + t_u\eta_w + V_w + \varepsilon_w \tag{3.3}$$

The treatment effect is given by δ_w, V_w is the unobserved heterogeneity which is assumed to be constant across repeated spells, and ε_w is assumed to be normally distributed with mean zero and unknown variance σ_w. In addition, we allow the log wage to vary with respect to the previous unemployment duration t_u.

Similarly to the duration of unemployment we specify a duration of employment, described by the probability of leaving employment and reentering unemployment in period t. We assume that all individual differences in the probability of reentering unemployment in t can be characterized by observed characteristics x, unobserved characteristics V_e and a treatment effect δ_e if a mini-job has been taken up in the previous unemployment spell. The probability of leaving employment in period t is given by:

$$\theta_e(t|x, V_e, t_u, t_m) = 1 - \exp(-\exp(\lambda_{te} + x_t'\beta_e + I(t_m \leq t_u)\delta_e + t_u\eta_e + V_e)) \qquad (3.4)$$

Similarly to the wage equation we allow θ_e to vary with respect to the previous unemployment duration t_u. In the empirical specification we include a linear and a quadratic term reflecting the previous unemployment duration in a flexible way. V_e is constant over time and uncorrelated with observed characteristics x. However, V_e and V_w might be correlated with the treatment indicator and the previous unemployment duration, which captures the dynamic selection into job matches.

3.4.3 Distribution of Unobserved Heterogeneity

We specify the distribution of unobserved heterogeneity G to have a discrete support with P support points. In order to ensure that the corresponding probabilities are between zero and one and to sum to one, we use a multinomial logit parameterization of the class probabilities:

$$\pi_p = \frac{exp(\omega_p)}{\sum_{p=1}^{P} exp(\omega_p)}, \quad p = 1, ..., P, \quad \omega_1 = 0 \qquad (3.5)$$

Each of the six components of the unobserved heterogeneity V takes on a specific value at support point p, whereby for identification reasons the values are set to be zero for $p = 1$. This implies that for a model with $P = 2$ G would be described by 5 parameters, for $P = 3$ we estimate 10 parameters, etc.[59] This approach allows for a flexible covariance matrix for the unobserved components. For a similar model for unobserved heterogeneity in the context of timing of events models see Crepon et al. (2010) and in the context of random coefficient models in the statistical literature see e.g. Aitkin (1999). Gaure et al. (2007) provide Monte Carlo evidence that modeling selection based on unobservables by a flexible discrete distribution works well in the context of timing of events models. In the estimation we increase the number of support points until the model fit cannot be further improved by an additional support point, evaluated on the basis of the Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC).

[59] For $P = 2$ we estimate the parameters V_{u2}, V_{m2}, V_{e2}, V_{w2}, ω_2. For $P = 3$ we estimate V_{u2}, V_{m2}, V_{e2}, V_{w2}, ω_2 and V_{u3}, V_{m3}, V_{e3}, V_{w3}, ω_3.

3.4.4 Likelihood Function

Given this setup, the likelihood contribution of an individual i with one sequence s, i.e., one unemployment spell of length t_u and one employment spell of length t_e, for given unobserved and observed characteristics V and x is given by:

$$
\begin{aligned}
L_{is}(x,V) \quad = \quad & \prod_{t=1}^{t_m} \Big[1-\theta_m(t|x,V_m)\Big] \left(\frac{\theta_m(t_m|x,V_m)}{1-\theta_m(t_m|x,V_m)}\right)^{\kappa_m} \\
& \prod_{t=1}^{t_u} \Big[1-\theta_u(t|x,V_u,t_m)\Big] \left(\frac{\theta_u(t_u|x,V_u,t_m)}{1-\theta_u(t_u|x,V_u,t_m)}\right)^{\kappa_u} \\
& \prod_{t=t_u+1}^{t_u+t_e} \Big[1-\theta_e(t|x,V_e,t_u,t_m)\Big]^{\kappa_u} \left(\frac{\theta_e(t_e|x,V_e,t_u,t_m)}{1-\theta_e(t_e|x,V_e,t_u,t_m)}\right)^{\kappa_u\kappa_e} \\
& \left(\frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(-\frac{(\ln w_i - \widehat{\ln w_i})^2}{2\sigma^2}\right)\right)^{\kappa_u} \qquad (3.6)
\end{aligned}
$$

The indicators κ_m, κ_u and κ_e take on the value one if a transition to a mini-job, to regular employment or to unemployment, respectively, is observed and zero otherwise. $\ln w_i$ is the logarithm of the observed wage in our data – in case we observe a transition from unemployment to a regular job – and $\widehat{\ln w_i}$ corresponds to the predicted value based on the coefficients β_w. We observe multiple spells for some individuals in our dataset. Therefore, the likelihood contribution of an individual corresponds to the product of the likelihood contributions of S sequences of unemployment and employment spells.

$$
L_i(x,V) \quad = \quad \prod_{s=1}^{S} L_{is}(x,V)
$$

Since we do not know the unobserved characteristics for an individual i, the "unconditional" log-likelihood contribution corresponds to the weighted sum of the contributions corresponding to the P points of support. The log-Likelihood function for the sample with N individuals is given by:

$$
\ln L = \sum_{i=1}^{N} \ln \sum_{p=1}^{P} \pi_p L_i(x, V(p)) \qquad (3.7)
$$

3.5 Results

We estimate the duration until finding a mini-job, the duration of unemployment, the duration of employment and the reemployment wage with jointly distributed unobserved heterogeneity. We estimate different empirical specifications of this model. Starting with a baseline model which allows for homogeneous treatment effects, in a second step we introduce effect heterogeneity with respect to selected observable characteristics. In a third step we estimate interaction effects of the treatment indicator with elapsed unemployment duration. Finally, we reestimate our model on a sample in which we re-define uncovered periods in the data as unemployment in order to test whether our results are robust with respect to this alternative specification of employment states.

3.5.1 Baseline Results

In Table 3.7 we report the treatment effects on different outcomes. We control for observable characteristics as reported in Table 3.2 and allow for flexible duration dependencies for the duration in unemployment, the duration until treatment and the employment duration. Moreover, we control for the quarter in which the corresponding spell starts and include time-varying dummy indicators for the current quarter to capture seasonal effects.[60] Our final specification includes 9 mass points (P=9), i.e. we estimate 40 additional parameters for the distribution of unobserved characteristics compared to a model without unobserved heterogeneity. A further increase of the mass points does not lead to a better model fit, evaluated on the basis of the AIC and the BIC. The coefficients of the preferred model with unobserved heterogeneity are reported in the columns (2) for the unemployment duration, in column (4) for the employment duration and in column (6) for the wages in Table 3.7. Columns (1), (3) and (5) refer to a model without controlling for selection based on unobserved characteristics.

Commencing with column (1), we report the coefficient of the time-varying treatment dummy for the probability of leaving unemployment for a regular job. The parameter is positive and significantly different from zero. Once we control for unobserved heterogeneity in column (2), the treatment effect clearly decreases and is no longer significantly different from zero. This suggests that mini-jobs are neither

[60] The complete set of coefficients including the distribution of the unobserved heterogeneity is reported in the Appendix in Table 3.11.

stepping-stones to regular jobs, nor do they lead to longer spells of unemployment.

Table 3.7: Baseline Estimation Results

	Transition UE to RE		Transition RE to UE		Linear wage equation	
	(1)	(2)	(3)	(4)	(5)	(6)
Mini-Job	0.129***	0.030	0.007	−0.277***	−0.074***	−0.002
	(0.037)	(0.049)	(0.045)	(0.065)	(0.010)	(0.009)
Unobs. Het. (P=9)	No	Yes	No	Yes	No	Yes

Note: Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation also includes control variables for duration dependence, seasonal dummies, individual socio-demographics, information on last job and local macroeconomic conditions. Table 3.11 in the appendix provides the full set of estimated coefficients. UE - Unemployment, RE - Regular employment.

Column (3) shows that we do not find any effect of the mini-job dummy on employment stability in a model without unobserved heterogeneity. However, once we control for selection, the estimated parameter suggests that treated individuals re-enter unemployment with a lower probability than individuals who have not been treated (column 4). Moreover, these individuals have nearly the same wages compared to the non-treated individuals when they take up a regular job (column 6). In the "naive" model without controlling for dynamic selection based on unobserved characteristics we estimate a significantly negative impact of mini-jobs on wages (column 5). These results underline the importance of controlling for dynamic selection. The correlations between the different components of unobserved heterogeneity are all statistically significant (see Table 3.11).

Overall, the baseline model suggests that mini-jobs are not increasing the outflow probability from unemployment and do not lead to higher paid jobs, but the treated individuals end up in more stable employment spells.

3.5.2 Heterogenous Treatment Effects

To investigate effect heterogeneity we interact the treatment dummy with selected observable characteristics. These characteristics include individuals' age, dummy variables for being unskilled and for having worked in the construction sector in the last regular job, and the local unemployment rate. Additionally, we include a dummy variable indicating whether or not the mini-job is in the same sector as the previous regular job. We distinguish between five sectors: construction, production, wholesale/retail, private sector services, and others. We particularly investigate the treatment effect for the construction sector, because this sector is characterized by strong seasonal employment patterns which might imply a specific role of mini-jobs for periods of unemployment. To allow for non-linear effects, we include the logarithm of age. The estimation results are reported in Table 3.8. The reference person is an individual of mean age located in a region with the mean local unemployment rate, not working in the construction sector, not being unskilled and having a mini-job in a different sector than the previous job. The coefficient of the treatment dummy reflects the treatment for this reference person, and the coefficients of the interaction terms capture the heterogenous effects for example with respect to the local unemployment rate or age.

Table 3.8: Treatment Effect Heterogeneity with respect to Observed Characteristics

	Transition UE to RE (1)	Transition RE to UE (2)	Linear wage equation (3)
Mini-Job	-0.088	-0.398^{***}	-0.027^{**}
	(0.076)	(0.101)	(0.013)
Mini-Job × Ln(Age)	-0.149	-0.031	0.038
	(0.187)	(0.248)	(0.036)
Mini-Job × Unskilled	0.016	-0.016	0.037^{***}
	(0.081)	(0.102)	(0.014)
Mini-Job × Construction	-0.009	0.163	0.035^{**}
	(0.085)	(0.120)	(0.015)
Mini-Job × Local UE-Rate	0.013	-0.024	-0.007^{**}
	(0.016)	(0.020)	(0.003)
Mini-Job × Same Sector	0.172^{***}	0.127	0.000
	(0.078)	(0.100)	(0.014)
Unobs. Het. (P=9)	Yes	Yes	Yes

Note: Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation also includes control variables for duration dependence, seasonal dummies, individual socio-demographics, information on last job and local macroeconomic conditions. Individuals who have no professional degree at entry into unemployment are categorized as "unskilled". "Same sector" indicates that the mini-job was taken up within the same sector (construction, production, wholesale/retail, private sector services, others) as the last regular job. The reference person is an individual of mean age located in a region with the mean local unemployment rate, not working in the construction sector and not being unskilled. UE - Unemployment, RE - Regular employment.

We do not find evidence for effect heterogeneity for the transition probability from unemployment to regular employment with respect to age, skill level, whether or not the unemployed has worked in the construction sector before entering unemployment, and the local unemployment rate. However, we find a significantly positive impact on the transition probability to regular employment, if the mini-job is in the same sector as the previous job. By contrast, having a mini-job in a different sector increases the income during unemployment, yet does not increase the probability of receiving an acceptable job offer. Potential positive effects of taking up a mini-job during unemployment, for example by lowering human capital deterioration, as a screening device for potential employers or by increasing the job offer arrival rate due to network effects, seem to only occur if the marginal employment is related to sectoral experience and skills of the unemployed workers. This is in line with the descriptive evidence presented above, which shows that the sector of the mini-job positively correlates with both the sector of the previous job and the sector of the post-unemployment job. For the duration of employment we do not find any evidence for effect heterogeneity.

For the impact on the initial wage in a new job we find some evidence for effect heterogeneity. While the reference person – a skilled worker not working in the construction sector – takes up jobs with lower wages, this is neither the case for unskilled workers nor for workers in the construction sector. Moreover, the treated individuals enter higher paid jobs when the local unemployment rate is lower. These results indicate that good labor market conditions allow the treated unemployed workers to be more selective with respect to wages and job stability, while otherwise they end up in more stable jobs only. One reason for the difference with respect to the skill-level might be that mini-jobs are seen as a rather negative signal for skilled workers while this is not the case for unskilled individuals. A mini-job in the same sector as the previous job does not have an impact on the post-unemployment wage.

In Table 3.9 we report the coefficients of the interaction effects of the treatment indicator with elapsed unemployment duration, allowing for different treatment effects in months 1-6, 7-12, 13-24 and 25-36. The results suggest a significantly positive effect of entering a mini-job after one year of unemployment, while we do not observe any significant impact on the probability of finding a job for the first 12 months (column 1). The effect in months 25-36 is positive but not statistically significant. However, the number of observations is decreasing over time and these estimates are based on a small number of unemployed individuals. These results suggest that there exist stepping stone effects of mini-jobs to regular jobs, but that these effects are only relevant for long-term unemployed workers.

Table 3.9: Treatment Effect Heterogeneity with respect to Elapsed Unemployment Duration

	Transition UE to RE (1)	Transition RE to UE (2)	Linear wage equation (3)
Mini-Job	−0.096	−0.153*	−0.011
	(0.066)	(0.085)	(0.012)
Mini-Job × 7-12 months	0.125	−0.133	0.027
	(0.096)	(0.119)	(0.017)
Mini-Job × 13-24 months	0.405***	−0.425***	0.024
	(0.101)	(0.136)	(0.019)
Mini-Job × 25-36 months	0.168	0.153	−0.007
	(0.141)	(0.270)	(0.032)
Unobs. Het. (P=9)	Yes	Yes	Yes

Note: Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation also includes control variables for duration dependence, seasonal dummies, individual socio-demographics, information on last job and local macroeconomic conditions. UE - Unemployment, RE - Regular employment.

For employment stability we find a negative effect of having a mini-job on the probability of re-entering unemployment for regular jobs which are found during months 1-6 of the unemployment spell (column 2). These effects are stronger if the jobs are taken up after 12 months in the unemployment spell. In contrast to this, we do not find evidence for time-varying treatment effects on initial wages. None of the estimated coefficients are significantly different from zero (see column 3).

One important determinant of the probability of leaving unemployment for a job – the receipt of unemployment benefits – depends on the elapsed unemployment duration. The maximum duration of benefit receipt depends on the time spent in regular employment in the preceding years and the age at entry into unemployment, and after benefit exhaustion unemployed job-seekers are eligible for means-tested unemployment assistance (see Section 3.2 for details). Due to the reduced replacement rate for unemployment assistance the income during unemployment decreases over time. However, the rules for additional earnings from marginal employment do not change, and the decrease in income is rather small. This suggests that the exhaustion of benefits cannot explain the strong evidence for time-varying treatment effects of taking up marginal employment.

Given our descriptive evidence on a decreasing share of transitions within the same firm after twelve months of unemployment, the positive impact on employment stability is probably not driven by an increasing role of mini-jobs as a probation period. The results suggest that the positive effects of entering marginal employment – which might occur due to signaling effects, network effects, or the reduced deterioration of human capital – seem to lead to both an increase of the job-finding probability and the employment stability. These effects seem to be less relevant at the beginning of an unemployment spell, which is plausible given that the contact frequency with former colleagues (network) and the deterioration of human capital are probably time-dependent.

3.5.3 Sensitivity Analysis

We have estimated the model based on an alternative definition of unemployment. In contrast to our preferred specification, here we additionally define periods of our sample members which are not covered within the data as unemployment. This leads to longer unemployment spells and a higher number of treated individuals. Overall, we find very similar results for this alternative definition of unemployment (see Tables 3.12 - 3.14 in the Appendix).

In the baseline model the effect of entering a mini-job on the job-finding probability is significantly positive at the 5%-level, which is probably driven by an increasing number of observations with longer unemployment durations. In line with this, we find stronger evidence for positive interaction effects of the treatment indicator with elapsed unemployment duration, see Table 3.14. In this specification the treatment effect is significantly positive for unemployment durations between 25-36 months. Similar to the main specification, we only find evidence for effect heterogeneity for the unemployment duration with respect to the sector of the mini-job. We only observe an increased transition probability into regular employment if the mini-job is in the same sector as the previous regular job, whereby this effect is significant at the 10% level. Again, we do not find any evidence for effect heterogeneity with respect to employment stability. For the initial wages the effect for the construction sector is no longer significantly different from zero, while the significant effects for the skill level and the local unemployment rate are stable. Although we observe more transitions into regular employment especially for longer unemployment spells, again we do not find any evidence for different effects depending on the elapsed unemployment duration on initial wages.

We observe that individuals who take up marginal employment during unemployment have a higher probability of entering other measures of ALMP than unemployed individuals who do not enter a mini-job (see Section 3.3.3). In order to test whether our results are driven by the participation in other programs, we have re-estimated our model including time-varying indicators for the participation in ALMP. Our results do not change, which indicates that the impact of an increased participation in other ALMP measure cannot explain our results.

3.6 Conclusion

In some countries such as Germany and Austria unemployed workers are allowed to work for some hours during job search by taking up "marginal employment". Marginal employment is defined as employment below an income threshold with reduced social security contributions and job seekers can increase their income during unemployment up to a threshold without any benefit deduction. For unemployed individuals, income from marginal employment is fully exempted from pay-roll taxes, and for employers it is only subject to reduced pay-roll taxes. Although marginal employment does not legally belong within active labor market policy programs in Germany, it is comparable from an economic perspective to a wage subsidy. We analyze the causal impact of entering marginal employment on unemployment duration and job match quality of unemployed individuals and investigate potential effect heterogeneity with respect to observed characteristics and elapsed unemployment duration.

Based on a random inflow sample into unemployment of male workers in West Germany, our results suggest that the treatment effects vary according to the time spent in unemployment. While we do not find any significant impact for the first 12 months of unemployment, job-finding probabilities clearly increase after one year, and the impact on job stability is stronger for individuals who are unemployed for longer. We find a significantly positive impact on the transition probability to regular employment if the mini-job is in the same sector as the previous job. With regards to wages, we do not find any evidence for an interaction effect with elapsed unemployment duration. However, the impact on wages seems to vary with the skill level and the sector. Skilled individuals have a lower wage in the initial job after leaving unemployment if they had a mini-job. Moreover, the results indicate that the wage effects are increasing if the economic situation within the local labor market is more favorable.

3.7 Appendix

Table 3.10: First Transition to ALMP or Mini-Job During First/Initial Unemployment Spell Within the Estimation Sample

	All
Number of individuals	10,000
First transition to programs of ALMP	
Job creation Schemes	0.6
Wage subsidy	3.4
Promotion of start-ups	4.9
Vocational training	4.9
Short-term training	9.3
Other programs	1.6
First transition to mini-job	7.2

	Having a Mini-Job during first UE spell	
	No	Yes
Number of individuals	9,066	934
First transition to programs of ALMP		
Job creation Schemes	0.6	⋆
Wage subsidy	3.4	6.9
Promotion of start-ups	5.2	4.7
Vocational training	4.9	9.7
Short-term training	9.2	20.3
Other programs	1.6	2.5

Note: Only the first unemployment spell is considered which explains the divergent number of observations compared to Table 3.1 and 3.2. All statistics are percentages (if not differently indicated). For instance, 3.4% received a wage subsidy as the first treatment while 7.2% entered a mini-job. UE - Unemployment, ALMP - Active labor market policy.
⋆ To secure data anonymity cells with less than 20 observations are not shown.

Table 3.11: Estimation Results

	Without unobs. het.		P=9	
Log-likelihood	77,145		74,694	
Equation 1: Transition from unemployment to employment				
Constant	−2.026***	(0.034)	−1.565***	(0.050)
Timing of transition (Ref. 1-2 months)				
3-4 months	0.400***	(0.022)	0.494***	(0.025)
5-6 months	−0.002	(0.029)	0.179***	(0.032)
7-8 months	−0.442***	(0.038)	−0.209***	(0.042)
9-10 months	−0.691***	(0.048)	−0.430***	(0.051)
11-12 months	−0.895***	(0.057)	−0.603***	(0.061)
13-18 months	−1.105***	(0.044)	−0.744***	(0.050)
19-36 months	−1.502***	(0.043)	−1.036***	(0.055)
Quarter of transition (Ref. 1st quarter)				
2nd quarter	0.107***	(0.023)	0.174***	(0.024)
3rd quarter	−0.101***	(0.029)	−0.028	(0.030)
4th quarter	−0.724***	(0.031)	−0.671***	(0.032)
Quarter of entry into unemployment (Ref. 1st quarter)				
2nd quarter	−0.395***	(0.030)	−0.403***	(0.034)
3rd quarter	−0.266***	(0.029)	−0.280***	(0.032)
4th quarter	−0.125***	(0.022)	−0.145***	(0.026)
Ln(Age) (Ref.: mean age)	−0.480***	(0.044)	−0.574***	(0.054)
Married	0.098***	(0.022)	0.109***	(0.026)
Children	0.100***	(0.029)	0.102***	(0.035)
Children $\leq$ 10 years	−0.031	(0.031)	−0.025	(0.037)
Non-German	−0.192***	(0.027)	−0.250***	(0.033)
Severely handicapped	−0.283***	(0.078)	−0.303***	(0.090)
Health restrictions	−0.370***	(0.031)	−0.422***	(0.037)
School leaving certificate (Ref. No degree or lower secondary school)				
Middle secondary school	−0.116***	(0.025)	−0.138***	(0.030)
(Specialized) Upper secondary school	−0.116***	(0.034)	−0.178***	(0.041)
Professional training (Ref. Unskilled)				
Apprenticeship or technical college	0.177***	(0.020)	0.193***	(0.025)
University degree	0.050	(0.055)	0.111*	(0.065)
Sector of last job (Ref. Others (public sector, agriculture))				
Construction	0.215***	(0.027)	0.189***	(0.034)
Production	−0.152***	(0.030)	−0.201***	(0.036)
Wholesale/Retail	−0.160***	(0.034)	−0.192***	(0.041)
Private sector services	−0.114***	(0.028)	−0.167***	(0.035)
Local macroeconomic conditions				
Unemployment rate (Ref.: mean rate)	−0.059***	(0.004)	−0.073***	(0.004)
Real GDP per capita (Ref.: mean GDP)	−0.007***	(0.001)	−0.007***	(0.001)
Mini-Job	0.129***	(0.037)	0.030	(0.049)

To be continued.

Table 3.11 continued.

	Without unobs. het.		P=9	
Equation 2: Transition from unemployment to marginal employment				
Constant	−4.203***	(0.101)	−4.770***	(0.101)
Timing of transition (Ref. 1-2 months)				
3-4 months	−0.172**	(0.071)	−0.049	(0.074)
5-6 months	−0.320***	(0.084)	−0.110	(0.092)
7-8 months	−0.381***	(0.098)	−0.089	(0.106)
9-10 months	−0.610***	(0.116)	−0.264**	(0.126)
11-12 months	−0.457***	(0.118)	−0.068	(0.129)
13-18 months	−0.758***	(0.096)	−0.299***	(0.110)
19-36 months	−0.772***	(0.082)	−0.199*	(0.112)
Quarter of transition (Ref. 1st quarter)				
2nd quarter	0.131*	(0.067)	0.136*	(0.070)
3rd quarter	0.096	(0.072)	0.110	(0.075)
4th quarter	−0.050	(0.067)	−0.060	(0.070)
Quarter of entry into unemployment (Ref. 1st quarter)				
2nd quarter	0.165**	(0.077)	0.105	(0.086)
3rd quarter	0.131*	(0.075)	0.043	(0.084)
4th quarter	0.178***	(0.063)	0.063	(0.072)
Ln(Age) (Ref.: mean age)	−0.208*	(0.117)	−0.135	(0.144)
Married	−0.010	(0.056)	−0.007	(0.070)
Children	0.091	(0.077)	0.135	(0.096)
Children $\leq$ 10 years	0.080	(0.079)	0.076	(0.100)
Non-German	0.160**	(0.064)	0.184**	(0.078)
Severely handicapped	−0.628***	(0.202)	−0.664***	(0.232)
Health restrictions	0.110*	(0.069)	0.128	(0.084)
School leaving certificate (Ref. No degree or lower secondary school)				
Middle secondary school	−0.060	(0.063)	−0.077	(0.080)
(Specialized) Upper secondary school	−0.678***	(0.110)	−0.747***	(0.131)
Professional training (Ref. Unskilled)				
Apprenticeship or technical college	−0.166***	(0.052)	−0.176***	(0.065)
University degree	−0.242	(0.182)	−0.229	(0.207)
Sector of last job (Ref. Others (public sector, agriculture))				
Construction	0.054	(0.079)	0.034	(0.101)
Production	−0.004	(0.083)	−0.013	(0.103)
Wholesale/Retail	0.091	(0.091)	0.083	(0.112)
Private sector services	0.249***	(0.076)	0.221**	(0.094)
Local macroeconomic conditions				
Unemployment rate (Ref.: mean rate)	0.068***	(0.009)	0.071***	(0.011)
Real GDP per capita (Ref.: mean GDP)	−0.005**	(0.002)	−0.004*	(0.003)

To be continued.

Table 3.11 continued.

	Without unobs. het.		P=9	
Equation 3: Transition from employment to unemployment				
Constant	−2.820***	(0.058)	−3.094***	(0.073)
Timing of transition (Ref. 1-2 months)				
3-4 months	−0.189***	(0.043)	−0.077*	(0.045)
5-6 months	−0.234***	(0.043)	−0.048	(0.046)
7-8 months	0.089**	(0.040)	0.316***	(0.045)
9-10 months	0.732***	(0.039)	1.033***	(0.045)
11-12 months	−0.035	(0.054)	0.318***	(0.060)
13-18 months	−0.872***	(0.054)	−0.462***	(0.060)
19-36 months	−0.923***	(0.047)	−0.399***	(0.059)
Quarter of transition (Ref. 1st quarter)				
2nd quarter	−0.667***	(0.043)	−0.692***	(0.044)
3rd quarter	−0.350***	(0.040)	−0.422***	(0.041)
4th quarter	0.900***	(0.031)	0.846***	(0.033)
Quarter of entry into employment (Ref. 1st quarter)				
2nd quarter	0.302***	(0.030)	0.357***	(0.035)
3rd quarter	0.177***	(0.037)	0.212***	(0.042)
4th quarter	0.108***	(0.041)	0.144***	(0.046)
Ln(Age) (Ref.: mean age)	0.231***	(0.056)	0.230***	(0.068)
Married	−0.016	(0.027)	−0.004	(0.033)
Children	−0.051	(0.037)	−0.039	(0.045)
Children $\leq$ 10 years	−0.030	(0.039)	−0.050	(0.047)
Non-German	0.162***	(0.032)	0.173***	(0.039)
Severely handicapped	0.221**	(0.097)	0.218*	(0.111)
Health restrictions	0.004	(0.038)	−0.003	(0.046)
School leaving certificate (Ref. No degree or lower secondary school)				
Middle secondary school	−0.138***	(0.031)	−0.194***	(0.037)
(Specialized) Upper secondary school	−0.386***	(0.045)	−0.455***	(0.054)
Professional training (Ref. Unskilled)				
Apprenticeship or technical college	−0.134***	(0.026)	−0.137***	(0.032)
University degree	−0.476***	(0.076)	−0.498***	(0.088)
Sector of last job (Ref. Others (public sector, agriculture))				
Construction	0.046	(0.035)	0.046	(0.044)
Production	−0.266***	(0.039)	−0.316***	(0.048)
Wholesale/Retail	−0.383***	(0.045)	−0.447***	(0.054)
Private sector services	−0.123***	(0.036)	−0.180***	(0.044)
Local macroeconomic conditions				
Unemployment rate (Ref.: mean rate)	0.046***	(0.004)	0.052***	(0.005)
Real GDP per capita (Ref.: mean GDP)	−0.002*	(0.001)	−0.003**	(0.001)
Duration of previous unemployment				
Level (in months)	0.030***	(0.006)	0.047***	(0.008)
Squared	−0.001***	(0.000)	−0.002***	(0.000)
Mini-Job	0.007	(0.045)	−0.277***	(0.065)

To be continued.

Table 3.11 continued.

	Without unobs. het.		P=9	
Equation 4: Linear wage equation (wage in first month of regular employment)				
Constant	4.140***	(0.008)	4.269***	(0.011)
Quarter of entry into employment (Ref. 1st quarter)				
2nd quarter	−0.024***	(0.007)	−0.009*	(0.006)
3rd quarter	−0.059***	(0.008)	−0.027***	(0.006)
4th quarter	−0.086***	(0.008)	−0.054***	(0.006)
Ln(Age) (Ref.: mean age)	0.193***	(0.011)	0.168***	(0.012)
Married	0.026***	(0.005)	0.022***	(0.006)
Children	0.036***	(0.007)	0.016**	(0.008)
Children $\leq$ 10 years	0.016**	(0.008)	0.027***	(0.008)
Non-German	−0.093***	(0.006)	−0.078***	(0.007)
Severely handicapped	−0.003	(0.020)	−0.026	(0.023)
Health restrictions	−0.065***	(0.008)	−0.060***	(0.009)
School leaving certificate (Ref. No degree or lower secondary school)				
Middle secondary school	0.046***	(0.006)	0.050***	(0.007)
(Specialized) Upper secondary school	0.198***	(0.007)	0.207***	(0.009)
Professional training (Ref. Unskilled)				
Apprenticeship or technical college	0.093***	(0.005)	0.094***	(0.006)
University degree	0.305***	(0.011)	0.360***	(0.014)
Sector of last job (Ref. Others (public sector, agriculture))				
Construction	0.150***	(0.007)	0.128***	(0.008)
Production	0.061***	(0.007)	0.039***	(0.009)
Wholesale/Retail	0.042***	(0.008)	0.038***	(0.010)
Private sector services	−0.031***	(0.006)	−0.022***	(0.008)
Local macroeconomic conditions				
Unemployment rate (Ref.: mean rate)	−0.009***	(0.001)	−0.010***	(0.001)
Real GDP per capita (Ref.: mean GDP)	−0.001***	(0.000)	0.000	(0.000)
Duration of previous unemployment (in months)				
Level	−0.015***	(0.001)	−0.005***	(0.001)
Squared	0.000***	(0.000)	0.000***	(0.000)
Ln(σ)	−1.208***	(0.004)	−1.742***	(0.006)
Mini-Job	−0.074***	(0.010)	−0.002	(0.009)

To be continued.

Table 3.11 continued.

	Without unobs. het.	P=9	
Unobserved heterogeneity			
V_{u2}		−0.018	(0.071)
V_{u3}		−0.707***	(0.058)
V_{u4}		−0.357***	(0.096)
V_{u5}		−0.535***	(0.067)
V_{u6}		−1.393***	(0.112)
V_{u7}		−1.700***	(0.115)
V_{u8}		0.373***	(0.093)
V_{u9}		−0.839***	(0.108)
V_{m2}		−0.448	(0.380)
V_{m3}		0.262	(0.194)
V_{m4}		3.022***	(0.171)
V_{m5}		1.125***	(0.159)
V_{m6}		−0.968*	(0.517)
V_{m7}		0.155	(0.220)
V_{m8}		−0.351	(0.483)
V_{m9}		0.796***	(0.228)
V_{e2}		−0.226***	(0.072)
V_{e3}		0.151**	(0.072)
V_{e4}		0.684***	(0.114)
V_{e5}		0.025	(0.063)
V_{e6}		−2.223***	(0.529)
V_{e7}		2.266***	(0.109)
V_{e8}		0.954***	(0.085)
V_{e9}		0.196**	(0.096)
V_{w2}		0.400***	(0.010)
V_{w3}		−0.196***	(0.007)
V_{w4}		−0.192***	(0.014)
V_{w5}		−0.604***	(0.010)
V_{w6}		−0.176***	(0.015)
V_{w7}		−0.513***	(0.014)
V_{w8}		−0.363***	(0.013)
V_{w9}		−1.030***	(0.014)
ω_2		−1.759***	(0.106)
ω_3		0.459***	(0.121)
ω_4		−2.018***	(0.191)
ω_5		−0.784***	(0.101)
ω_6		−0.208	(0.166)
ω_7		−1.150***	(0.145)
ω_8		−2.270***	(0.203)
ω_9		−2.161***	(0.139)
Correlations between unobserved terms			
Corr(V_u,V_m)		0.302***	(0.117)
Corr(V_u,V_e)		0.232*	(0.122)
Corr(V_u,V_w)		0.410***	(0.039)
Corr(V_m,V_e)		0.579***	(0.145)
Corr(V_m,V_w)		−0.390***	(0.062)
Corr(V_e,V_w)		−0.262***	(0.033)

Note: Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. V_{u2}-V_{u9}, V_{m2}-V_{m9} and V_{e2}-V_{e9} are the masspoints for the unemployment, treatment and employment probability; V_{w2}-V_{w9} are the masspoints for the wage equation. ω_2-ω_9 are the parameters to calculate the distribution of the masspoints as depicted in Equation 3.5. For identification V_{u1}, V_{m1}, V_{e1}, V_{w1} and ω_1 are set to be zero.

Table 3.12: Alternative Definition of Unemployment: Baseline Estimation Results

	Transition UE to RE		Transition RE to UE		Linear wage equation	
	(1)	(2)	(3)	(4)	(5)	(6)
Mini-Job	0.178***	0.100**	−0.001	−0.188***	−0.074***	0.002
	(0.034)	(0.047)	(0.041)	(0.062)	(0.009)	(0.009)
Unobs. Het. (P=9)	No	Yes	No	Yes	No	Yes

Note: Depicted are estimation results using an alternative definition of unemployment. In contrast to the preferred specification (see Table 3.7), here we additionally define periods not covered by the data as unemployment. Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation also includes control variables for duration dependence, seasonal dummies, individual socio-demographics, information on last job and local macroeconomic conditions. UE - Unemployment, RE - Regular employment.

Table 3.13: Alternative Definition of Unemployment: Effect Heterogeneity

	Transition UE to RE	Transition RE to UE	Linear wage equation
	(1)	(2)	(3)
Mini-Job	0.017	−0.289***	−0.023*
	(0.072)	(0.092)	(0.014)
Mini-Job × Ln(Age)	−0.090	−0.111	0.057
	(0.172)	(0.215)	(0.038)
Mini-Job × Unskilled	0.048	−0.012	0.049***
	(0.076)	(0.090)	(0.015)
Mini-Job × Construction	−0.069	0.059	0.024
	(0.081)	(0.101)	(0.016)
Mini-Job × Local UE-Rate	0.020	−0.024	−0.006**
	(0.014)	(0.017)	(0.003)
Mini-Job × Same Sector	0.133*	0.136	0.006
	(0.073)	(0.087)	(0.015)
Unobs. Het. (P=9)	Yes	Yes	Yes

Note: Depicted are estimation results using an alternative definition of unemployment. In contrast to the preferred specification (see Table 3.8), here we additionally define periods not covered by the data as unemployment. Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation also includes control variables for duration dependence, seasonal dummies, individual socio-demographics, information on last job and local macroeconomic conditions. Individuals who have no professional degree at entry into unemployment are categorized as "unskilled". "Same sector" indicates that the mini-job was taken up within the same sector (construction, production, wholesale/retail, private sector services, others) as the last regular job. UE - Unemployment, RE - Regular employment.

Table 3.14: Alternative Definition of Unemployment: Treatment Effect and Elapsed Unemployment Duration

	Transition UE to RE	Transition RE to UE	Linear wage equation
	(1)	(2)	(3)
Mini-Job	−0.074	−0.138*	−0.006
	(0.064)	(0.083)	(0.013)
Mini-Job × 7-12 months	0.161*	−0.067	0.021
	(0.091)	(0.106)	(0.018)
Mini-Job × 13-24 months	0.562***	−0.251**	0.024
	(0.094)	(0.117)	(0.018)
Mini-Job × 25-36 months	0.326***	0.225	−0.013
	(0.125)	(0.210)	(0.031)
Unobs. Het. (P=9)	Yes	Yes	Yes

Note: Depicted are estimation results using an alternative definition of unemployment. In contrast to the preferred specification (see Table 3.9), here we additionally define periods not covered by the data as unemployment. Coefficients are statistically significant at the * 10%, ** 5%, *** 1% level. The estimation also includes control variables for duration dependence, seasonal dummies, individual socio-demographics, information on last job and local macroeconomic conditions. UE - Unemployment, RE - Regular employment.

4 Youth Unemployment and the Effects of Active Labor Market Policy

A substantial number of young unemployed participate in active labor market programs in Germany each year. While the aims of these programs are clear—a fast re-integration into employment or enrollment in further education—a comprehensive analysis of their effectiveness has yet to be conducted. This chapter fills this gap using administrative data on youth unemployment entries and analyze the short- and long-term impacts for a variety of different programs. The results indicate positive long-term employment effects for nearly all measures aimed at labor market integration. Measures aimed at integrating youths in apprenticeships are effective in terms of education participation, but fail to show any impact on employment outcomes until the end of our observation period. Public sector job creation is found to be harmful for the medium-term employment prospects and ineffective in the long-run. The analysis further indicates that the targeting of German ALMP systematically ignores low-educated youths as neediest of labor market groups. While no employment program shows a positive impact on further education participation for any subgroup, the employment impact of participation is often significantly lower for low-educated youths.[61]

[61] This chapter is based on joint work with Marco Caliendo and Ricarda Schmidl (Caliendo et al., 2011).

4.1 Introduction

Young individuals entering the labor market are generally considered a population at risk, exhibiting an above-average turnover rate between jobs and an increased probability of entering unemployment. The employment situation of youths[62] is also particularly sensitive to economic fluctuations (Verick, 2011), which was recently demonstrated in the aftermath of the financial crisis. Between 2008 and 2009, youths in the European Union experienced an increase in unemployment rates of about five percentage points to a 20% average, compared to a two percentage-point increase for adults to an average level of 11%.[63]

The prevalent youth-adult unemployment gap can be explained naturally by the initially low search skills and little work experience of labor market entrants, which results in increased levels of turn-over. Although this vulnerability is expected to be only transitory, some youths encounter difficulties during the school-to-work transition process caused by more structural problems. Recent studies on the youth labor market situation in developed countries show that a persistent share of youths experience longer-term unemployment spells, with a strong imbalance towards youths with low educational attainment (Quintini et al., 2007). From an individual and a social perspective, this is a point of concern. Long unemployment spells are found to exhibit "scarring" effects on later labor market outcomes that seem to be more severe for young than for adult workers (compare, e.g., Ellwood, 1983). While the adverse effects on future employment probabilities are particularly persistent for low-educated youths (Burgess et al., 2003), the negative effects on wages seem to persist independently of individual characteristics (Gregg and Tominey, 2005). Potentially driven by foregone work experience or negative signalling, Korpi (1997) and Goldsmith et al. (1997) also show that the unemployment experience is associated with a decrease in subjective well-being and self-esteem, which might lead to a negative effect on current and future employment probabilities. In terms of social costs, there is evidence that rising levels of youths unemployment are not only related to an increase in spending on unemployment benefits and social assistance, but also to the depreciation of human capital, rising crime rates, drug abuse and vandalism (see Bell and Blanchflower, 2010, for an overview).

Against this backdrop, the majority of European countries spends signifi-

[62] We follow the general definition of youth as being 25 years or younger.

[63] Based on unemployment rates for youths (aged 15 and 24) and adults (aged 25 and 54) in 2008 and 2009 in the EU-27, from *Eurostat*.

cant resources each year to fight youth unemployment and improve the integration prospects of struggling youths. Active labor market programs (ALMP) are a common tool to achieve these goals. Between 1999 and 2002, countries in the EU-15 spent a yearly average of 1.3 billion euros on ALMP specifically targeted at unemployed youths (OECD, 2004). Although the primary objective of these programs lies in the fast integration in the first labor market, they may also target the continuation or take-up of vocational training for under-educated youths. The types of programs in use are manifold, ranging from targeted measures that account for the specific needs of labor market entrants, to the use of more "standard" ALMP, such as training, wages subsidies or job creation schemes. The prevalence of youth ALMP—introduced during the 1980s and 1990s—has continually increased during the past decade. In 2007 the number of young ALMP participants in the EU-15 amounted to approximately 14% of the youth labor force (between 15 to 24 years). The quantitative importance of ALMP thereby stands in stark contrast to the low level of knowledge regarding their effectiveness. Existing evaluation results of youth ALMP in Europe provide a rather heterogeneous picture of program benefit[64] suggesting that some of the measures implemented significantly reduce the employment probabilities of youths in the short to medium run. More evidence on the effectiveness of ALMP for youths is hence urgently needed to draw lessons for future policy design. Extrapolating from evaluation results for the adult workforce is misleading, given the distinctive characteristics of young labor market entrants. Moreover, the assessment of long-term effects is particularly important, as ALMP may not affect employment outcomes directly, but through their impact on participation decisions in longer-term education.

This chapter uses Germany as a case study to contribute to the evaluation literature of youth ALMP in Europe. Due to data restrictions, so far no comprehensive quantitative analysis of the effectiveness of ALMP for youths in Germany was conducted.[65] Our study aims to fill this gap. Even though Germany is considered a role-model of youth labor market integration, with its extensive dual-apprenticeship system, a non-negligible share of youths faces structural difficulties of integrating into the labor market. After leaving general education, youths face two stylized bar-

[64] See, e.g., Centeno et al. (2009) for Portugal; Dorsett (2006) for the UK; Larrson (2003) for Sweden; and Brodaty et al. (2001) for France and Caliendo and Schmidl (2011) for a recent overview.

[65] Compare Ehlert et al. (2010) for a recent evaluation of an innovative pilot project that was conducted in three German cities.

riers: the transition from general education to vocational schooling or training ("first barrier") and the subsequent transition from training to employment ("second barrier").[66] In the late 1990s specific ALMP targeted at unemployed youths were put into place, with measures more suited to accommodate the specific barriers faced by youths. Participation in ALMP has since substantially increased, calling for a thorough assessment of their effectiveness. We analyze the impact of participation in various ALMP in Germany, including job creation schemes, wage subsidies, short- and longer-term vocational training measures, as well as measure aimed at promoting participation in the vocational training system. We use administrative data on an inflow sample of youths into unemployment in 2002, in which we observe participants and non-participants of ALMP for a period of six years, until 2008. The main outcome of interest is the probability to be in regular employment, but we also investigate the effects on participation in further education as an intermediate policy objective. The long observation period allows a meaningful assessment of the short- and long-term program impacts in both cases.

Exploiting the detailed information on individual pretreatment characteristics we identify the program impact in a quasi-experimental evaluation framework. Based on a justifiable conditional independence assumption, we apply Inverse Probability Weighting (IPW). To account for dynamic treatment assignment and differences in program availability, we estimate the treatment effects separately by elapsed unemployment duration and calendar month of entry into unemployment. We further account for the differential labor market characteristics of East and West Germany, by conducting the analysis separately for the two regions.

The chapter is organized as follows. Section 4.2 briefly depicts the labor market situation of youths in Germany and the structure of the education system. Section 4.3 sets the stage for the evaluation analysis by providing details on the estimation approach, the data used and the programs analyzed. Section 4.4 focuses on the implementation of the estimation strategy, and the results are presented in Section 4.5 before Section 4.6 concludes.

[66] See Dietrich (2001) for an in-depth discussion of the barrier-concept.

4.2 Youth Unemployment and ALMP in Germany

4.2.1 The German Education System

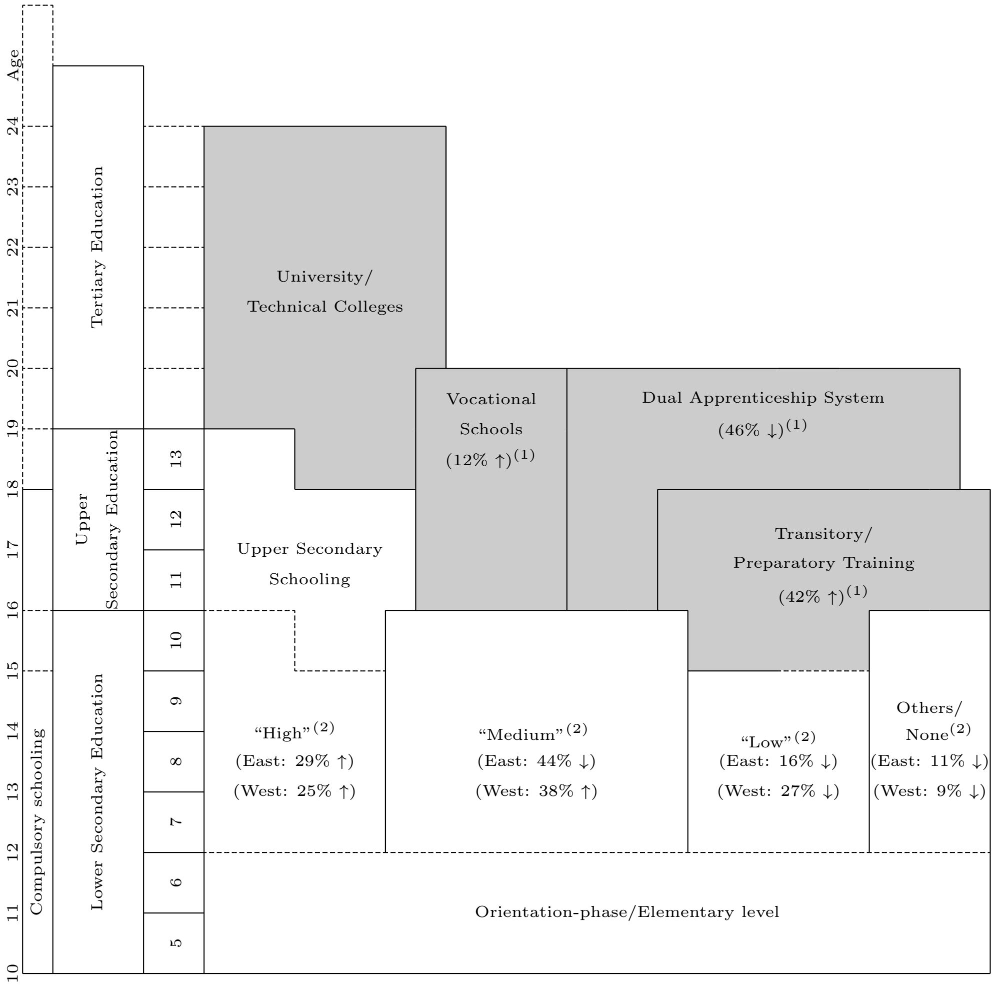
To set the stage for the following analysis it is helpful to briefly recall the structure of the German education and vocational training system (see Figure 4.1 for an overview).[67] The general secondary schooling system precedes the selection into the vocational training system and has three parallel types of schools: low (*Hauptschule*), medium (*Realschule*) and high (*Gymnasium*) secondary schooling. The vocational training system (*'upper secondary'* and *'tertiary'*) accommodates a variety of pathways that differ with respect to their degree of work–training interaction and their academic content; the higher the academic content, the higher is the required secondary schooling certificate needed to enter. For pupils finishing the lowest type of school the only immediately available vocational training option is the dual apprenticeship, unless they decide to acquire a higher general schooling degree. Pupils who obtain a medium schooling certificate, regularly spent one more year in general schooling and can choose between entering the dual apprenticeship system or full-time vocational schooling, where a state-approved professional degree can be obtained outside the dual system, in a broader range of professions. Finally, pupils who finish the highest schooling type are allowed to participate in any type of vocational education (see shaded areas in Figure 4.1). The shares in Figure 4.1 indicate that medium secondary schooling is by far the most important one in Germany, with an average share of 38% (44%) of graduates in West (East) Germany.[68] It can also be seen that youths in the East have on average a higher level of schooling attainment than their Western counterparts. In both regions a persistent share of 10% leaves lower secondary school with no certificate.

The dual apprenticeship system is the most important option of the vocational training system, accounting for roughly half of all entries each year; where the majority (roughly 80% in 2004) of the applicants has a certificate from a low or medium level school (see Autorengruppe Bildungsberichterstattung, 2006). Since the demand for apprenticeships mostly exceeded supply in the early 2000s, access to the dual apprenticeship system is competitive and particularly problematic for youths with

[67]Unless otherwise indicated, the following section relies heavily on the official description of the German education system provided by the Kultusministerkonferenz Germany and the EURIDYCE Unit (2009).

[68]Statistics are taken from Bundesministerium für Bildung und Forschung (2009) and the Federal Statistical Office.

Figure 4.1: The German Education System



Source: BIBB 2009, Federal Statistical Office.
Note: Shaded areas denote the vocational part of the education system. (1) Average annual shares of yearly entries into vocational education between 1998 and 2006. (2) Average annual shares of yearly school leavers at the secondary level between 1998 and 2006. Arrows indicate trends in these years.

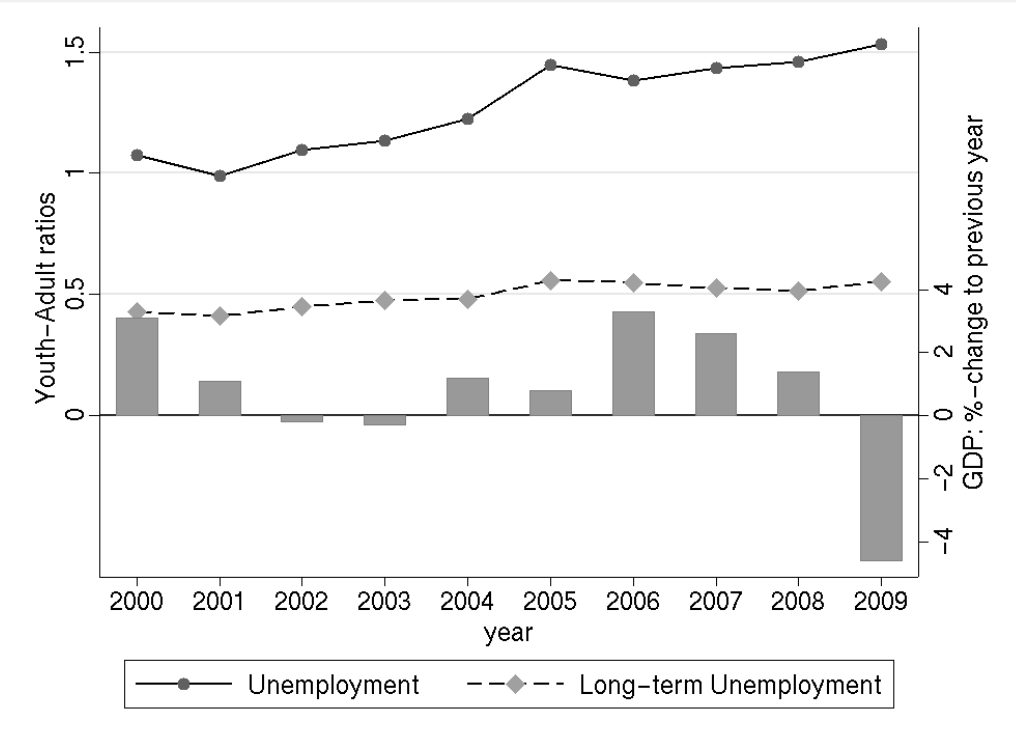
low previous educational attainment. Given that it is particularly these youths who have only few further options for obtaining vocational education, they are likely to enter unemployment at this "first barrier". At risk of experiencing longer unemployment spells or exiting into inactivity, an extensive preparatory/transitory training system has been put into place aiming to prepare these youths towards a successful entry into the apprenticeship system or other options of the vocational education (see Neumann et al., 2010, for an overview). From 2000 to 2010, participation rates in the preparatory system have increased by about 50% —in years of low demand for apprentices, more youths enter the preparatory system than the apprenticeship system (Bundesministerium für Bildung und Forschung, 2009).

Due to the high labor market orientation of the vocational training system in Germany, the transition from vocational training into employment is generally characterized by relatively low levels of friction—although not all youths manage a smooth transition at this "second barrier". A lack of data that tracks youths after graduation from vocational education makes it difficult to assess the specific unemployment risks youths face after graduation. Reinberg and Hummel (2005) provide general figures for the unemployment risk of youths with different levels of vocational education. They show that individuals with no vocational qualification are up to three times more likely to be unemployed than youths with qualification—compared to youths with tertiary education they are eight times as likely.

4.2.2 Youth Unemployment and ALMP in Germany

To assess the particularities of the employment situation of youths compared to the general population, it is helpful to relate youth labor market outcomes to the ones of more senior workers. A persistent pattern to be found across all European countries is that youths are usually more likely to enter unemployment than adults, but that their unemployment spells are more transitory, i.e., they exit unemployment more often than older workers (compare, e.g., Caliendo and Schmidl, 2011, for a recent overview on the employment patterns of youths across the EU-15). Descriptive evidence on the overall economic conditions and the unemployment situation of youths in Germany during the period of our investigation exhibit a similar pattern, as can be seen from Figure 4.2. In particular, the youth-adult unemployment ratio gradually increased from almost identical levels in 2000 to about 1.5 in 2009, whereas the long-term unemployment ratio oscillates persistently at around 0.5. Compared to the EU-average, where the unemployment ratio is around 2 to 3, youths in Germany face a comparably low risk of entering unemployment, which is generally attributed to the strong labor market link of the apprenticeship system. In terms of the probability for young people to enter long-term unemployment, however, Germany is amongst the European countries with the highest risk and this is clearly cause for concern. The rise in the youth-adult unemployment ratio during the observation period can be partially explained by the slowing German economy after 2000, but potentially also by an institutional reform in 2001, reducing the legal restrictions on part-time and fixed-term work. The extensive labor market reforms between 2002 and 2005 (the *Job AQTIV Act* and *Hartz*-reforms) further extended the realm of temporary work arrangements (see Jacobi and Kluve, 2007, for a more detailed description of the *Hartz*-reform changes), thereby leading to a strong increase in youths entering the labor market in "atypical" employment relationships with less stable long-term employment outcomes.

Figure 4.2: Unemployment and Long-term Unemployment Youth-Adult Ratios, and GDP Growth Rates in Germany between 2000 and 2009



Source: Federal Statistical Office; Statistics of the Federal Employment Agency

To fight unemployment Germany strongly relies on ALMP. The majority of ALMP schemes are financed by the federal government and the regulations regarding their implementation are contained in the *Social Act III (SGB III)*. Unemployed youths who fulfill the eligibility criteria, are entitled to participate in the standard ALMP schemes available in the *SGB III*, e.g., training measures, wage subsidies, job creation schemes, etc. As part of the above-mentioned labor market reforms, significant adjustment of the implementation practice of ALMP were made after 2000, with the objective of reaching a faster activation of unemployed individuals. Besides an increase in monitoring efforts, this lead to the expansion of ALMP offering job search assistance and short-term training courses. Furthermore, the *Job-AQTIV Act* of 2002 integrated specialized youths measures within the *SGB III*, that became effective only in 2004. Before the integration of these measures into the *SGB III*, the only youth-specific ALMP on the federal level existed within the program of *Immediate Action Program for Lowering Youth Unemployment (JUMP)*. *JUMP* was introduced in 1999, following an increasing importance of ALMP in European and German policy debate as means to deal with the increasing number of youths who were unemployed or unable to find an apprenticeship placement. By the provision of additional financial means of around one billion euros per year, and the facilitation the access to ALMP by reducing the eligibility criteria for unemployed and disadvantaged youths, it was intended to enable a faster integration of youths into ALMP.[69] Furthermore, *JUMP* introduced some new measures specifically aligned to the requirements of unemployed youths, which have later on been partly integrated into the *SGB III*. Originally set up for only one year, *JUMP* was extended each year and finally expired in 2004 (between July 2003 and December 2004 the program was called *JUMP Plus* intending to support 100,000 long-term unemployed youth).

4.2.3 Programs Under Consideration

Statistics from the German federal employment agency on the overall numbers of entries into ALMP indicate a substantial increase in participation rates among youths between 2000 and 2010. In 1999 around 600,000 youths were registered in ALMP within *SGB III*—in 2009 the figure was 1.9 million. Between 1999 and 2003, there was on average an extra of 156,000 youths each year entering the programs of *JUMP* (see Dornette and Jacob, 2006, for a detailed participant structure of *JUMP*). Regarding the type of assistance offered, the ALMP in place can be grouped into three broad categories. The most important one in terms of entry numbers are counseling and placement help, with about 60% (50%) of all yearly entries in the *SGB III* in East (West) Germany.[70] Furthermore there are longer-term measures either aiming to promote the integration of youths into an apprenticeship or to help them integrate into the first labor market (training programs, wage and self-employment subsidies, and job creation schemes). Participation in ALMP (compared to the workforce) is generally higher in East Germany, where labor market conditions are less favorable.

In our analysis we assess the impact of seven types of programs, which constitute the most important ones in terms of participation numbers during the period under study (compare Section 4.3.3). Table 4.1 contains a list of the programs, a brief description of their content and their duration. Similar programs offered in *JUMP* and *SGB III* simultaneously are grouped together if official implementation

[69] For a detailed synopsis of the objectives and measures associated to the introduction of *JUMP*, see Bundesministerium für Arbeit und Soziales/Bundesministerium für Bildung und Forschung (1999)

[70] Shares are provided by the statistical office of the federal labor agency; entries into ALMP between 1999 to 2009 without mobility aid, which technically only includes a cash-transfer to increase the mobility of youths.

guidelines, participant structure and program duration suggested similar content.[71]

Job search measures (JS) include job search monitoring and the assessment of the career opportunities of individuals. Short-term training programs (STT) offer courses of a very short duration to improve auxiliary skills that are important in the application process, e.g. computer classes or language courses. The intended short duration of both programs aims to facilitate job search activities during participation, so that locking-in in these programs is expected to be small. However, due to this short duration JS and STT measures are not suited to reduce structural deficits of labor market entrants. Often used as device to assess the employability of youths, it is particularly likely that youths participate in further ALMP subsequent to participation in JS or STT. As sequential program participation renders causal estimation of the impact of short-term programs more difficult, we address this issue in Section 4.5.3 specifically. Job creation schemes (JCS) and further training (FT) are longer-term measures with a median duration of five to seven months, aimed at overcoming more structural problems of integration in the labor market. JCS are predominantly practically oriented, providing some type of work experience for youths with very little previous labor market experience and potentially low labor market attachment. Although participants receive only low levels of remuneration during program participation, locking-in in these programs is expected to be high for youths with few outside options. In contrast, FT measures are predominantly focused on youths with vocational qualification, who seem to require additional qualification to succeed in the labor market. The program usually comprises classroom training and may vary between part- or full-time courses.

In contrast to these supply-oriented measures, the wage subsidies offered within the *SGB III* (WS) and *JUMP* (JWS), are aimed to overcome demand side restrictions. The two programs differ with respect to the size of the subsidy and the time period for which it is granted. While the subsidy in WS was regularly limited to one year and provided 50% of the monthly wage, JWS could either be taken up for one year and 60% replacement, or two years and 40% of replacement; employers had to guarantee a period of post-subsidy employment which was equivalent to the subsidized period for WS and half the subsidized period for JWS. While the objec-

[71]The administrative data used contains a very detailed listing of programs, differentiated by content and sources of funding, we aggregate programs with comparable content. In the case where *JUMP* contained a program similar to the regular activation measures, we compared the two measures with respect to their duration, participant structure, etc. and formed a common group only if they did not significantly diverge.

Table 4.1: Description of the Programs Under Scrutiny and Sample Frequencies

Abbr.	Program content and regulatory framework	Participants		Observed duration (months)			
		East	West	East		West	
				50%-ile	90%-ile	50%-ile	90%-ile
JS	*Job Search and Assessement of Employability:* So-called “profiling” immediately after individuals enter unemployment, including professional counseling by the employment agency (EA), short-term measures to improve employability and mobility aid. Conclusion of an informal contract to systematize and monitor search effort, as well as measures to be taken by the EA for a quick and successful re-integration of the unemployed.	1,345 (25.1%)	1,915 (27.3%)	1	2	1	3
STT	*Short-Term Training:* Full- or part-time training measures aimed at improving the employability of youths, including coaching for the application process, and training of specific skills. In the *SGB III* the former should have a maximum duration of two weeks, the latter of eight weeks. *JUMP* measures are not considered.	979 (18.3%)	1,885 (26.8%)	2	4	2	6
JWS	*JUMP Wage Subsidies:* Wage subsidy to regular employment with minimum 15 hours per day at the maximum amount of 60% (40%) of the full wage, for a maximum duration of one (two) years. No minimum duration in unemployment necessary. Post-subsidy employment of half the subsidized period.	991 (18.5%)	628 (8.9%)	12	21	6	13
WS	*SGB III Wage Subsidies:* Wage subsidy to regular employment at the maximum amount of 50% of the full wage, for a maximum of one year. No minimum duration in unemployment. Post-subsidy employment of the same duration as the subsidized period, but a maximum of 12 months.	439 (8.2%)	502 (7.1%)	6	13	4	11
JCS	*Job Creation Schemes:* Working opportunity in areas of the public interest, e.g. infrastructure, social work. Low level of remuneration subsidized by the EA. In the *SGB III* the maximum duration of 12 months could be extended if it leads to regular employment. Very similar program within *JUMP*, here placement subordinate to placement in training or regular employment—parallel qualification measures should be implemented, but could be suppressed if they do not seem sensible.	680 (12.7%)	570 (8.1%)	7	12	7	12
FT	*Further Training Measures:* Long-term training measures for youths with or without professional degree, providing them with job-specific skills. Intensity of training was normalized to 25 to 35 hours per week. The total duration of the measures should not exceed one third of the regular vocational training, i.e. approximately one year, but could be extended if necessary. *JUMP* measures are not considered.	409 (7.6%)	515 (7.3%)	6	12	5	11
PT	*Preparatory Training:* Practical training/internship within a company that should help find and successfully participate in regular vocational training. Duration of training could vary within the *SGB III*. Within *JUMP* it was limited to one year, and potentially also included catching up on the lower secondary schooling degree.	510 (9.5%)	1,012 (14.4%)	7	12	6	12
Total		5,353	7,027				

Note: Program description based on policy guidelines for *JUMP* and the legal text of the *SGB III* in place in 2002. Calculations based on the estimation sample. Abbr. - Abbreviation.

tive of the previous programs is a direct labor market entry, preparatory practical training measures (PT) aim to enhance the chances of youths struggling at the "first barrier", i.e., at entering the vocational training system. The program consists in a subsidized internship within a firm where predominantly basic practical skills and literacy are conveyed. Some employers might also use this as a probation period before offering a full apprenticeship position within the firm.

4.3 Estimation Strategy and Data

4.3.1 Identification of Causal Effects

We base our analysis on the potential outcome framework (Roy, 1951; Rubin, 1974) where D denotes the treatment indicator, Y^1 the potential outcome in the case of treatment ($D = 1$) and Y^0 the outcome without treatment ($D = 0$). The observed outcome for each individual i is given by $Y_i = Y_i^1 \cdot D_i + (1 - D_i) \cdot Y_i^0$. Our aim is to estimate the average treatment effect on the treated (ATT) on the population level that is formally given by $\tau = E(Y^1 \mid D = 1) - E(Y^0 \mid D = 1)$. As we are faced with the fundamental evaluation problem of not observing each individual simultaneously in the both the treatment and the non-treatment state, we need a meaningful substitute for the counterfactual (the second term on the right hand side). Approximation by the observed average non-treatment outcome of the non-treated, i.e., $E(Y^0 \mid D = 0)$ does generally not lead to a meaningful estimate, as participants and non-participants are likely to be (self-)selected groups with differential outcomes even in absence of the program. In the absence of random treatment assignment selection into the treatment is assumed to occur systematically based on observable or unobservable characteristics (or both).[72]

In the case where the participation decision depends on observable characteristics W only, we can estimate the ATT by conditioning on these variables, rendering the counterfactual outcome independent of treatment, i.e., $Y^0 \amalg D|W$ (*conditional independence assumption*, CIA). Rosenbaum and Rubin (1983) show that instead of conditioning on a potentially extensive set of characteristics W directly, conditioning on the probability of treatment participation $P(D = 1|W)$ (the propensity score) suffices to achieve balance between treatment and control group. To ensure that we can find an adequate counterfactual for each treated individual it is furthermore

[72] See, e.g., Caliendo and Hujer (2006) for further discussion.

required that the covariates influencing assignment and outcome do not deterministically predict treatment participation, i.e. that $Pr(D = 1 \mid W) < 1$ holds for all W (*weak overlap*). Furthermore, it is required that general equilibrium effects do not occur, e.g., the treatment participation of one individual can not have an impact on the outcomes of other individuals, independent of their treatment participation (*stable unit treatment value assumption*, SUTVA). The validity of this assumption is likely to depend on the scope of the program as well as size of the resulting effects (Imbens and Wooldridge, 2009). As on average only 12% of the active youth population in Germany participated in ALMP from 2000 to 2007, the scope for general equilibrium effects seems rather limited in our case, so that we expect the SUTVA to hold.

The validity of the CIA is more difficult to justify, as it requires that all relevant variables that simultaneously influence participation and outcome can be controlled for (compare, e.g., Smith and Todd, 2005 or Sianesi, 2004). The availability of informative data is therefore crucial. Although there is no common rule on the particular set of information necessary, the ALMP evaluation literature provides helpful guidance on the question which variables to include. Lechner and Wunsch (2011) argue that more information lowers the bias, and highlight the importance of information on labor market history, caseworker assessments, job search effort, timing of unemployment and program start, health indicators, characteristics of last employer and regional characteristics. As our data is based on detailed administrative records, we are able to reproduce the set of variables suggested by Lechner and Wunsch (2011) to a very large extent (see Table 4.4). When dealing with youths, however, the importance of, e.g., observing past labor market histories to capture relevant but potentially unobservable selection variables (motivation, labor market skills, regional particularities, etc.) is likely to lose substantial power as labor market biographies do not yet exist, or are only limited. Hence, besides including labor market histories for those youth who have already labor market experience (employment and earnings, unemployment, inactivity and treatment participation during the three years prior to unemployment entry), we also include further productivity signals which are likely to justify the CIA. Specifically, we rely on information from the caseworkers (number of placements offers and last contact to labor agency before current unemployment spell) which show to be powerful predictors of treatment assignment. This is not surprising as the caseworkers perception on the labor market performance of unemployed is likely to be more important for the participation decision of low experience youths than for adults. Provided with this additional strong signal of unobserved ability of young unemployed, we argue that the CIA is a reasonable identification strategy in our context.

4.3.2 Definition of Treatment and Control Group

To estimate causal effects in the potential outcome framework, the definition of the treatment status requires clarification. Our question of interest is whether participation in an ALMP program has an impact on labor market outcomes of youths, in contrast to a situation where the program had not been available. In our setting, all unemployed youths are potentially eligible to participate in a program—and they may do so at different points in time—which complicates the straightforward definition of a group of participants and non-participants. As pointed out by Sianesi (2004), defining a treatment group by conditioning on ever observing individuals in treatment simultaneously restricts the control group to individuals who have successfully exited into employment before they could participate in a program, which would introduce bias in the effect estimates.

In the evaluation literature two streams exist to deal with this issue, a "static" and a "dynamic" approach. The dynamic approach makes no direct assumptions about the occurrence of the treatment but considers the timing of treatment as a stochastic process.[73] For the definition of the two groups this means that the distinction between treated and controls is made recurrently at each point in time, based on the observed state of all eligible individuals, and is therefore independent of any treatment status at a later point. Although this selection mechanism is realistic in our setting, the approach has the disadvantage of limited interpretability of the estimates. As the control group includes future program participants, the estimated effects have to be seen as a mixture of "participation vs. non-participation" and "participation now vs. participation later" (see Lechner et al., 2011). In the case of multiple available programs the estimated effects additionally include a relative effect compared to participation in a different program. The static approach on the other hand considers participation vs. non-participation in a particular program based on observing individuals up to a pre-determined point in time and thereby requires conditioning on future outcomes for the non-treatment group (Lechner et al., 2011).

[73]See Abbring and van den Berg (2003, 2004) for a discussion in a duration model framework and Fredriksson and Johansson (2008); Sianesi (2004) for an application of semi-parametric matching.

The interpretation of the estimated effects is more obvious as only "never-treated" (within a certain time period) non-participants contribute to the counterfactual outcome. As pointed out, the restriction on future outcomes is likely to create a control group consisting of a positively selected subgroup of all eligible unemployed and might therefore bias the results downwards.[74]

As we are interested in the effect of participation vs. non-participation, and given the variety of ALMP offered in Germany which render relative effects rather untransparent, we follow Lechner et al. (2011) and apply the static evaluation approach.[75] To do so we have to define a cut-off in unemployment duration at which individuals are assigned to the treatment group (if they participate before the cut-off) and control group (if not). The choice of the cut-off should balance two opposing influences. On the one hand, the estimation bias due to the restriction on future outcomes is increasing with the time window (Fredriksson and Johansson, 2003); on the other hand, a small entry window increases the variance of the estimates due to lower observation numbers, and might also reduce the external validity of the results due to potential seasonal effects. Therefore, we decide to specify the first 12 months of unemployment as our entry window. First, this is not too restrictive on control outcomes since 50% (40%) of non-treated individuals in East (West) Germany are still unemployed after 12 months. Second, it secures a sufficient number of treated observations and reduces the influence of seasonal effects as it captures the complete year.[76] Hence, we assign youths to the group of participants if they enter an ALMP program under consideration (see Table 4.1) within the first 12 months of their unemployment spell and to the group of controls if not. Note, that we discard individuals who participate in any other program within the first 12 months. When individuals participate in multiple programs during their unemployment spell, we focus on the first one in the main analysis.[77]

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[74] Lechner et al. (2011) argue that this argument would even strengthen the effectiveness of programs in the case of positive results.

[75] We test the sensitivity of our results with respect to the choice of the evaluation approach and provide in Section 4.5.3) results using the dynamic approach.

[76] The dynamic changes in the selection process due to the changes in the composition of unemployed, and potential changes in the types of programs offered during this time period are controlled for in the estimation process (see Section 4.4.2).

[77] About 50% (33%) of treated in the East (West) participated in multiple programs during their unemployment spell, with about 10% (5%) participating in further ALMP within the first 12 months. However, we focus on the first program as subsequent program participation has to be considered as the outcome of the first treatment.

4.3.3 Data and Descriptives

To assess the impact of program participation on labor market outcomes, we use data from the administrative part of the *IZA Evaluation Dataset*.[78] It is based on the *Integrated Employment Biographies* (IEB) by the Institute for Employment Research (IAB) and consists of a random draw of unemployment entries between 2001 and 2008. It combines different administrative data sources, i.e., the Employment History, Benefit Recipient History, Training Participant History and Job Search History, and contains detailed daily information on spells in employment subject to social security contribution, unemployment, and participation in ALMP.[79] Linked to the information on the respective labor market status, the data include information on income from wages and benefits, on the previous labor market history and socio-economic characteristics of individuals.

We restrict our estimation sample to unemployment inflows in 2002.[80] This guarantees a sufficiently large observation window (at least 72 months after entry into unemployment) and allows us to obtain long-term impact estimates even for the longer running programs. Our choice of the year 2002 also takes account of the adoption of the *JobAqtiv Act* in the beginning of 2002, which entailed significant changes in the strategy of unemployment activation and implementation practice. Besides avoiding potential structural breaks in the implementation of programs between 2001 and 2002, the evaluation results for the programs under the new "regime" are also more relevant for current policy discussion, as their set-up resembles much more the set-up of programs in place today. Based on our initial inflow sample into unemployment in 2002, we only keep youths (aged 25 or younger) and apply several further sample selection criteria which are summarized in detail in Table 4.9 in the Appendix. We end up with an estimation sample of 51,019 unemployment entrants, corresponding to 17,515 youths from East and 33,504 youths from West Germany. Applying the definition of treatment status as discussed above, we identify 5,353 (7,027) youths in the East (West) participating in one of the programs under scrutiny within the first 12 months of unemployment. By restricting treatment to those ALMP entries in the first 12 months after unemployment entry, we capture about 62% (65%) of all individuals who enter one of the programs in our

[78]For a detailed description of the *IZA Evaluation Dataset* see Caliendo et al. (2011).

[79]This does not include information about self-employment, civil servants or inactivity.

[80]Where we observe multiple entries into unemployment per individual, one spell is randomly drawn.

total observation period of 72 months in the East (West). Non-participants are defined as individuals who do not participate in any ALMP within the first 12 months of unemployment but who are potentially treated later in months 13-72, which is relevant for approximately 27% (14%) of non-participants in the East (West). Since the administrative data records only specific labor market states, we have missing observations for spells of schooling and education, military service, self-employment or inactivity. Some of these states are particularly likely to occur for young individuals. To overcome this problem we apply an imputation method that relies on information for the planned activity in the subsequent spell recorded within each spell and in case of unemployment spells for the last activity before unemployment. By this procedure we are able to fill 92% of all missing monthly information, decreasing the share of monthly missings from initially 25.7% to 2.1%. Inspection of the type of information filled further reveals that non-randomly missing information does not pose a problem in our analysis (see Section 4.7.1 in the Appendix for details).

Table 4.1 provides the number of observations for each of the programs under investigation and moments of the distribution of program duration. As expected we find that the majority of our participants enter short-term measures, i.e., job search (JS) and short-term training measures (STT). Together, they account for almost half of participants in East and West Germany. This is naturally explained by our definition of treatment, as we focus on the first treatment after unemployment entry. Wage subsidies constitute the second most important types of measures. While WS are equally important in terms of participation shares in East and West, JWS are taken up twice as frequently in the East than in the West and have a longer duration. Furthermore JCS measures are used more extensively in East than in West Germany, potentially reflecting the lack of employment opportunities for low-educated youths in the East. Finally we find that PT are used in the West more often than in the East, with 14% of youths in the West and 10% of ALMP participants in the East.

Table 4.2 provides selected descriptive statistics of the program participants in East and West Germany (measured on entering unemployment). About two thirds of program participants are male, with youths being usually older than 20 years. The regional migrant participation rates reflect the strong populations differences between East and West Germany with 3% (12%) of participants having a migration background in the East (West). Further differences across East and West emerge in terms of the pretreatment educational attainment. While the average program

Table 4.2: Selected Descriptive Statistics of Participants and Non-Participants

	JS	STT	JWS	WS	JCS	FT	PT	NP
				East Germany				
Gender (Female)	0.37	0.42	0.42	0.40	0.30	0.31	0.41	0.39
Age (above 20 years)	0.72	0.73	0.71	0.72	0.66	0.77	0.27	0.56
Migration status	0.02	0.09	0.01	0.04	0.03	0.02	0.07	0.05
Having children	0.05	0.08	0.05	0.05	0.08	0.07	0.07	0.05
Health restrictions	0.10	0.08	0.04	0.06	0.17	0.05	0.09	0.07
School leaving certificate								
None	0.06	0.05	0.01	0.03	0.14	0.05	0.19	0.07
Lower secondary school	0.37	0.30	0.23	0.26	0.47	0.30	0.44	0.25
Middle secondary school	0.52	0.56	0.65	0.59	0.36	0.58	0.32	0.44
Upper/specialized secondary School	0.06	0.08	0.10	0.11	0.03	0.07	0.06	0.24
Professional training								
None	0.23	0.29	0.13	0.22	0.47	0.17	0.89	0.52
Apprenticeship/university	0.77	0.71	0.87	0.78	0.53	0.83	0.11	0.48
During the last three years before unemployment entry, months spent in ...								
regular employment	18.26	15.05	21.06	18.84	13.00	16.44	3.69	11.69
ALMP	4.42	4.41	3.24	3.64	5.17	5.10	3.47	2.71
inactivity	8.02	11.64	7.70	8.72	11.38	9.18	24.06	16.54
unemployment	5.76	5.24	4.12	4.98	6.85	6.18	3.99	3.99
Last activity before entry into unemployment								
Regular employment	0.63	0.59	0.62	0.56	0.58	0.64	0.40	0.54
Education, training, never employed	0.28	0.34	0.34	0.36	0.31	0.28	0.40	0.36
Other	0.08	0.07	0.05	0.08	0.12	0.07	0.20	0.10
Number of placement propositions	4.77	3.95	3.27	3.24	3.45	3.93	0.93	1.89
				West Germany				
Gender (Female)	0.36	0.38	0.34	0.36	0.30	0.33	0.38	0.39
Age (above 20 years)	0.72	0.70	0.73	0.77	0.48	0.81	0.29	0.61
Migration status	0.16	0.27	0.13	0.19	0.17	0.16	0.19	0.16
Having children	0.05	0.06	0.05	0.06	0.03	0.06	0.03	0.05
Health restrictions	0.08	0.07	0.06	0.07	0.08	0.08	0.06	0.07
School leaving certificate								
None	0.13	0.12	0.09	0.10	0.31	0.10	0.23	0.13
Lower secondary school	0.50	0.49	0.50	0.52	0.55	0.50	0.52	0.44
Middle secondary school	0.29	0.31	0.33	0.29	0.12	0.34	0.21	0.28
Upper/specialized secondary School	0.08	0.07	0.08	0.09	0.02	0.06	0.04	0.15
Professional training								
None	0.46	0.48	0.35	0.40	0.85	0.36	0.93	0.55
Apprenticeship/University	0.54	0.52	0.65	0.60	0.15	0.64	0.07	0.45
During the last three years before unemployment entry, months spent in ...								
regular employment	18.94	16.77	19.73	18.95	8.92	20.10	5.71	14.81
ALMP	2.72	2.15	2.74	2.34	3.10	2.31	3.14	1.77
unemployment	4.35	3.71	4.28	4.61	4.98	4.35	3.20	3.24
inactivity	9.50	12.89	8.70	9.67	17.78	9.08	21.46	14.68
Last activity before entry into unemployment								
Regular employment	0.70	0.65	0.74	0.69	0.59	0.74	0.48	0.63
Education, training, never employed	0.21	0.27	0.18	0.23	0.24	0.19	0.38	0.26
Other	0.09	0.08	0.07	0.08	0.17	0.07	0.15	0.11
Number of placement propositions	4.22	3.85	4.27	4.56	3.02	3.58	1.47	2.32

Note: Characteristics are measured at point of entry into unemployment. Numbers are shares unless otherwise stated. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training; NP - non-participants.

participant in the East has acquired a middle secondary school certificate, their counterpart in the West has a lower secondary school certificate. Furthermore, about 75% of youths in the East have already received some type of apprenticeship training compared to only about 50% in the West. In line with the observed differences in program importance this underscores that youths in the West seem to require help at overcoming supply-sided restrictions caused by their insufficient level of educational attainment, while unemployed youths in the East are rather held back by the low labor demand. For example, the importance of measures to overcome the “first barrier” in the West can be explained by the low schooling levels of West German youths.

The comparison of participant characteristics across program types shows a clear divide in terms of labor market attachment. The labor market histories during the three years preceding unemployment entry show that youths in either type of wage subsidies (WS and JWS), longer-term training measures (FT) and job search assistance (JS) have spent more time in (full-time) employment, less time in inactivity (e.g. schooling), as participants in other programs and non-participants. Although they have spent a comparable amount of time in unemployment, they are also slightly older, have received a larger number of placement offers during their current unemployment spell, and in the East they are also better educated than the rest. The greater attachment of these youths to the labor market compared to non-participants is somewhat suggestive of “cream-skimming” or at least a positive selection into these program based on these observed characteristics.

Individuals with adverse labor market prospects seem to be concentrated in JCS and PT programs. Given the differential objective of PT measures, the adverse characteristics (e.g., they are on average younger, did not obtain a school leaving certificate, and have received significantly fewer placement offers) of participants in PT are not surprising. The characteristics of JCS participants are similarly adverse, suggesting that it is also the low educational attainment that keeps them from integrating into the first labor market. Furthermore JCS participants are older and exhibit above average shares of youths with health restrictions in the East—suggestive of more structural difficulties of integrating in the labor market than the other program participants. Note, that the programs’ objective (compare Section 4.2.3) is the provision of work experience but not the increase in educational attainment. The first descriptive assessment of program characteristics hence suggests that placement in JCS is not primarily seen as stepping stone to further employment, but more as last resort for keeping these youths in the labor force.

4.4 Empirical Implementation

4.4.1 Inverse Probability Weighting

Based on the assumptions outlined in Section 4.3.1, the treatment and control group can be made comparable by conditioning on the propensity score (PS), i.e., $E(Y^0 \mid D = 1, P(W)) = E(Y^0 \mid D = 0, P(W))$, which then identifies the the average treatment effect on the treated τ. Based on the PS, different approaches have been suggested to estimate an adequate counterfactual outcome, where the predominately used methods are semi-parametric matching or reweighting (see, e.g., Imbens, 2004). The most suitable method has to be chosen depending on the study and context. Given our large set of covariates and the relatively homogenous groups of treated and controls we apply inverse probability weighting (Imbens, 2000, 2004). The IPW estimator has preferable finite sample properties compared to different matching algorithms under the requirement that the propensity scores are estimated and the weights are normalized to one (shown by Busso et al., 2009, in a Monte Carlo study). Huber et al. (2010) also show that IPW performs well under extensive variation of the data set-up, although it is outperformed by some advanced matching estimators. Given the major advantage of a lower computational burden during the bootstrapping procedure for the estimation of standard errors IPW seems to be an appropriate choice in our setting.

The idea of IPW is to adjust the outcomes of the non-treated by weighting them with the inverse of the estimated propensity scores $\hat{P}(W)$. The estimate of our parameter of interest τ^{IPW} is then obtained as the difference between the average outcome of the treated and the reweighted average outcome of the non-treated:

$$\tau^{IPW} = \left[\frac{1}{N^1}\sum_{i\in I^1} Y_i\right] - \left[\sum_{i\in I^0} \frac{Y_i \hat{P}(W_i)}{1-\hat{P}(W_i)} \Bigg/ \sum_{i\in I^0} \frac{\hat{P}(W_i)}{1-\hat{P}(W_i)}\right] \tag{4.1}$$

where $\hat{P}(W_i)$ is the estimated propensity score and the division of the counterfactual outcome by $\sum_{i\in I^0} \frac{\hat{P}(W_i)}{1-\hat{P}(W_i)}$ ensures that the weights add up to one (see Imbens, 2004). One concern associated with IPW is that it is particularly sensitive to large values of the propensity scores as they receive disproportionately large weights in the construction of the counterfactual (see Frölich, 2004). However, the relevance of this problem decreases with sample size as each observation has asymptotically less influence on the estimate (Huber et al., 2010). This problem should only play a minor role in our study as we apply a very restrictive common support condition (see Section 4.4.3) and have a large number of non-treated observation which leads to an average treated-control ratio of approximately 1 to 20. In addition, we test the sensitivity of our results with respect to this issue in Section 4.5.3 by trimming the distribution of the propensity scores of the non-treated.

4.4.2 Perfect Alignment of Treatment and Control Groups

As pointed out by the previous literature, participant characteristics and the type of treatment received may vary with the timing of entry into a program (compare, e.g. Sianesi, 2004 and Fitzenberger and Speckesser, 2007). As we define treatment over a period of 12 months after entry into unemployment we need to take into account potential dynamics in the selection into treatment or out of unemployment during this period. To mimic the selection process up to a particular point in time only individuals with similar unemployment durations should be compared. Given the small number of monthly treatment entries in our sample, estimation of the propensity score within monthly cells is not feasible. Instead we adopt an approach suggested by Fitzenberger and Speckesser (2007), consisting of stratified estimation of the PS within larger time windows combined with a "perfect" (i.e. monthly) alignment of treated and controls for the estimation of the treatment effect.

For the estimation of the PS we stratify the sample of treated into three subgroups based on their elapsed unemployment duration until treatment entry: (1) one to three months of unemployment duration, (2) four to six months and (3) six to twelve months. The treatment group in the respective cells hence consists of all individuals receiving treatment within these months of their unemployment spell. The control group consists of youths who are still unemployed in the first months of the respective stratum and who are not treated in the first 12 months of their unemployment spell. Based on the estimated propensity score, weighting of the controls is done within the "alignment cells". Besides aligning individuals perfectly on the month of entry into the program, we further take account of seasonal labor market conditions and program variability across calender time (see Sianesi, 2004), by aligning individuals perfectly by calender month of entry into unemployment.[81] The

[81]Note, that the propensity score specification includes indicators for the calendar month of unemployment entry

construction of counterfactual is hence done within *monthly* cells of both the unemployment entry and unemployment duration, whereby only controls receive weights that were unemployed at least until the month of program entry of the treated. The resulting estimator can be written as:

$$\tau^{IPW} = \frac{1}{N^1} \sum_{c=1}^{12} \sum_{p=1}^{12} \tau_{cp}^{IPW} \cdot N_{cp}^1 \tag{4.2}$$

where τ_{cp}^{IPW} is then estimated in each cell following Equation (4.1). N^1 denotes the total number of treated and N_{cp}^1 the number of treated in each cell defined by calendar month of unemployment entry c and the months in unemployment before treatment entry p. As the estimation of treatment effects within each cell yields 144 single effects τ_{cp}^{IPW}, with c denoting calendar month of entry into unemployment and p the month of entry into treatment, we aggregate the single effects to τ^{IPW}.[82] The aggregation is obtained by creating a weighted average of the monthly effects, with weights being determined by the distribution of monthly program starts and monthly unemployment entries among participants. See Section 4.7.1 in the Appendix for a more detailed description of perfect alignment.

4.4.3 Propensity Score Estimation and Weighting Implementation

Table 4.3 provides the number of observations for each of the three subgroups of treatment entry. It can be seen that treatment participation is strongly concentrated on the first quarter of unemployment duration—except for the case of JCS in the East, where youths are most likely to enter after six months in unemployment. It can also be seen that controls are highly likely to exit unemployment during the first quarter of their unemployment spell. In particular, we see a reduction of the control sample for about one quarter (one third) in the East (West) during the first three months in unemployment. Despite the reduction in sample sizes with increasing unemployment duration, each time window contains a sufficient number of treated and controls to obtain a meaningful estimate of the propensity score.

For each program we estimate three binary *probit* models on participation in the program vs. not participating in any program within each of the respective time

[82]Note that while treated are assigned to mutually exclusive cells defined by c_1 and p_1, they are opposed to non-treated with the same entry into unemployment $c_1 = c_0$ but $p_1 \leq p_0$.

Table 4.3: Timing of (Potential) Entry into Treatment, for Participants and Non-Participants

Entry	JS	STT	JWS	WS	JCS	FT	PT	NP
				East Germany				
1 – 3 months	758	516	609	299	202	181	257	12,119
(in %)	(56.36)	(52.71)	(61.45)	(68.11)	(29.71)	(44.25)	(50.39)	(100.00)
4 – 6 months	256	228	195	75	156	136	127	9,304
(in %)	(19.03)	(23.29)	(19.68)	(17.08)	(22.94)	(33.25)	(24.90)	(76.77)
7 – 12 months	331	235	187	65	322	92	126	8,444
(in %)	(24.61)	(24.00)	(18.87)	(14.81)	(47.35)	(22.49)	(24.71)	(69.68)
Total	1,345	979	991	439	680	409	510	
				West Germany				
1 – 3 months	1,059	1,049	311	322	283	289	588	26,410
(in %)	(55.30)	(55.65)	(49.52)	(64.14)	(49.65)	(56.12)	(58.10)	(100.00)
4 – 6 months	438	429	177	115	121	123	230	17,561
(in %)	(22.87)	(22.76)	(28.18)	(22.91)	(21.23)	(23.88)	(22.73)	(66.49)
7 – 12 months	418	407	140	65	166	103	194	14,874
(in %)	(21.83)	(21.59)	(22.29)	(12.95)	(29.12)	(20.00)	(19.17)	(56.32)
Total	1,915	1,885	628	502	570	515	1,012	

Note: Depicted are number of observations unless otherwise stated; calculations are based on the estimation sample. Non-participants are considered controls in the respective time window if they are observed unemployed at least until the first month of the time window. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training; NP - non-participants.

windows. The specification of the respective models was chosen as to include all covariates that potentially influence the selection into treatment and the success of the program. Table 4.4 contains a listing of the covariates used in our preferred specification. We include all variables that show up highly significant in at least one of the models. We only modify the estimation when there is a lack of variation between treated and controls in the respective time windows.[83] Given the differential characteristics of program participants, the sign and power of control variables in predicting treatment vary strongly across programs and entry time, in particular for the extensive set of information on past labor market history. Independent of program, the most important variables include schooling and vocational training information, calendar month of entry into treatment; potential entry in 2003; last contact to the employment agency; and the number of placement offers.[84] The latter two variables are of particular interest, as they proxy the closeness between youths and the employment agency and give potential signals for the labor market performance of youths as perceived by the caseworker. In particular, we observe a strong and significant inversely U-shaped relation between placement propositions

[83] We tested the sensitivity of our results by specifying more parsimonious models but found very little differences in the estimated effects.

[84] The predictive power of the respective models ranges closely around 70% for all models, see Table 4.10 in the Appendix. Full estimation results are available on request.

and treatment participation for all programs except PT, which means that youths with extremely low or high number of employment options are less likely to participate in ALMP.

Table 4.4: Set of Covariates Included in the Propensity Score Estimation

Information category	Specification details
Socio-demographic characteristics	Gender (dummy: Female) Age (dummy: below or above 20 years) Living situation: - living alone - living together married - living together not married Migration status (dummy) Having children (dummy)
Education level and health condition	School leaving certificate - none - lower secondary degree - middle secondary degree - upper/specialized secondary degree Having finished professional/vocational training (dummy) Health restrictions (dummy)
Information on last activity/employment	Last activity before entry into unemployment - regular employment - education, training, never employed - other Occupational group of previous job - agriculture - manufacturing, technical occupations - services - other Having professional experience (dummy) Daily income from last regular employment (log) Information available on working time at last employer (dummy)
Labor market history for past year and past three years	During the last year before unemployment entry (linear) - months spent in regular employment - months spent in unemployment - months spent in ALMP - months spent in inactivity - months spent in full-time employment(1) - months spent in part-time employment(1) During the last three years up to unemployment entry (linear) - months spent in regular employment - months spent in unemployment - months spent in ALMP - months spent in inactivity - months spent in full-time employment(1) - months spent in part-time employment(1) During the last three years up to unemployment entry (dummy) - never been in regular employment - never been in ALMP - never been in inactivity - never in full-time employment(1) - never in part-time employment(1)

Table to be continued.

Based on the predicted values of the propensity scores, weights are constructed within each of the 144 cells. To ensure that we only compare individuals with similar values of the PS and reduce the incidence of extreme values in the PS distri-

Table 4.4 continued.

Information category	Specification details
Information on current unemployment and caseworker information	Months of remaining benefit entitlement (linear) Quarter of entry into unemployment (4 dummies) Unemployment spell lasts until 2003 (dummy) Months since last contact to employment agency - never contacted before - less than six months - more than six months - information missing Information available on preferred working time (dummy) Number of placement propositions by caseworker (linear and squared)
Regional Characteristics	Unemployment rate (linear) GDP growth during last year (log)

Note: This baseline specification was modified if observations where dropped from the analysis due to lack of variation. In particular we dropped the variable "information of working time wanted" for the case of JCS, WS, PT and FT measures; information on previous employment occupation for PT and FT; the square of the placement proposition for WS and PT; and the information on migration status for FT.
(1) The information of working time available can be divided into three categories, full-time, part-time and "not quite full-time". The latter was dropped from the analysis.

bution we exclude observations outside the region of common support by dropping treated and non-treated individuals who have PS values above (below) the maximum (minimum) value of the respective other group (Dehejia and Wahba, 1999). This predominantly yields to a deletion of non-treated individuals at the lower end, and very few treated individuals at the upper end of the PS distribution (see Table 4.10 in the Appendix).[85] After imposing common support we perform weighting for all outcomes in each of the 60 months following program entry to obtain the short-, medium- and long-term treatment effects; standard errors are obtained by bootstrapping the entire matching procedure (including propensity score estimation) using 200 replications.

4.4.4 Balancing Tests

As the essential objective of IPW is to balance the distribution of observable characteristics between participants and non-participants, we test the success of the procedure by comparing the differences in the distributions of covariates of treated and weighted controls. Among the many approaches to do so, we choose a simple comparison of means (*t-test*), and the *mean standardized bias* (MSB) in the weighted sample.[86] The MSB is defined as the differences in covariate means as a percentage of the square root of the average sample variances of the treatment and control group, whereby it is generally assumed that a MSB below 5% reflects a well-

[85] We investigate the robustness of our results with respect to the choice of the common support and potential outliers in the sensitivity analysis in Section 4.5.3.

[86] See Caliendo and Kopeinig (2008) for a more detailed discussion of matching quality issues.

balanced covariate distribution in the sample. We control for 53 variables in our PS specification and find that around half of the variables are rejected to have equal means in a one-sided 5% significance t-test before weighting is conducted. After weighting, however, the same test finds for all programs that none of the variables has unequal means. Similarly encouraging results are obtained using the MSB as a criterion. Before weighting the MSB is around 20%, but afterwards it is below 3% for all programs and time windows in East Germany and below 2% in the West. Overall, this indicates that reweighting yields a control group that is very similar to the treatment group with respect to their observable characteristics at point of entry into treatment.[87]

4.5 Main Results and Sensitivity

4.5.1 Key Results

As our primary outcome of interest we consider the integration in unsubsidized regular employment.[88] Figures 4.3 (East Germany) and 4.4 (West Germany) plot the treatment effects on the employment probabilities during the 60 months following program entry. Monthly effects are calculated as the difference between treated and (weighted) control outcomes, which we also plot to facilitate interpretation. Additionally, we provide the cumulative effects of program participation after 30 and 60 months in Table 4.5. We focus on overall effects irrespective of timing of entry and address differences only if they are of interest.

The monthly outcome plots reveal that except for JCS and PT measures, all programs significantly improve the labor market prospects of participants. Following initial locking-in and transition phases, the treatment impact stabilizes for all programs at around two years after program entry. The long-run impact of program participation—after the third year of program entry and onwards—amounts to a monthly employment boost between 5 to 20 percentage points, depending on program and region. We see that WS and JWS are the most successful programs in East Germany in the long-run (i.e. at the end of our observation period) with an average impact of 20 to 25 percentage points. Similarly, JWS is the most successful

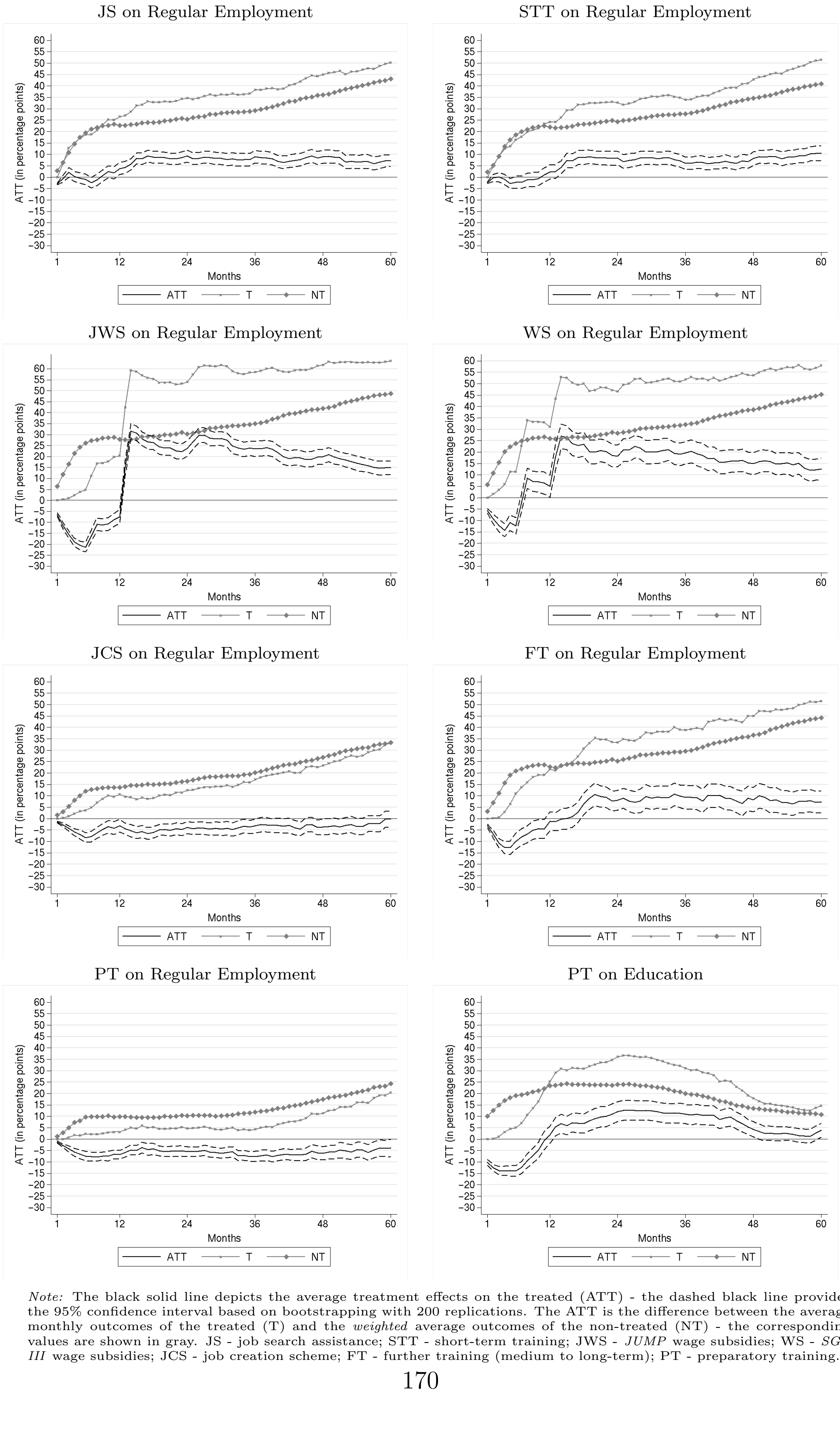
[87]See Tables 4.11 and 4.12 in the Appendix for the detailed results of the t-test and the MSB.

[88]We only consider employment subject to social security contributions as a success. This excludes "marginal employment", i.e. jobs that pay only up to 400 Euro and entail reduced social security contributions from the employer.

program in West Germany, with a 20 percentage point program impact, while here the effects of WS and FT are around 10 percentage points. The difference in relative impacts of wage subsidies and training measures in both regions seems to be in line with the notion, that West German program participants are more constraint by their adverse labor market characteristics than demand side restrictions. Hence, programs that aim at gradually enhancing labor market skills, i.e. long-term classroom training or long-term practical experience are more apt to overcome the entry barriers faced by West German youths.

The labor market integration of participants in wage subsidies (JWS and WS) takes place in discontinuous jumps, suggesting an immediate integration into the labor market. As firms were required to offer a minimal period of unsubsidized employment following the subsidy, this is driven by the continuation of the employment relationship within the same firm. Even though we see a small decline in the employment probabilities when the employment guarantees expire, the overall employment levels of the treated remain remarkably high (between 45% to 60%), such that wage subsidies can be seen as stepping stone into stable unsubsidized employment. In contrast to the immediate integration of participants in wage subsidies, participants in training measures (JS, STT and FT) experience a period of high intensity transitions into employment after the program has ended. This period lasts for about six to twelve months and can be seen as causal for the persistent employment gap between treated and non-treated individuals during the rest of the observation period. Training measures in the East perform similar independent of their duration—with a long-term employment impact of about 10 percentage points; whereas in the West short-term training (JS and STT) increases the employment probabilities of participants less than long-term training (FT). The effects for JS and STT have to be interpreted with caution, since a significant share of youths in the East (40%) and West (27%) participate in further ALMP programs. We address this issue in our sensitivity analysis in Section 4.5.3. In contrast to the previous programs, JCS and PT do not exhibit any positive long-term employment impact on program participants. In particular we find that participation in these programs decreases the probability of entering employment in the medium-run, even though the negative effect phases out to zero over the course of the observation period.

Figure 4.3: Causal Effects of Program Participation in East Germany Over Time—Aggregate Results Over All Program Entries



Note: The black solid line depicts the average treatment effects on the treated (ATT) - the dashed black line provides the 95% confidence interval based on bootstrapping with 200 replications. The ATT is the difference between the average monthly outcomes of the treated (T) and the *weighted* average outcomes of the non-treated (NT) - the corresponding values are shown in gray. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

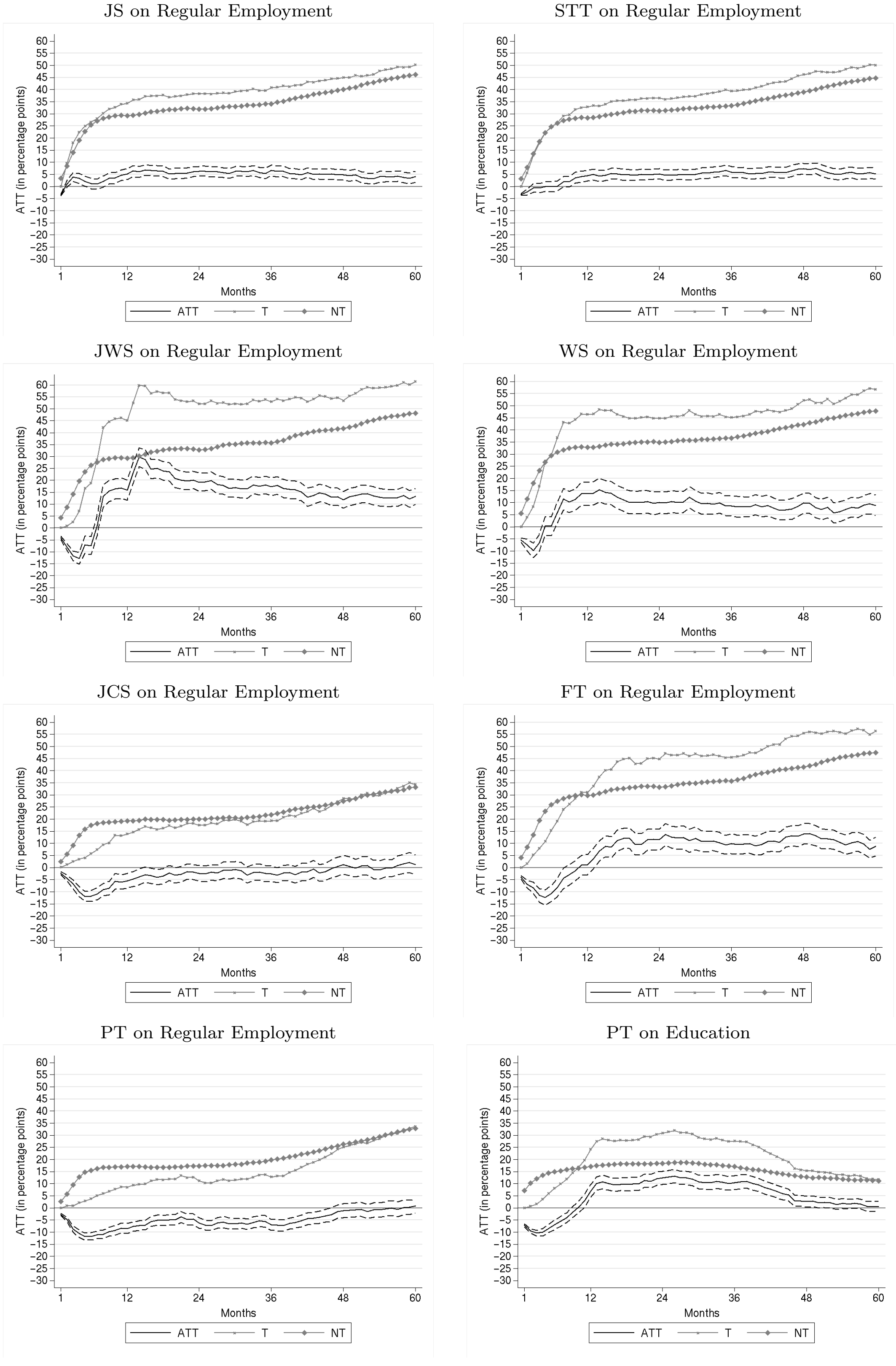
A further thing to note is that youths participating in longer-term measures experience severe locking-in effects during program participation—around 10 to 20 percentage points. If one interprets the level of locking-in during program participation as an initial investment, the cumulative benefit of program participation should be taken as measure for the net program effectiveness. The strength of locking-in depends on the opportunity costs of participation that are a function of, e.g., the program duration and the timing of entry into the program. Since non-participating youths experience particularly strong transitions out of unemployment during the first six months in their unemployment spell, this substantially aggravates the opportunity costs of entering the program during this phase. Table 4.5 presents the cumulative employment effects (30 and 60 months after program entry) overall and differentiated by entry strata.

Several issues emerge considering the employment outcomes: First, it can be seen that the relative cumulative long-run effectiveness of programs is largely consistent with the relative monthly long-run effectiveness. After 60 months, participants in wage subsidies yield the largest cumulative effects (up to nine months in East and five to nine months in West Germany). For the shorter programs JS and STT the cumulative effects are significantly positive between three and four months. For the longer FT measures, the effects are partly not significant after 30 months (due to long duration of the program), but turn positive after 60 months (3 and 4.5 months in East and West Germany). JCS and PT are the two programs with negative cumulative employment effects throughout. Second, we find that for almost all programs the cumulative effects are increasing with the timing of entry. In particular, we do not find significant differences in the monthly employment effects by entry time[89], so that the high opportunity costs of an early entry largely drive these results. Compared to individuals entering in the first three months of their unemployment spell, the locking-in costs are significantly reduced for later program entries. The largest differences across entry strata occur for JWS in the West, with a six-months cumulative gap for the earliest and the latest entries after 60 months.

Even though the integration into regular employment is the primary outcome of interest, we also test whether programs increase the participation in further unsubsized education or training, i.e., apprenticeships or higher secondary/tertiary schooling. As the administrative data only records apprenticeship participation

[89]Detailed monthly outcome plots by entry time into the program are available from the authors upon request.

Figure 4.4: Causal Effects of Program Participation in West Germany Over Time—Aggregate Results Over All Program Entries



Note: The black solid line depicts the average treatment effects on the treated (ATT) - the dashed black line provides the 95% confidence interval based on bootstrapping with 200 replications. The ATT is the difference between the average monthly outcomes of the treated (T) and the *weighted* average outcomes of the non-treated (NT) - the corresponding values are shown in gray. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Table 4.5: Cumulative Treatment Effects 30 and 60 Months After Program Entry on Regular Employment

		East Germany				West Germany			
			Entry strata				Entry strata		
		All	1-3	4-6	7-12	All	1-3	4-6	7-12
JS	Σ 30	**1.49**	**0.94**	**2.28**	**2.15**	**1.37**	0.48	**2.09**	**2.86**
	(s.e.)	(0.25)	(0.35)	(0.56)	(0.54)	(0.22)	(0.28)	(0.43)	(0.51)
	Σ 60	**3.81**	**3.33**	**5.35**	**3.74**	**2.85**	**1.41**	**3.54**	**5.76**
	(s.e.)	(0.54)	(0.72)	(1.18)	(1.13)	(0.42)	(0.56)	(0.83)	(0.99)
STT	Σ 30	**1.27**	0.61	**1.75**	**2.28**	**0.98**	0.02	**2.18**	**2.17**
	(s.e.)	(0.31)	(0.43)	(0.58)	(0.70)	(0.23)	(0.32)	(0.48)	(0.48)
	Σ 60	**3.65**	**2.82**	**4.86**	**4.28**	**2.75**	**1.86**	**4.69**	**3.00**
	(s.e.)	(0.57)	(0.82)	(1.16)	(1.34)	(0.45)	(0.61)	(0.88)	(1.03)
JWS	Σ 30	**3.10**	**1.60**	**5.47**	**5.51**	**4.16**	**2.34**	**4.86**	**7.28**
	(s.e.)	(0.31)	(0.38)	(0.62)	(0.73)	(0.38)	(0.50)	(0.57)	(0.80)
	Σ 60	**9.09**	**7.37**	**12.36**	**11.27**	**8.53**	**6.16**	**9.20**	**12.92**
	(s.e.)	(0.62)	(0.78)	(1.39)	(1.55)	(0.71)	(0.99)	(1.23)	(1.63)
WS	Σ 30	**3.53**	**2.94**	**5.55**	**3.89**	**2.42**	**1.80**	**3.22**	**4.11**
	(s.e.)	(0.49)	(0.56)	(1.08)	(1.17)	(0.47)	(0.53)	(0.87)	(1.46)
	Σ 60	**8.49**	**8.12**	**10.40**	**7.96**	**4.92**	**3.60**	**6.70**	**8.32**
	(s.e.)	(1.02)	(1.14)	(2.36)	(2.57)	(0.86)	(1.00)	(1.62)	(2.60)
JCS	Σ 30	**-1.47**	**-2.86**	**-1.01**	-0.81	**-1.38**	**-2.47**	-0.02	-0.52
	(s.e.)	(0.25)	(0.46)	(0.49)	(0.42)	(0.30)	(0.40)	(0.70)	(0.58)
	Σ 60	**-2.38**	**-3.76**	-1.12	**-2.13**	**-1.63**	**-2.59**	-0.47	-0.84
	(s.e.)	(0.56)	(1.01)	(1.07)	(0.84)	(0.64)	(0.95)	(1.50)	(1.21)
FT	Σ 30	0.27	**-1.79**	**1.81**	**2.09**	**1.23**	0.48	**2.35**	**2.00**
	(s.e.)	(0.44)	(0.61)	(0.71)	(1.01)	(0.44)	(0.58)	(0.85)	(0.90)
	Σ 60	**2.86**	-0.07	**5.15**	**5.28**	**4.47**	**3.61**	**6.03**	**5.04**
	((s.e.)	(0.98)	(1.35)	(1.53)	(2.17)	(0.83)	(1.09)	(1.69)	(2.01)
PT	Σ 30	**-1.64**	**-2.09**	**-0.87**	**-1.50**	**-2.14**	**-2.65**	**-0.99**	**-1.96**
	(s.e.)	(0.20)	(0.29)	(0.31)	(0.44)	(0.20)	(0.24)	(0.38)	(0.49)
	Σ 60	**-3.43**	**-4.13**	**-2.45**	**-3.01**	**-3.09**	**-3.98**	**-1.15**	**-2.69**
	(s.e.)	(0.43)	(0.59)	(0.70)	(0.93)	(0.42)	(0.51)	(0.86)	(0.95)

Note: Cumulative effects are obtained by summing up the monthly treatment effects. Standard errors in parentheses are obtained by bootstrapping with 200 replications. Bold numbers indicate significance at the 5% level. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

we use the filling procedure described already in Section 4.3.3 (further details in Section 4.7.1 in the Appendix). The monthly treatment effects on the probability to participate in unsubsidized education are depicted for participants in PT programs—predominantly aimed at integrating youths in unsubsidized education or professional training—in the lower right panel of Figures 4.3 and 4.4. For the other measures—which are aimed at integration into employment—the cumulative impacts on education participation are depicted in Table 4.6. It can be seen that PT measures do indeed significantly improve participation in education. After about one year after entry into the program, participants experience a stable positive increase in education probabilities of around 10 percentage points between month 12 to 48. Coinciding with the approximate three-year duration of an apprenticeship in Germany this is indicative of successful completion of a professional training.

Table 4.6: Cumulative Treatment Effect 30 and 60 Months After Program Entry on Education Participation

		East Germany Entry strata				West Germany Entry strata			
		All	1-3	4-6	7-12	All	1-3	4-6	7-12
JS	Σ 30	**-1.14**	**-1.02**	-0.62	**-1.82**	**-0.99**	**-0.55**	**-1.16**	**-1.93**
	(s.e.)	(0.14)	(0.16)	(0.39)	(0.31)	(0.14)	(0.19)	(0.27)	(0.28)
	Σ 60	**-1.64**	**-1.54**	-0.84	**-2.49**	**-1.4**	**-0.71**	**-1.61**	**-2.93**
	(s.e.)	(0.26)	(0.31)	(0.68)	(0.62)	(0.25)	(0.34)	(0.48)	(0.45)
STT	Σ 30	**-1.26**	**-0.76**	**-2.10**	**-1.56**	**-1.00**	**-0.73**	**-1.15**	**-1.55**
	(s.e.)	(0.19)	(0.28)	(0.30)	(0.40)	(0.15)	(0.21)	(0.30)	(0.33)
	Σ 60	**-1.54**	**-1.06**	**-2.64**	**-1.54**	**-1.31**	**-1.04**	**-1.65**	**-1.65**
	(s.e.)	(0.34)	(0.47)	(0.58)	(0.73)	(0.25)	(0.36)	(0.50)	(0.59)
JWS	Σ 30	**-2.49**	**-2.23**	**-2.77**	**-3.07**	**-2.20**	**-1.70**	**-2.43**	**-3.01**
	(s.e.)	(0.15)	(0.18)	(0.28)	(0.37)	(0.16)	(0.25)	(0.27)	(0.34)
	Σ 60	**-3.91**	**-3.48**	**-4.14**	**-5.08**	**-3.16**	**-2.14**	**-4.15**	**-4.17**
	(s.e.)	(0.27)	(0.35)	(0.64)	(0.54)	(0.32)	(0.52)	(0.48)	(0.77)
WS	Σ 30	**-2.32**	**-2.23**	**-3.18**	**-1.73**	**-1.34**	**-1.05**	**-2.01**	**-1.55**
	(s.e.)	(0.23)	(0.26)	(0.47)	(0.71)	(0.22)	(0.29)	(0.40)	(0.58)
	Σ 60	**-3.73**	**-3.98**	**-4.01**	-2.28	**-2.20**	**-1.84**	**-2.98**	**-2.57**
	(s.e.)	(0.40)	(0.45)	(0.94)	(1.18)	(0.40)	(0.52)	(0.81)	(0.89)
JCS	Σ 30	**-1.58**	**-1.30**	**-1.32**	**-1.88**	**-0.96**	-0.21	**-1.64**	**-1.75**
	(s.e.)	(0.22)	(0.37)	(0.38)	(0.36)	(0.28)	(0.42)	(0.52)	(0.43)
	Σ 60	**-1.82**	-1.26	**-1.77**	**-2.2**	-0.73	0.25	-1.19	**-2.06**
	(s.e.)	(0.43)	(0.71)	(0.79)	(0.66)	(0.54)	(0.80)	(1.13)	(0.80)
FT	Σ 30	**-1.85**	**-1.60**	**-2.12**	**-1.96**	**-1.79**	**-1.67**	**-1.94**	**-1.95**
	(s.e.)	(0.21)	(0.34)	(0.33)	(0.58)	(0.21)	(0.27)	(0.40)	(0.58)
	Σ 60	**-2.91**	**-2.87**	**-2.73**	**-3.25**	**-2.40**	**-2.19**	**-3.00**	**-2.26**
	(s.e.)	(0.43)	(0.65)	(0.69)	(0.98)	(0.43)	(0.51)	(0.75)	(1.07)
PT	Σ 30	0.65	0.82	-0.26	1.22	**1.47**	**2.17**	1.09	-0.23
	(s.e.)	(0.42)	(0.63)	(0.83)	(0.87)	(0.27)	(0.38)	(0.57)	(0.56)
	Σ 60	**2.67**	**3.01**	0.81	**3.88**	**3.14**	**4.40**	**2.42**	0.17
	(s.e)	(0.71)	(1.06)	(1.32)	(1.40)	(0.47)	(0.65)	(0.96)	(0.99)

Note: Cumulative effects are obtained by summing up the monthly treatment effects. Standard errors in parentheses are obtained by bootstrapping with 200 replications. Bold numbers indicate significance at the 5% level. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Further evidence for the education success of PT measures is given by a descriptive analysis of the share of youths having obtained a professional qualification until the end of our observation period (i.e. at most 72 months after initial unemployment entry). As Table 4.7 reveals, the share of youths with a professional qualification increases by 20% in the East and 17% in the West for participants - in contrast to 8% and 6% for non-participants. Again we find that the timing of program entry matters, as we observe an actual decline in effectiveness for later entries (see Table 4.6) in the West. Potentially driven by discouragement or rapid reduction in human capital for the rather young participants of PT, the fast integration into education seems to be crucial in order to avoid negative long-term effects of unemployment.

Table 4.6 also shows that none of the programs aimed at integrating youths into the first labor market have a positive impact on the education probabilities. The descriptive comparison of professional training shares at entry into unemployment and 72 months later shows that the average level of professional training did not increase strongly (about 3% on average) for youths who participated in employment programs. For East Germany this is not surprising as youths exhibited above average shares of professional training already at program start. In the West, however, about one third of participating youths still do not have any type of professional training at the end of our observation period. Again, youths participating in JCS fare much worse than the rest with about 40% (75%) of youths being without any professional degree after 72 months.

Table 4.7: Comparison of Participant and Non-Participant Highest Vocational Degree at Point of Entry into Unemployment (t=0) and 72 Months Later

		East Germany			West Germany		
		$t = 0$	$t = 72$	Δ	$t = 0$	$t = 72$	Δ
JS	None	0.23	0.18	**-0.05**	0.46	0.39	**-0.07**
	Apprenticeship	0.76	0.80	**0.04**	0.53	0.59	**0.06**
	University	0.00	0.02	**0.02**	0.01	0.02	**0.01**
STT	None	0.29	0.24	**-0.05**	0.48	0.41	**-0.07**
	Apprenticeship	0.70	0.73	**0.03**	0.50	0.56	**0.06**
	University	0.02	0.03	**0.01**	0.02	0.03	**0.01**
JWS	None	0.13	0.10	**-0.03**	0.35	0.32	**-0.03**
	Apprenticeship	0.85	0.88	**0.03**	0.64	0.67	**0.03**
	University	0.01	0.02	**0.01**	0.01	0.02	**0.01**
WS	None	0.22	0.18	**-0.04**	0.40	0.34	**-0.06**
	Apprenticeship	0.76	0.79	**0.03**	0.59	0.63	**0.04**
	University	0.02	0.03	**0.01**	0.01	0.03	**0.02**
JCS	None	0.47	0.39	**-0.08**	0.85	0.74	**-0.11**
	Apprenticeship	0.52	0.58	**0.06**	0.14	0.22	**0.08**
	University	0.01	0.03	**0.02**	0.01	0.04	**0.03**
FT	None	0.17	0.13	**-0.04**	0.36	0.32	**-0.04**
	Apprenticeship	0.83	0.86	**0.03**	0.62	0.65	**0.03**
	University	0.01	0.01	0.00	0.02	0.03	**0.01**
PT	None	0.89	0.68	**-0.21**	0.93	0.74	**-0.19**
	Apprenticeship	0.09	0.29	**0.20**	0.06	0.23	**0.17**
	University	0.02	0.03	**0.01**	0.01	0.03	**0.02**
NP	None	0.52	0.41	**-0.11**	0.55	0.48	**-0.07**
	Apprenticeship	0.46	0.54	**0.08**	0.43	0.49	**0.06**
	University	0.02	0.04	**0.02**	0.02	0.03	**0.01**

Note: Depicted are shares with a certain professional training (none, apprenticeship, university degree). Δ depicts raw differences between the two values; bold numbers indicate significance at the 5%-level from a one-sided t-test. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training; NP - non-participants.

4.5.2 Effect Heterogeneity

In this section we inspect effect heterogeneity across gender and pretreatment schooling levels (below/equal vs. above lower secondary schooling certificate). To account for potential differences in the timing and nature of selection into treatment and to ensure that we only compare treated and non-treated within the region of common support we repeat the estimation procedure outlined in Section 4.3 for each of the respective subgroups. This leaves us with 14 distinct program-subgroup cells in East and West Germany (compare Table 4.13 in the Appendix for details).[90] What should be kept in mind is that the separation of the analysis for the respective subgroups entails that the results are not directly comparable. For example, a higher level in the estimated effects for women does not indicate that the program is more beneficial for women than it is for men, but that women have a higher benefit compared to non-participating women than men have compared to non-participating men. In the Appendix we present selected monthly treatment effects estimates on the employment probabilities in Tables 4.14 (gender) and 4.16 (schooling levels); cumulative effects on employment and education outcomes can be found in Tables 4.15 and 4.17.

Effects by Gender: Our estimates reveal very minor differences in the monthly employment effects across gender. Only the long-run persistency of effects appears to differ for some programs. In East Germany we find for all programs except PT, that two to three years after program entry the average monthly treatment impact of women declines substantially and then stabilizes again at a lower (but positive) level towards the end of the observation period. In the West we find a similar, but less pronounced long-term reduction in treatment effects for female participants in STT, JS and WS. This is potentially explained by an increased labor force attachment among women with a successful program participation, who delay their timing of fertility in order to remain in the labor force (compare Lechner and Wiehler, 2011, for similar results on ALMP in Austria). Examples on short-to medium-run differences between young men and women occur for participants in WS, and training measures in the West. For the case of WS we find that after an initially similar program impact, the employment probabilities of men in East and women in the West decline substantially during the 12 months following program participation, while they remain stable for the other groups. These differences are most likely driven by differences in take-over probabilities of the firm receiving the subsidy, the cause of which would however require a more in-depth analysis of firm

[90] Due to the small number of observations within some cells, we modify the original PS specification on a case-by-case basis by successively excluding covariates with low explanatory value to obtain the optimal specification in terms of correct predictions rates. Full estimations results and further details are available upon request.

and participant characteristics. In the case of STT and FT measures in the West we find that women seem to benefit much less from STT measures than men (the cumulated effect only amounts to 1.5 months), but benefit more from longer-term training in FT. The latter finding is in line with the observation that young women generally perform better in school-based training than young men—a validation would require a direct comparison of the subgroups however.

Effects by Schooling Levels: Youths with different levels of pretreatment schooling have different returns to program participation. By and large these differences can be summarized into programs being more effective for high-skilled youths in terms of employment outcomes. In particular we find that participants in WS, JS, STT and FT with high levels of pretreatment schooling spend on average six months longer in employment than their non-treated counterparts over the whole observation period—compared to three months for youths with low schooling levels (Table 4.16). We also observe that the periods of locking-in go beyond the median program duration for youths with a low schooling degree, which would correspond to further program enrollment. In the case of a successful further participation, the true gap in program success for youths with low and high pretreatment schooling in the first program is expected to be even larger. An exception from these differential effects is given by JWS and JCS measures, which seem to be equally beneficial (detrimental) in terms of employment outcomes. The program effect of participation in JCS is either zero or slightly negative for both subgroups, while *all* youths participating in JWS have a cumulative employment gain of eight to ten months. As such the finding on JWS is an encouraging deviation from the our earlier findings as it is also driven by similar long-run effects, and not solely by the leveling of locking-in and program effects. In terms of education outcomes for participants in PT measures (last two rows of Table 4.16), we also observe that youths with higher schooling levels experience higher rates of education participation between month 12 to 36.

4.5.3 Sensitivity Analysis

We test the sensitivity of our results with respect to the crucial assumptions made in the main analysis. First, we consider the problem of further program participation and investigate to what extent our treatment estimates of the first participation in JS and STT measures are driven by participation in further measures. Second, we apply a dynamic evaluation approach that changes the composition of the control group. Finally, we check whether different variants of imposing common support alter our results. Table 4.18 presents the estimated cumulative employment effects from the sensitivity analysis together with the results obtained in the main analysis as a reference.[91]

Further Program Participation: We have noted in Section 4.5.1 that the effects for JS and STT have to be interpreted with caution, since a significant share of youths in participate in further ALMP programs. To be more specific, 44% (31%) of the JS participants in East (West) Germany participate in a further ALMP program within one year and the same is true for 38% (24%) of the participants in STT. As only individuals for whom the program did not lead to an entry into employment are assigned to further programs, the effectiveness of the initial measures would require the consideration of fully dynamic selection effects, which is beyond the scope of this chapter (see Lechner and Miquel, 2010, for an estimation approach). Instead we assess the sensitivity of our findings by restricting the sample of treated to individuals who participate in only one program during the first twelve months of their unemployment spell. This is insightful as it provides an indication whether any of the positive employment effects are attributable to participation in the initial program. As we exclude only youths for whom the program was unsuccessful, our sensitivity estimates are likely to be more positive than for the average participant. The results in Table 4.18 show that the new results are very similar to the results from the main analysis. To be on the safe side we repeated this exercise not only for participants in JS and STT but also for the other programs (where the probabilities of subsequent participation is much lower). The medium- and long-run cumulative effects are very similar to the reference estimates for all programs; none of the cumulative effects after 60 months in the sensitivity analysis differs significantly from the main results.

Dynamic Evaluation Approach: We assess the sensitivity of our results with respect to the choice of the evaluation approach and re-estimate our results using a dynamic approach, as outlined in Section 4.3.2. We hence redefine our control group to include youths who participate at any point in time later during their unemployment spell and who potentially participate in other programs. We find that the point estimates are slightly increased or reduced using the dynamic approach (compare Table 4.18), but none of these changes are significant at a conventional

[91] Results on education probabilities are not presented separately as their sensitivity is very similar to employment outcomes. But they are available upon request.

level. The observed increase in effects for the majority of programs is most likely due to controls entering other programs under investigation. As they experience periods of locking-in themselves, the opportunity cost of participating in the program of investigation is reduced. Given the large size of our never-treated control group, all of the observed changes are only minor and insignificant. We hence conclude that the choice of the evaluation approach has no significant implications for our results and using the dynamic approach does not change the overall evidence on program effectiveness.

Alternative imposition of Common Support: A necessary condition for the identification of treatment effects is the existence of corresponding non-participants over the whole support of the treated PS distribution, where limited overlap may be particularly distorting when using IPW (as pointed out by Frölich, 2004). We chose the "Min-Max"-condition in Section 4.4.3, but several alternatives have been suggested. Black and Smith (2004) argue that the imposition of a more restrictive trimming of the PS distribution might be beneficial if treated (controls) with very low (high) values of the PS are more likely to suffer from measurement error in the treatment variable, and remaining unobserved factors are more important here. To assess the sensitivity of our results with respect to this issue we conduct several robustness tests. First, we exclude control observations with very large values of the PS (above the 99 percentile). Second, we exclude areas of the distribution where there is only low overlap between treated and controls and restrict the common support to an "optimal" area defined by $\alpha \leq P(W) \leq 1 - \alpha$, whereby α is chosen to balance two opposing variance components (as suggested by Crump et al., 2009). While the variance of the estimate increases due to the lower number of observations, it decreases with an improved level of overlap between treated and non-treated.[92] Finally, we restrict the propensity score distribution even more, by dividing the distribution into twenty equidistant percentiles and estimate the effects only in regions where we have at least 5% of treated and non-treated observations. Clearly, restricting the estimation to areas of "thick support" reduces the validity of the results and might potentially lead to changes in estimated effects. This has the drawback that it is unclear whether changes are due to effect heterogeneity, large weights of outliers, or unobserved heterogeneity in characteristics. The results in Table 4.18 show that our effect estimates hardly change. This confirms our

[92] The implementation of this is done using the STATA tool *optselect.ado* provided by the Crump et al. (2009).

expectations discussed in Section 4.4.1, namely that due to a large sample of non-participants and a restrictive common support condition ("Min-Max" cut off rule) this issue is of minor relevance in our case.

4.6 Conclusion

Plagued with a persistent problem of long-term unemployment among youths, Germany is one of the European countries with the highest expenditures on youth ALMP—at 1.7 billion euros per year between 1999 and 2002. Between 2000 and 2010 about 1.4 million youths entries into ALMP were recorded each year—and the number is increasing. This evaluation study provides the first comprehensive assessment of the short-to-long-term employment impact of participation in various ALMP programs in place. Based on a representative sample on young unemployment entries in 2002, we investigate the effectiveness of program participation vs. non-participation using an quasi-experimental estimation approach with IPW. Analyzing a broad range of instruments that belong to the common set of policy tools employed in European countries, we add to the previous European evaluation literature dealing with youth ALMP. We conduct the analysis separately for youths in East and West Germany, shedding some light on the effectiveness of the respective measures to improve the employment situation of youths under differential social, economic and labor market conditions.

In terms of improving the employment probabilities of unemployed youths, the overall picture of the different ALMP analyzed is rather positive, indicating a persistent and stable employment effect. In particular, we find a significant increase in employment probabilities of participating youths for almost all measures examined. Focusing on the long-term employment impact, the strongest effects are observed for participants in wage subsidies (10 to 20 percentage points); job search assistance, short- and longer term training measures yield smaller but also persistently positive effects (5 to 10 percentage points). With respect to education outcomes we find that preparatory programs aimed at integrating youths into an apprenticeship are successful in doing so. In contrast to the aforementioned beneficial employment programs, public sector job creation schemes (JCS) are found to be harmful for the employment prospects of participants in the short- to medium-run and ineffective in the long-run. Put more drastically, if one considers the initial program participation as investment into future labor market outcome, the return of participating in JCS is negative throughout the whole observation period of five years. This is consistent with previous evaluation results for other countries that show the ineffectiveness of JCS for youths (compare, e.g., Dorsett, 2006, for the “environmental task force” implemented in the New Deal for Young People in the UK), and for the adult population (compare, e.g., Caliendo et al., 2008).

In terms of a differential impact of the respective measures under different labor market conditions, our analysis provides evidence from the comparison of the employment impact for program participants in East and West Germany. For all measures we find similar qualitative results, suggesting that the programs can be sufficiently adapted to benefit in either type of economic environment. However, we also find that the relative benefit of longer-term training measures (FT) compared to wage subsidies (WS) seems to be higher in the West than in the East, which needs to be interpreted with the significantly lower pretreatment education levels of West German youths in mind. While youths in the East are characterized by high initial schooling levels, the provision of work experience by removing demand-side barriers seems to be the most important hurdle to integrating into the labor market. In contrast, youths in the West have much less favorable labor market characteristics and hence seem to benefit more from an improvement in human capital endowment. Further evidence for this is given by our finding that only youths with high schooling levels in the West experience a positive long-term employment impact of participation in preparatory training. For youths in the East, the acquisition of a professional degree might not be sufficient to protect them from struggling at the “second barrier”.

We further find that all programs except JWS improve the labor market prospects of youths with high levels of pretreatment schooling to a greater extent than that of youths with low levels of pretreatment schooling. This suggests an insufficient adjustment of the respective measures for the requirements of unskilled youths. We further find that youths who are assigned to the most successful employment measures within the first twelve months in unemployment, compared to later- or never-participants, have much better characteristics in terms of their pretreatment employment chances. As the program assignment process is likely to favor individuals for whom the measures are most beneficial, the observed strong positive selection of youths into ALMP—in particular in the East—supports our interpretation of a systematic lack of ALMP alternatives that could benefit low-educated youths.

4.7 Appendix

4.7.1 Technical Appendix

Imputation of Missing Information

To overcome the potential problem of non-randomly missing outcome information, we impute missing spells with information that is recorded with every registered spell of unemployment, employment or benefit receipt. In particular, for each of these spells the main planned activity subsequent to the spell is available; for each registered spell of unemployment, additional information on the previous activity is recorded by the caseworker.

Table 4.8: Documentation of Filling Procedure

	Individuals		Months	
	N	%	N	%
Total	51,019	100	3,673,368	100
Affected by missings	36,493	71.53	942,564	25.66
Filled			866,707	23.59
participants			113,278	13.07
non-participants			753,429	86.93
Remaining missings	6,576	12.88	75,857	2.07
Filling details				
Participants				
% positive employment			21,430	19.30
% positive education			20,179	17.81
Non-participants				
% positive employment			145,454	18.92
% positive education			161,270	21.40

Source: Calculations are based on the estimation sample.

For example, if an individual's status of being registered as unemployed changes because he has to serve in the army (which was compulsory for men within our observation period), he disappears from the registered data. Military service is recorded as the reason for leaving the unemployment status and we fill the missing period with this information. If this individual once again registers as unemployed after having served in the army, this can be verified, as we again should observe the military service as the previous activity. However, we only observe the previous activity if the individual registers as unemployed. If he or she finds employment, we have to rely on the initial leaving information of the unemployment spell before military service. Table 4.8 summarizes the missing information that could be filled using this method.

From the distribution of missing information across program participants and non-participants we see that the administrative records contain significantly more missings for non-participants. This can be explained by a lower attachment of these individuals to the FEA and hence a lower contact frequency to the caseworker. However, we also find that the type of imputed information is similarly distributed across the two groups for both outcomes considered, so that non-randomly distributed missings should not pose a problem for our analysis.

Details on Perfect Alignment

The participants and non-participants are matched directly conditional on the calendar month of entry into unemployment and elapsed unemployment duration. As a starting point we estimate the average treatment effect on the treated for each cell, i.e., participants who entered unemployment in the c^{th} month of the year and have a program start after p months in unemployment are compared to non-participants who are also entered unemployment in the c^{th} month and are still unemployed after p months. Hence, within each cell defined by calendar month of unemployment entry and months elapsed before program entry, the effects are defined as:

$$\begin{aligned}\tau_{cp}^{IPW} &= E\left(Y^1 \mid D=1, P(W), \text{ UE-Entry} = c, \text{ Prg-Entry} = \text{p}\right) - \\ & \quad E\left(Y^0 \mid D=1, P(W), \text{ UE-Entry} = c, \text{ UE-Duration} \geq p\right)\end{aligned}$$

In a second step the single cell-effects are aggregated to obtain the aggregate effect τ^{IPW}. For this the 144 monthly effects τ_{cp}^{IPW} are weighted by the distribution of participants across cells:

$$\tau^{IPW} = \sum_{c=1}^{12} \left(\sum_{p=1}^{12} \tau_{cp}^{IPW} \cdot \frac{N_{cp}^1}{N_c^1} \right) \cdot \frac{N_c^1}{N^1},$$

with N_{cp}^1 denoting the number of treated observations within each cell defined by unemployment and treatment entry; N_c^1 denoting the number of treated by calendar month of unemployment entry, and N^1 denoting the total number of treated. After canceling N_c^1 out the total effect τ^{IPW} can be written as:

$$\tau^{IPW} = \frac{1}{N^1} \sum_{c=1}^{12} \sum_{p=1}^{12} \tau_{cp}^{IPW} \cdot N_{cp}^1.$$

4.7.2 Supplementary Tables

Table 4.9: Documentation of Sample Reduction

	Loss of Individuals	Number of Individuals
Total inflows into unemployment		851,258
Implemented restrictions		
Entries in 2002 only	607,702	243,556
Youth only (≤25 years)	187,898	55,658
Data cleaning[(1)]	913	54,745
Other programs[(2)]	2,960	51,797
Missing in any variables of the PS specification	778	51,019
Estimation sample		51,019
East Germany		17,515
Participants		5,353
Non-participants		12,162
West Germany		33,504
Participants		7,027
Non-participants		26,477

[(1)] We exclude individuals with missing information only (except an unemployment spell of a maximum of one week) and also individuals who die during our observation period.
[(2)] Individuals participating in different programs of ALMP to those under scrutiny (see Table 4.1) are excluded.

Table 4.10: Hit Rates of Predicted Propensity Scores and Number of Observations Deleted in the Min-Max Common Support (CS) for Each Entry Strata

	1-3 months			4-6 months			7-12 months		
	Hit Rate	CS NT	CS T	Hit Rate	CS NT	CS T	Hit Rate	CS NT	CS T
				East Germany					
JS	68%	762	0	71%	968	2	72%	1,179	0
STT	67%	511	0	68%	428	0	70%	1,364	1
JWS	68%	31	0	70%	1,745	0	74%	1,014	1
WS	62%	896	0	71%	2,637	0	77%	2,802	0
JCS	72%	107	0	71%	2,747	1	71%	411	5
FT	67%	2,292	0	74%	3,137	1	76%	2,663	0
PT	77%	2,873	0	75%	3,276	0	79%	2,815	0
				West Germany					
JS	65%	191	0	67%	2,296	0	69%	2,002	0
STT	64%	113	1	66%	44	0	68%	1,515	1
JWS	66%	1,701	0	71%	3,474	0	71%	199	0
WS	65%	692	0	70%	1,585	0	79%	8,057	0
JCS	74%	6,348	0	73%	3,159	0	73%	4,853	0
FT	64%	679	0	72%	4,260	0	73%	4,032	0
PT	73%	6,607	0	70%	299	0	74%	2,800	0

Note: The number of deleted observations for treated and controls are the sum of the respective upper and lower bound restrictions. *Hit rate:* Share of participants correctly predicted by the propensity score; *CS NT:* Number of non-treated individuals deleted due to the imposition of the common support condition. *CS T:* Number of treated individuals deleted due to the imposition of the common support condition. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Table 4.11: Balancing Quality of IPW in East Germany

		JS	STT	JWS	WS	FT	JCS	PT
		Entry strata: 1-3 months						
t-test of equal means								
1%-level	Unmatched	33	20	31	19	25	17	26
	Matched	0	0	0	0	0	0	0
5%-level	Unmatched	39	26	35	24	30	24	29
	Matched	0	0	0	0	0	0	0
10%-level	Unmatched	42	30	41	26	34	27	35
	Matched	0	0	0	0	0	1	0
Mean standardized bias								
	Unmatched	19.93	12.88	23.34	14.73	21.60	15.66	23.65
	Matched	0.85	1.22	1.06	0.83	1.53	1.59	1.76
		Entry strata: 4-6 months						
t-test of equal means								
1%-level	Unmatched	34	31	26	8	12	18	7
	Matched	0	0	0	0	0	0	0
5%-level	Unmatched	35	34	30	16	12	21	13
	Matched	0	0	0	0	0	0	0
10%-level	Unmatched	37	34	33	24	19	25	18
	Matched	0	0	0	0	0	1	0
Mean standardized bias								
	Unmatched	26.94	23.16	21.81	17.30	12.38	18.11	14.29
	Matched	1.17	0.97	1.05	1.27	1.31	1.61	2.00
		Entry strata: 7-12 months						
t-test of equal means								
1%-level	Unmatched	33	26	28	16	30	12	14
	Matched	0	0	0	0	0	0	0
5%-level	Unmatched	37	35	31	22	35	24	22
	Matched	0	0	0	0	0	0	0
10%-level	Unmatched	40	39	35	26	43	31	25
	Matched	0	0	0	0	0	1	0
Mean standardized bias								
	Unmatched	24.10	19.67	29.55	23.84	19.31	21.22	17.98
	Matched	1.58	1.36	1.48	2.81	1.35	2.67	2.18

Note: All variables that were used in the respective PS-specifications are included; the baseline specification contains 53 covariates in total. *t-test*: Depicted is the number of covariates which differ significantly between treated and controls at the respective significance level. The decision is based on a simple t-test of equal means. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Table 4.12: Balancing Quality of IPW in West Germany

		JS	STT	JWS	WS	FT	JCS	PT
		Entry strata: 1-3 months						
t-test of equal means								
1%-level	Unmatched	28	31	22	17	28	24	41
	Matched	0	0	0	0	0	0	0
5%-level	Unmatched	31	35	29	25	33	31	43
	Matched	0	0	0	0	0	0	0
5%-level	Unmatched	33	36	36	27	35	37	45
	Matched	0	0	0	0	0	0	0
Mean standardized bias								
	Unmatched	11.68	10.28	13.87	11.23	18.69	15.71	22.73
	Matched	0.52	1.01	0.64	0.60	1.60	1.08	1.13
		Entry strata: 4-6 months						
t-test of equal means								
1%-level	Unmatched	33	30	20	20	11	19	27
	Matched	0	0	0	0	0	0	0
5%-level	Unmatched	37	37	27	26	15	24	36
	Matched	0	0	0	0	0	0	0
10%-level	Unmatched	39	38	30	29	21	32	38
	Matched	0	0	0	0	0	1	1
Mean standardized bias								
	Unmatched	18.31	17.93	17.17	20.54	14.68	21.52	25.43
	Matched	0.71	0.60	1.05	1.23	1.81	1.78	1.15
		Entry strata: 7-12 months						
t-test of equal means								
1%-level	Unmatched	34	33	27	5	15	19	20
	Matched	0	0	0	0	0	0	0
5%-level	Unmatched	38	36	32	7	22	22	25
	Matched	0	0	0	0	0	0	0
10%-level	Unmatched	42	38	36	15	28	23	29
	Matched	0	0	0	0	0	1	0
Mean standardized bias								
	Unmatched	19.58	20.01	26.60	15.43	13.81	19.22	19.19
	Matched	0.93	0.61	1.75	2.63	1.00	1.79	1.36

Note: All variables that were used in the respective PS-specifications are included; the baseline specification contains 53 covariates in total. *t-test*: Depicted is the number of covariates which differ significantly between treated and controls at the respective significance level. The decision is based on a simple t-test of equal means. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Table 4.13: Number of Observations by Gender and Pre-Treatment Schooling Levels for Program Participants and Non-Participants

	By gender				By pre-treatment schooling level			
	East Germany		West Germany		East Germany		West Germany	
	M	W	M	W	Low	High	Low	High
JS	854	491	1,230	685	590	813	1,202	713
(%)	(63.49)	(36.51)	(64.23)	(35.77)	(42.05)	(57.95)	(62.77)	(37.23)
STT	564	415	1,165	720	354	654	1187	749
(%)	(57.61)	(42.39)	(61.80)	(38.20)	(35.12)	(64.88)	(61.31)	(38.69)
JWS	574	417	412	221	248	757	380	260
(%)	(57.92)	(42.08)	(65.09)	(34.91)	(24.68)	(75.32)	(59.38)	(40.63)
WS	262	177	320	182	134	313	324	190
(%)	(59.68)	(40.32)	(63.75)	(36.25)	(29.98)	(70.02)	(63.04)	(36.96)
JCS	473	207	400	170	416	268	500	79
(%)	(69.56)	(30.44)	(70.18)	(29.82)	(60.82)	(39.18)	(86.36)	(13.64)
FT	282	127	343	172	146	266	317	212
(%)	(68.95)	(31.05)	(66.60)	(33.40)	(35.44)	(64.56)	(59.92)	(40.08)
PT	301	209	627	385	319	194	766	253
(%)	(59.02)	(40.98)	(61.96)	(38.04)	(62.18)	(37.82)	(75.17)	(24.83)
NP	7,367	4,752	15,926	8,690	3,767	8,157	14,890	11,871
(%)	(60.79)	(39.21)	(64.70)	(35.30)	(31.59)	(68.41)	(55.64)	(44.36)

Note: Depicted are number of observations unless otherwise stated. Calculations are based on the estimation sample. Low levels of schooling indicate a lower secondary schooling degree levels or none; high levels of schooling indicate a medium or higher secondary schooling degree. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training; NP - non-participants.

Table 4.14: Treatment Effect Heterogeneity by Gender - Selected Monthly Effects on Regular Employment and Education Participation

		Effect in month ...						
		1	6	12	24	36	48	60
				East Germany				
Regular employment								
JS	Men	**-2.67**	-0.96	3.07	**9.05**	**10.52**	**11.44**	**9.12**
	(s.e.)	(0.25)	(1.50)	(1.65)	(1.88)	(1.80)	(1.70)	(1.85)
	Women	**-3.30**	-1.34	3.69	**8.81**	**5.33**	4.23	3.41
	(s.e.)	(0.38)	(2.06)	(2.13)	(2.28)	(2.26)	(2.32)	(2.39)
STT	Men	**-2.21**	-1.55	2.00	**7.93**	**6.40**	**10.80**	**11.41**
	(s.e.)	(0.21)	(1.79)	(2.00)	(2.13)	(2.01)	(2.09)	(2.11)
	Women	**-2.72**	**-4.33**	1.38	**7.84**	**5.10**	4.46	**9.03**
	(s.e.)	(0.37)	(2.13)	(2.27)	(2.36)	(2.51)	(2.35)	(2.41)
JWS	Men	**-6.31**	**-20.24**	**-7.11**	**21.27**	**20.98**	**20.81**	**15.58**
	(s.e.)	(0.55)	(1.24)	(1.99)	(2.09)	(2.43)	(2.27)	(2.28)
	Women	**-6.84**	**-24.13**	**-9.52**	**25.69**	**26.63**	**18.85**	**13.91**
	(s.e.)	(0.72)	(1.81)	(2.36)	(2.70)	(2.77)	(2.67)	(2.73)
WS	Men	**-4.87**	**-9.34**	**6.13**	**16.33**	**18.90**	**14.84**	**13.15**
	(s.e.)	(0.56)	(2.08)	(2.96)	(3.11)	(3.25)	(3.33)	(3.42)
	Women	**-6.55**	**-13.25**	4.59	**23.17**	**22.59**	**17.24**	**12.66**
	(s.e.)	(0.92)	(2.95)	(3.54)	(3.72)	(4.43)	(3.94)	(3.75)
JCS	Men	**-1.67**	**-8.69**	-2.95	-2.15	**-4.48**	-2.69	-0.13
	(s.e.)	(0.28)	(1.32)	(1.63)	(1.67)	(1.94)	(2.30)	(2.36)
	Women	**-1.26**	**-7.07**	-2.26	**-6.48**	0.55	-5.27	0.56
	(s.e.)	(0.44)	(2.06)	(2.59)	(2.31)	(3.33)	(3.08)	(3.26)
FT	Men	**-2.94**	**-10.04**	-0.08	**9.26**	**9.81**	**9.34**	**7.69**
	(s.e.)	(0.37)	(2.27)	(2.71)	(3.18)	(3.27)	(3.14)	(3.17)
	Women	**-3.73**	**-9.89**	-3.45	6.48	**10.50**	5.32	6.56
	(s.e.)	(0.84)	(3.28)	(4.36)	(4.49)	(4.61)	(4.61)	(4.78)
PT	Men	**-1.22**	**-7.63**	**-7.22**	**-5.06**	**-7.65**	**-6.11**	**-5.28**
	(s.e.)	(0.30)	(1.38)	(1.44)	(1.57)	(1.64)	(1.91)	(2.68)
	Women	**-1.26**	**-7.36**	**-6.69**	**-6.75**	**-8.10**	**-5.95**	-3.29
	(s.e.)	(0.50)	(2.26)	(1.87)	(1.97)	(2.01)	(2.56)	(2.93)
Education participation								
PT	Men	**-9.70**	**-14.40**	0.23	**11.47**	**10.06**	**7.93**	**4.99**
	(s.e.)	(0.95)	(1.66)	(2.80)	(3.06)	(2.99)	(2.62)	(2.39)
	Women	**-10.69**	**-12.95**	3.60	**12.36**	**11.48**	-2.62	1.42
	(s.e.)	(1.33)	(2.39)	(2.99)	(3.33)	(3.46)	(2.68)	(2.36)
				West Germany				
Regular employment								
JS	Men	**-3.03**	1.94	**6.44**	**5.89**	**7.06**	**6.41**	**3.93**
	(s.e.)	(0.19)	(1.24)	(1.33)	(1.35)	(1.38)	(1.37)	(1.41)
	Women	**-4.24**	-0.55	1.90	**7.14**	**5.75**	1.94	**3.89**
	(s.e.)	(0.36)	(1.95)	(1.78)	(2.03)	(1.96)	(1.89)	(1.80)
STT	Men	**-2.93**	0.42	**4.38**	**6.23**	**7.86**	**9.66**	**7.30**
	(s.e.)	(0.18)	(1.34)	(1.52)	(1.43)	(1.35)	(1.43)	(1.32)
	Women	**-3.86**	-1.26	**4.25**	**3.55**	3.04	**3.82**	2.35
	(s.e.)	(0.30)	(1.52)	(1.79)	(1.68)	(1.84)	(1.74)	(1.89)
JWS	Men	**-3.70**	**-5.89**	**14.46**	**19.57**	**19.48**	**13.10**	**14.43**
	(s.e.)	(0.44)	(1.97)	(2.46)	(2.60)	(2.29)	(2.54)	(2.23)
	Women	**-4.98**	**-6.60**	**22.09**	**21.28**	**15.23**	**11.81**	**13.50**
	(s.e.)	(0.64)	(3.00)	(3.52)	(3.35)	(3.14)	(3.33)	(3.14)

Table to be continued.

Table 4.14 continued.

		Effect in month ...						
		1	6	12	24	36	48	60
WS	Men	**-4.90**	2.00	**16.04**	**11.83**	**11.62**	**11.71**	**11.82**
	(s.e.)	(0.53)	(2.62)	(2.62)	(2.89)	(2.71)	(2.64)	(2.65)
	Women	**-6.29**	-3.00	**8.66**	6.11	1.70	5.22	1.64
	(s.e.)	(0.91)	(3.54)	(3.52)	(3.57)	(3.85)	(3.57)	(3.63)
JCS	Men	**-2.33**	**-9.86**	**-4.64**	-1.16	-1.21	4.46	2.53
	(s.e.)	(0.36)	(1.60)	(1.89)	(1.98)	(2.13)	(2.46)	(2.54)
	Women	**-2.51**	**-16.21**	**-6.79**	**-4.97**	-5.29	-6.18	-0.83
	(s.e.)	(0.56)	(1.95)	(2.79)	(2.50)	(2.84)	(3.25)	(3.41)
FT	Men	**-4.29**	**-12.67**	-0.44	**11.64**	**8.08**	**13.44**	**9.18**
	(s.e.)	(0.46)	(2.06)	(2.72)	(2.70)	(2.66)	(2.42)	(2.63)
	Women	**-3.82**	**-7.88**	3.61	**11.43**	**13.28**	**14.44**	6.19
	(s.e.)	(0.64)	(3.44)	(3.83)	(4.08)	(3.67)	(3.74)	(3.91)
PT	Men	**-2.93**	**-11.95**	**-8.63**	**-7.13**	**-7.88**	-2.03	-0.56
	(s.e.)	(0.31)	(1.06)	(1.36)	(1.44)	(1.60)	(2.03)	(1.96)
	Women	**-2.10**	**-11.75**	**-9.14**	**-4.97**	**-6.34**	-0.50	1.98
	(s.e.)	(0.29)	(1.78)	(1.86)	(1.86)	(1.71)	(2.15)	(2.41)
Education participation								
PT	Men	**-7.01**	**-6.19**	**6.88**	**13.61**	**11.88**	**3.98**	1.82
	(s.e.)	(0.50)	(1.17)	(1.75)	(1.83)	(1.81)	(1.66)	(1.36)
	Women	**-7.56**	**-7.94**	**7.46**	**11.11**	**8.77**	-0.07	-1.72
	(s.e.)	(0.70)	(1.70)	(2.41)	(2.29)	(1.95)	(1.76)	(1.66)

Note: Depicted are monthly ATT estimates on employment probabilities. The ATT are written in bold when they are significant at conventional 5%-level. Standard errors are obtained by bootstrapping with 200 replications and are depicted in parentheses. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Table 4.15: Treatment Effect Heterogeneity by Gender - Cumulated Effects After 30 and 60 Months

		East Germany				West Germany			
		Employment		Education		Employment		Education	
		Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60
JS	Men	**1.39**	**4.19**	**-0.91**	**-1.51**	**1.39**	**3.06**	**-1.01**	**-1.52**
	(s.e)	(0.35)	(0.68)	(0.19)	(0.36)	(0.26)	(0.54)	(0.17)	(0.30)
	Women	**1.44**	**2.74**	**-1.53**	**-1.85**	**1.16**	**2.15**	**-0.98**	**-1.21**
	(s.e)	(0.44)	(0.86)	(0.26)	(0.49)	(0.41)	(0.79)	(0.26)	(0.44)
STT	Men	**1.23**	**4.02**	**-1.29**	**-1.82**	**1.14**	**3.56**	**-1.14**	**-1.56**
	(s.e)	(0.40)	(0.81)	(0.23)	(0.41)	(0.29)	(0.55)	(0.18)	(0.31)
	Women	**1.00**	**2.71**	**-1.42**	**-1.53**	**0.67**	**1.42**	**-0.78**	-0.83
	(s.e)	(0.50)	(0.90)	(0.31)	(0.58)	(0.34)	(0.68)	(0.26)	(0.47)
JWS	Men	**2.70**	**8.49**	**-2.14**	**-3.63**	**4.13**	**8.96**	**-2.21**	**-3.42**
	(s.e)	(0.39)	(0.84)	(0.18)	(0.38)	(0.46)	(0.91)	(0.18)	(0.38)
	Women	**3.24**	**9.44**	**-2.65**	**-3.73**	**4.31**	**9.42**	**-2.32**	**-3.30**
	(s.e)	(0.48)	(0.96)	(0.25)	(0.44)	(0.62)	(1.30)	(0.34)	(0.66)
WS	Men	**3.42**	**8.28**	**-1.66**	**-3.11**	**2.89**	**6.03**	**-1.27**	**-2.32**
	(s.e)	(0.57)	(1.25)	(0.28)	(0.49)	(0.52)	(1.03)	(0.29)	(0.48)
	Women	**4.23**	**9.77**	**-3.09**	**-4.23**	**1.43**	2.28	**-1.42**	**-1.81**
	(s.e)	(0.66)	(1.46)	(0.40)	(0.70)	(0.76)	(1.46)	(0.45)	(0.88)
JCS	Men	**-1.36**	**-2.26**	**-1.36**	**-1.38**	**-0.99**	-0.57	**-0.81**	-0.66
	(s.e)	(0.30)	(0.73)	(0.28)	(0.52)	(0.34)	(0.78)	(0.29)	(0.56)
	Women	**-1.46**	**-2.16**	**-2.05**	**-2.87**	**-2.28**	**-3.86**	**-1.30**	-0.99
	(s.e)	(0.47)	(1.05)	(0.49)	(0.81)	(0.49)	(0.99)	(0.47)	(0.99)
FT	Men	0.64	**3.57**	**-1.91**	**-3.18**	0.84	**3.85**	**-1.85**	**-2.48**
	(s.e)	(0.56)	(1.18)	(0.30)	(0.55)	(0.50)	(0.94)	(0.27)	(0.52)
	Women	-0.26	1.66	**-2.29**	**-2.98**	**1.79**	**5.37**	**-2.28**	**-3.08**
	(s.e)	(0.85)	(1.78)	(0.41)	(0.72)	(0.80)	(1.57)	(0.39)	(0.74)
PT	Men	**-1.70**	**-3.42**	0.60	**2.85**	**-2.23**	**-3.55**	**1.65**	**3.74**
	(s.e)	(0.30)	(0.64)	(0.63)	(1.03)	(0.26)	(0.59)	(0.36)	(0.62)
	Women	**-1.66**	**-3.54**	0.62	**1.89**	**-2.06**	**-2.69**	**1.26**	**2.33**
	(s.e)	(0.41)	(0.79)	(0.64)	(1.06)	(0.38)	(0.75)	(0.50)	(0.76)

Note: Depicted are the cumulated treatment effects, summing up the monthly ATT between for 30 or 60 months following treatment entry. The effects are written in bold when they are significant at conventional 5%-level. Standard errors are obtained by bootstrapping with 200 replications and are depicted in parentheses. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Table 4.16: Treatment Effect Heterogeneity by Pretreatment Schooling - Selected Monthly Effects on Regular Employment and Education Participation

		Effect in month ...						
		1	6	12	24	36	48	60
				East Germany				
Regular employment								
JS	Low	**-2.31**	0.11	**4.47**	**7.01**	**9.26**	**6.91**	**5.34**
	(s.e.)	(0.29)	(1.69)	(1.90)	(1.96)	(1.97)	(1.98)	(2.05)
	High	**-3.37**	2.00	**6.78**	**14.38**	**11.99**	**13.02**	**9.58**
	(s.e.)	(0.27)	(1.42)	(1.78)	(1.98)	(1.96)	(1.94)	(1.93)
STT	Low	**-1.75**	-1.37	1.46	**7.38**	4.75	**6.40**	**11.84**
	(s.e.)	(0.23)	(1.61)	(1.92)	(2.43)	(2.55)	(2.65)	(2.87)
	High	**-2.66**	0.53	**5.90**	**12.37**	**10.17**	**12.20**	**10.68**
	(s.e.)	(0.24)	(1.65)	(1.90)	(2.05)	(2.12)	(2.11)	(2.03)
JWS	Low	**-5.22**	**-15.26**	0.46	**25.02**	**24.21**	**22.63**	**17.74**
	(s.e.)	(0.67)	(1.86)	(2.93)	(3.40)	(3.59)	(3.51)	(3.46)
	High	**-6.80**	**-19.95**	**-7.76**	**25.27**	**24.93**	**20.05**	**14.89**
	(s.e.)	(0.45)	(0.98)	(1.63)	(2.09)	(1.90)	(1.86)	(1.72)
WS	Low	**-3.18**	**-6.41**	5.53	**17.45**	**14.86**	6.66	7.96
	(s.e.)	(0.56)	(2.70)	(3.55)	(3.72)	(3.37)	(3.66)	(4.24)
	High	**-6.81**	**-11.26**	**7.98**	**20.09**	**22.46**	**18.94**	**15.37**
	(s.e.)	(0.61)	(2.03)	(2.81)	(2.70)	(2.79)	(2.88)	(2.77)
JCS	Low	**-1.23**	**-6.63**	**-3.67**	**-3.25**	**-3.97**	-2.97	-2.77
	(s.e.)	(0.23)	(1.00)	(1.44)	(1.51)	(1.80)	(2.22)	(2.24)
	High	**-1.55**	**-5.49**	1.42	-2.07	-0.38	-4.34	2.87
	(s.e.)	(0.25)	(1.54)	(2.39)	(2.23)	(2.66)	(2.69)	(2.98)
FT	Low	**-2.88**	**-7.08**	1.78	2.91	2.76	2.95	3.02
	(s.e.)	(0.56)	(2.28)	(3.88)	(4.08)	(4.17)	(3.85)	(4.03)
	High	**-2.98**	**-5.94**	0.74	**14.26**	**16.25**	**14.63**	**12.23**
	(s.e.)	(0.41)	(2.46)	(2.73)	(3.11)	(3.00	(3.14)	(3.10)
PT	Low	**-0.81**	**-4.19**	**-5.02**	**-4.17**	**-5.27**	**-4.71**	-3.34
	(s.e.)	(0.20)	(1.02)	(1.07)	(1.39)	(0.99)	(1.51)	(2.25)
	High	**-1.40**	**-6.53**	**-6.34**	**-6.20**	**-10.84**	**-7.28**	-4.37
	(s.e.)	(0.27)	(1.37)	(1.43)	(1.79)	(1.99)	(2.68)	(3.44)
Education participation								
PT	Low	**-7.83**	**-12.80**	**2.15**	**10.21**	**10.65**	**5.65**	3.07
	(s.e.)	(0.79)	(1.32)	(2.24)	(2.68)	(2.79)	(2.21)	(2.08)
	High	**-12.79**	**-14.72**	2.18	**15.17**	**12.13**	1.44	3.47
	(s.e.)	(1.21)	(2.67)	(3.82)	(3.52)	(3.18)	(2.94)	(2.88)
				West Germany				
Regular employment								
JS	Low	**-2.80**	0.75	**4.17**	**4.96**	**4.76**	**3.04**	**3.31**
	(s.e.)	(0.21)	(1.35)	(1.33)	(1.29)	(1.48)	(1.36)	(1.51)
	High	**-4.68**	1.62	**6.79**	**8.74**	**9.83**	**8.07**	**6.04**
	(s.e.)	(0.40)	(1.78)	(1.81)	(1.90)	(1.91)	(2.04)	(2.08)
STT	Low	**-2.70**	2.18	**6.21**	**6.15**	**6.70**	**9.07**	**7.13**
	(s.e.)	(0.16)	(1.15)	(1.36)	(1.31)	(1.46)	(1.47)	(1.48)
	High	**-3.75**	**4.17**	**8.50**	**10.04**	**9.19**	**8.34**	**5.54**
	(s.e.)	(0.28)	(1.71)	(1.70)	(1.72)	(1.70)	(1.81)	(1.65)
JWS	Low	**-3.37**	-2.32	**16.23**	**15.95**	**13.13**	**12.34**	**12.33**
	(s.e.)	(0.43)	(2.30)	(2.61)	(2.35)	(2.36)	(2.42)	(2.21)
	High	**-4.76**	**-9.10**	**20.40**	**27.27**	**25.83**	**12.89**	**15.39**
	(s.e.)	(0.71)	(2.59)	(3.25)	(3.31)	(3.02)	(3.13)	(3.07)

Table to be continued.

Table 4.16 continued.

		Effect in month ...						
		1	6	12	24	36	48	60
WS	Low	**-4.09**	2.33	**12.96**	**8.66**	**7.95**	**8.87**	**9.88**
	(s.e.)	(0.42)	(2.21)	(2.62)	(2.61)	(2.37)	(2.43)	(2.61)
	High	**-6.98**	5.47	**23.01**	**19.58**	**15.20**	**15.64**	**9.06**
	(s.e.)	(0.69)	(3.43)	(3.54)	(3.77)	(3.74)	(3.74)	(3.85)
JCS	Low	**-2.09**	**-9.17**	**-3.73**	-2.61	-1.90	2.53	1.99
	(s.e.)	(0.31)	(1.08)	(1.45)	(1.46)	(1.56)	(1.87)	(2.09)
	High	**-2.48**	**-7.57**	-3.05	5.30	-3.25	-5.14	-5.75
	(s.e.)	(0.58)	(2.89)	(4.23)	(5.39)	(4.80)	(6.02)	(6.35)
FT	Low	**-3.18**	**-7.14**	-0.19	**11.64**	**8.33**	**14.50**	**8.86**
	(s.e.)	(0.36)	(2.07)	(2.32)	(2.98)	(3.05)	(2.80)	(2.60)
	High	**-4.90**	**-11.55**	**8.42**	**16.64**	**14.48**	**15.27**	**8.80**
	(s.e.)	(0.65)	(2.52)	(3.23)	(3.50)	(3.47)	(3.46)	(3.60)
PT	Low	**-2.16**	**-8.94**	**-6.78**	**-4.49**	**-5.56**	-1.76	-0.37
	(s.e.)	(0.19)	(0.81)	(1.08)	(1.26)	(1.30)	(1.42)	(1.70)
	High	**-2.77**	**-11.32**	**-7.90**	**-7.86**	**-8.76**	0.28	3.66
	(s.e.)	(0.36)	(1.22)	(2.01)	(1.98)	(2.44)	(3.34)	(3.25)
Education participation								
PT	Low	**-6.04**	**-5.77**	**5.07**	**10.99**	9.98	**2.97**	1.99
	(s.e.)	(0.34)	(0.98)	(1.46)	(1.63)	(1.59)	(1.27)	(1.25)
	High	**-12.01**	**-11.44**	**12.02**	**16.57**	13.26	2.13	-2.94
	(s.e.)	(0.95)	(2.26)	(2.97)	(3.25)	(3.19)	(2.59)	(2.05)

Note: Depicted are monthly ATT estimates on employment probabilities. Low levels of schooling indicate a lower secondary schooling qualification or none; high levels of schooling indicate a medium or higher secondary schooling qualification. Depicted are the average treatment effects (ATT) on the employment probabilities in the months following treatment entry. The ATT are written in bold when they are significant at conventional 5%-level. Standard errors are obtained by bootstrapping with 200 replications and are depicted in parentheses. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training. training; NP: non-participants.

Table 4.17: Treatment Effect Heterogeneity by Pretreatment Schooling - Cumulated Effects After 30 and 60 Months

		East Germany				West Germany			
		Employment		Education		Employment		Education	
		Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60
JS	Low	**1.37**	**3.24**	**-0.57**	-0.71	**1.04**	**2.07**	**-0.73**	**-0.85**
	(s.e.)	(0.40)	(0.81)	(0.19)	(0.37)	(0.28)	(0.54)	(0.15)	(0.28)
	High	**2.58**	**5.90**	**-2.52**	**-3.61**	**1.92**	**4.22**	**-1.38**	**-2.27**
	(s.e.)	(0.36)	(0.73)	(0.21)	(0.37)	(0.38)	(0.79)	(0.26)	(0.41)
STT	Low	**1.24**	**3.24**	**-0.87**	**-1.26**	**1.36**	**3.59**	**-0.94**	**-1.07**
	(s.e.)	(0.41)	(0.89)	(0.30)	(0.50)	(0.25)	(0.52)	(0.16)	(0.29)
	High	**2.16**	**5.45**	**-2.61**	**-3.36**	**2.19**	**4.45**	**-2.18**	**-3.27**
	(s.e.)	(0.41)	(0.87)	(0.26)	(0.47)	(0.34)	(0.68)	(0.29)	(0.47)
JWS	Low	**4.00**	**10.49**	**-1.17**	**-1.68**	**3.83**	**8.01**	**-1.53**	**-2.53**
	(s.e.)	(0.56)	(1.26)	(0.23)	(0.40)	(0.44)	(0.87)	(0.19)	(0.40)
	High	**3.44**	**9.63**	**-3.54**	**-5.50**	**5.73**	**10.94**	**-3.84**	**-5.16**
	(s.e.)	(0.34)	(0.70)	(0.16)	(0.31)	(0.57)	(1.21)	(0.30)	(0.63)
WS	Low	**3.39**	**6.66**	**-1.38**	**-2.03**	**2.29**	**4.65**	**-1.14**	**-2.05**
	(s.e.)	(0.65)	(1.37)	(0.32)	(0.65)	(0.45)	(0.90)	(0.24)	(0.44)
	High	**4.22**	**10.09**	**-3.20**	**-5.15**	**4.85**	**8.83**	**-2.62**	**-3.99**
	(s.e.)	(0.52)	(1.06)	(0.28)	(0.45)	(0.79)	(1.52)	(0.48)	(0.82)
JCS	Low	**-1.35**	**-2.46**	**-1.40**	**-1.85**	**-0.99**	-1.00	**-0.98**	-0.90
	(s.e.)	(0.23)	(0.59)	(0.26)	(0.50)	(0.26)	(0.55)	(0.25)	(0.49)
	High	-0.74	-1.22	**-3.04**	**-3.62**	-0.47	-1.20	**-3.57**	**-3.59**
	(s.e.)	(0.43)	(0.96)	(0.39)	(0.75)	(0.83)	(1.99)	(0.95)	(1.65)
FT	Low	0.02	0.89	**-1.21**	**-1.98**	**1.11**	**4.16**	**-1.52**	**-2.01**
	(s.e.)	(0.65)	(1.39)	(0.30)	(0.51)	(0.51)	(1.07)	(0.22)	(0.50)
	High	**1.47**	**5.89**	**-3.15**	**-4.76**	**2.64**	**6.69**	**-2.75**	**-3.87**
	(s.e.)	(0.55)	(1.16)	(0.30)	(0.57)	(0.66)	(1.39)	(0.42)	(0.72)
PT	Low	**-1.10**	**-2.30**	0.72	**2.73**	**-1.61**	**-2.50**	**1.28**	**2.98**
	(s.e.)	(0.21)	(0.44)	(0.48)	(0.84)	(0.20)	(0.43)	(0.31)	(0.52)
	High	**-1.64**	**-4.20**	0.66	**2.60**	**-2.17**	**-3.07**	**1.86**	**3.69**
	(s.e.)	(0.31)	(0.72)	(0.77)	(1.11)	(0.38)	(0.89)	(0.65)	(1.06)

Note: Low levels of schooling indicate a lower secondary schooling qualification or none; high levels of schooling indicate a medium or higher secondary schooling qualification. Depicted are the cumulated treatment effects, summing up the monthly ATT between for 30 or 60 months following treatment entry. The effects are written in bold when they are significant at conventional 5%-level. Standard errors are obtained by bootstrapping with 200 replications and are depicted in parentheses.JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.

Table 4.18: Sensitivity of the ATT on Regular Employment

	JS		STT		JWS		WS		JCS		FT		PT	
	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60
							East Germany							
Results from the main analysis														
ATT	**1.49**	**3.81**	**1.27**	**3.65**	**3.10**	**9.09**	**3.53**	**8.49**	**-1.47**	**-2.38**	0.27	**2.86**	**-1.64**	**-3.43**
(s.e)	(0.25)	(0.54)	(0.31)	(0.57)	(0.31)	(0.62)	(0.49)	(1.02)	(0.25)	(0.56)	(0.44)	(0.98)	(0.20)	(0.43)
A) Further program participation														
ATT	**2.16**	**3.92**	**1.53**	**3.61**	**3.33**	**9.37**	**4.09**	**9.55**	**-1.37**	**-2.32**	0.43	**2.75**	**-1.57**	**-3.30**
(s.e)	(0.32)	(0.61)	(0.36)	(0.70)	(0.32)	(0.64)	(0.47)	(0.95)	(0.26)	(0.60)	(0.48)	(1.04)	(0.24)	(0.53)
B) Dynamic evaluation approach														
ATT	**1.70**	**3.95**	**1.48**	**3.81**	**3.31**	**9.09**	**3.78**	**8.57**	**-1.31**	**-2.22**	0.44	**2.91**	**-1.44**	**-3.19**
(s.e)	(0.23)	(0.52)	(0.27)	(0.56)	(0.27)	(0.57)	(0.44)	(0.88)	(0.24)	(0.54)	(0.41)	(0.90)	(0.20)	(0.46)
C) Alternative imposition of common support														
C1) ATT	**1.62**	**4.15**	**1.28**	**3.58**	**3.32**	**9.53**	**3.61**	**8.65**	**-1.61**	**-2.71**	0.32	**2.99**	**-1.63**	**-3.56**
(s.e)	(0.28)	(0.56)	(0.30)	(0.56)	(0.31)	(0.60)	(0.45)	(0.92)	(0.26)	(0.58)	(0.44)	(0.88)	(0.20)	(0.46)
C2) ATT	**1.19**	**3.42**	**1.03**	**3.54**	**2.66**	**8.52**	**3.46**	**8.75**	**-2.07**	**-3.40**	0.03	**2.56**	**-1.78**	**-3.63**
(s.e)	(0.28)	(0.59)	(0.37)	(0.73)	(0.37)	(0.69)	(0.55)	(1.17)	(0.30)	(0.64)	(0.53)	(1.19)	(0.30)	(0.63)
C3) ATT	**1.73**	**4.39**	**1.52**	**3.99**	**3.32**	**9.22**	**3.59**	**8.82**	**-1.49**	**-2.44**	0.26	**3.08**	**-1.63**	**-3.18**
(s.e)	(0.33)	(0.64)	(0.32)	(0.62)	(0.31)	(0.70)	(0.47)	(0.98)	(0.30)	(0.71)	(0.62)	(1.22)	(0.29)	(0.63)

Table to be continued.

Table 4.18 continued.

	JS		STT		JWS		WS		JCS		FT		PT	
	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60	Σ 30	Σ 60
							West Germany							
Results from the main analysis														
ATT	**1.37**	**2.85**	**0.98**	**2.75**	**4.16**	**8.53**	**2.42**	**4.92**	**-1.38**	**-1.63**	**1.23**	**4.47**	**-2.14**	**-3.09**
(s.e.)	(0.22)	(0.42)	(0.23)	(0.45)	(0.38)	(0.71)	(0.47)	(0.86)	(0.30)	(0.64)	(0.44)	(0.83)	(0.20)	(0.42)
A) Further program participation														
ATT	**2.43**	**4.29**	**1.57**	**3.22**	**4.49**	**9.09**	**2.97**	**5.33**	**-1.15**	-1.13	**1.32**	**4.65**	**-2.07**	**-2.90**
(s.e.)	(0.25)	(0.49)	(0.26)	(0.48)	(0.36)	(0.72)	(0.50)	(0.98)	(0.31)	(0.66)	(0.41)	(0.84)	(0.21)	(0.47)
B) Dynamic evaluation approach														
ATT	**1.52**	**2.95**	**1.13**	**2.91**	**4.16**	**8.44**	**2.50**	**4.93**	**-1.20**	**-1.34**	**1.28**	**4.46**	**-1.92**	**-2.85**
(s.e.)	(0.21)	(0.42)	(0.22)	(0.42)	(0.32)	(0.61)	(0.43)	(0.86)	(0.30)	(0.60)	(0.41)	(0.86)	(0.18)	(0.41)
C) Alternative imposition of common support														
C1) ATT	**1.44**	**2.95**	**1.02**	**2.78**	**4.24**	**8.68**	**2.49**	**5.00**	**-1.50**	**-1.93**	**1.29**	**4.61**	**-2.17**	**-3.17**
(s.e.)	(0.21)	(0.42)	(0.21)	(0.43)	(0.36)	(0.69)	(0.44)	(0.84)	(0.28)	(0.62)	(0.44)	(0.85)	(0.20)	(0.46)
C2) ATT	**1.09**	**2.43**	**0.70**	**2.39**	**3.76**	**8.25**	**1.83**	**3.87**	**-1.79**	**-2.23**	0.83	**4.04**	**-2.13**	**-3.06**
(s.e.)	(0.28)	(0.54)	(0.27)	(0.53)	(0.42)	(0.82)	(0.52)	(0.98)	(0.36)	(0.78)	(0.48)	(0.98)	(0.25)	(0.51)
C3) ATT	**1.78**	**3.49**	**1.27**	**3.43**	**4.20**	**8.60**	**2.72**	**5.27**	**-1.15**	**-1.40**	**1.37**	**4.68**	**-1.96**	**-2.82**
(s.e.)	(0.30)	(0.55)	(0.25)	(0.48)	(0.35)	(0.73)	(0.44)	(0.86)	(0.35)	(0.81)	(0.45)	(0.89)	(0.23)	(0.53)

Note: The cumulative effects are obtained by summing up the monthly program effects over a period of 30 or 60 months after program entry. Standard errors in parentheses are obtained by bootstrapping the estimation procedure with 200 replications. Bold numbers indicate significance at the 5% level. The results from the main analysis are the aggregate cumulative effects from Table 4.5. JS - job search assistance; STT - short-term training; JWS - *JUMP* wage subsidies; WS - *SGB III* wage subsidies; JCS - job creation scheme; FT - further training (medium to long-term); PT - preparatory training.
Sensitivity A) refers to the exclusion of further program participants within one year of unemployment duration.
Sensitivity B) refers to the extension of the control group to all future program participants and other program participants.
Sensitivity C) refers to modifications in the PS distribution that is used to weigh the nonparticipant outcomes. We estimate the effects in C1) by excluding non-participants with PS-values above the 99th percentile. In C2) we only include participants and non-participants in the analysis within the optimal region of common support: $\alpha < P(W) < (1-\alpha)$ as suggested by Crump et al. (2009). For C3) we divide the PS-distribution in 20 equidistant percentiles, and only estimate the ATT in regions where the density is above 5% ($F(P(W) > 5\%)$) in both groups.

5 Final Conclusion

This chapter provides an overall summary of the results. It reviews the addressed research questions and summarizes the main findings of each chapter. Policy conclusions are discussed and limitations of the analysis are highlighted.

This book extends the existing evidence on ALMP program evaluation in three directions: First, the promotion of self-employment among the unemployed, a relatively recent ALMP program type, is evaluated. Second, the impact of being marginally employed (and therefore having additional earnings) during unemployment on unemployment duration and subsequent job quality is analyzed. And finally, the impact of participation in ALMP programs for unemployed youth, a subgroup of the labor market that is of high interest but often neglected in existing evaluation studies, is evaluated.

Chapter 2 investigates the impact of two distinct subsidy programs in Germany designed to turn unemployment into self-employment. The programs differ in their design and attract different types of individuals. Based on an unique dataset combining administrative with survey data, we are able to add three substantial aspects to the previous literature: First of all, we observe individuals for almost five years following start-up, so that we are able to provide missing evidence on the long-term impact of these programs. In particular, we find that both programs are effective in improving the employment and income situation of participants compared to non-participants in the long-run. Second, we consider effect heterogeneity with respect to education, professional qualification, age and nationality of participants and with respect to local economic conditions at start-up. The results indicate that both subsidy programs are particularly effective for disadvantaged groups in the labor market like low educated or low qualified individuals and in regions with unfavorable economic conditions. This suggests that the promotion of self-employment among the unemployed is a sensible strategy to fight long-term unemployment, social exclusion and therefore poverty. Third, we provide empirical evidence on the effectiveness of start-up programs with respect to unemployed women. Due to higher preferences for flexible working hours and limited part-time jobs, unemployed women often face limited opportunities in the labor market. Traditional ALMP programs primarily focus on the integration in dependent employment where flexible working schemes are limited. Therefore, existing evidence shows an increase in labor market attachment for female participants, however, also a reduction in fertility which is from a societal perspective worrisome. In this context, we find that start-up subsidy programs are more promising as unemployed women become self-employed which gives them more flexibility to reconcile work and family. In fact, we find that start-up programs persistently integrate former unemployed women into the labor market and partly improve their income situations, while the impact on fertility is less detrimental than for traditional programs of ALMP. This suggests that policy makers should simplify access to such programs, in particular for unemployed women. In August 2006 the two programs were replaced by one single program, the so-called "new start-up subsidy", for which unemployment benefit entitlement is a mandatory eligibility criterion. The additional restriction is likely to be less binding for the participation decision of men as men have on average higher labor market participation rates and therefore are more likely to receive unemployment benefits. For women, however, the restriction might be detrimental in the sense that it impedes labor force participation for some women. Against the background of our findings, policy makers should re-think recent reforms of those programs as women particularly gained from the simplified access to the start-up subsidy introduced in 2003.

However, we also want to point out potential limitations of the results in Chapter 2 and outline further research needed. First of all, it needs to be emphasized that our partial-equilibrium analysis focusses solely on the effects for participating individuals while any macroeconomic or general equilibrium impacts, e.g. substitution effects and crowding-out, are not considered. Hence, our positive findings (on an individual level) need to be verified on a macroeconomic level in order to judge the scope of the programs to generate any positive macro effects. Second, our estimation approach does not allow us to identify deadweight losses. The definition and identification of a deadweight loss in the context of start-up subsidies is—compared to other labor market policies such as wage subsidies—not straightforward. If an employer hires an unemployed person whose wage is subsidized but would have hired this unemployed anyway, we talk about a deadweight loss. In the context of the start-up subsidies this translates into the question whether the individuals would have founded the business even without a subsidy, and whether their success (or failure) would have had the same probabilities with and without the subsidy. Even if we know that people would have started without the subsidy, we are not able to answer the question whether the businesses would have been equally successful. A possible solution would be to compare subsidized start-ups out of unemployment with other start-ups. To do so, we need information on "regular" start-ups (unsubsidized, out of employment). We did not address the open research questions here due to data availability.

Chapter 3 considers the impact of being marginally employed (and therefore having additional earnings) during unemployment on labor market outcomes of the unemployed. Additional earnings during unemployment are expected to lead to higher reservation wages, thereby prolonging the duration of unemployment, but also giving unemployed individuals more time to search for better and more stable jobs. Furthermore, marginal employment might lower human capital deterioration and raise the job arrival rate due to an improved network. Its impact on unemployment duration and subsequent job quality is therefore ambiguous from a theoretical perspective and empirical evidence is needed. Using a random sample of male unemployment entries in West Germany, Chapter 3 analyzes the causal impact of entering marginal employment on unemployment duration and job match quality. The main finding is that being marginally employed has no significant impact on the job finding probability for the first 12 months of unemployment, however, significantly increases after one year. The impact on job stability is also stronger for long-term unemployed individuals. With respect to wages, we do not find any evidence for an interaction effect between taking up marginal employment and elapsed unemployment duration. Our descriptive analysis suggests that the positive impact on unemployment exit and employment stability especially for longer unemployed workers is probably not driven by an increasing role of mini-jobs as a probation period. It appears more plausible that mechanisms that are more relevant at later stages of an unemployment spell, like the deterioration of human capital and changing networks due to changing contact frequency with colleagues, could drive these effects. As the analysis is based on administrative data, we do not have information about the search behavior of unemployed individuals with and without mini-jobs, nor on the changes in human capital over time. This makes an identification of underlying mechanisms difficult. Future research should shed more light on the underlying mechanisms which might explain the positive effect of entering marginal employment on the employment outcomes.

In summary our results suggest that – at least at the individual level – mini-jobs can be an effective instrument to help long-term unemployed individuals to find (stable) jobs. This is in particularly interesting given the persistently high shares of long-term unemployed individuals in industrialized countries as depicted in Figure 1.2 in Chapter 1 for the EU 15 countries. Among the clear advantages of marginal employment are the lack of direct program costs and the low administrative burden as unemployed workers are searching for mini-jobs on their own. The findings are also highly relevant for the design and the timing of active labor market programs. As it is found that labor market attachment and hence capital deterioration and network effects become more important with increasing unemployment duration, the long-term unemployed should be primarily assigned to ALMP programs that have a strong link to the labor market, such as integration subsidies. Future research should shed some light on the interaction of marginal employment and measures such as job search assistance and training programs for unemployed workers.

Chapter 4 focusses on ALMP for unemployed youth, a population at risk in the labor market due to their lower search skills and little work experience compared to adults. By their higher unemployment rates they are also much more often participants in ALMP, albeit only little is known about the effectiveness of ALMP programs for youth. For Germany no comprehensive assessment exists so far although Germany has one of the highest expenditures on youth ALMP in Europe. Based on a representative sample on young unemployment entries in 2002 in Germany, we investigate the effectiveness of several ALMP programs. We are able to follow individuals for six years after unemployment entry and compare participants of seven programs such as short-term training, job creation schemes or wage subsidies with non-participants in terms of post-treatment employment probabilities and education outcomes. An interesting extension of the analysis would be the consideration of subsequent job quality, expressed by wages for instance. However, with the data at hand we only observe daily income without having any information about working time which makes a reliable evaluation of the impact on wages impossible and will be therefore left for future research.

The analysis in Chapter 4 shows an overall positive picture with respect to post-treatment employment probabilities. After initial locking-in effects, we find a significant increase in employment probabilities of participating youths which is persistent over time for the majority of measures examined. Focusing on the long-term employment impact, the strongest effects are observed for participants in wage subsidies, followed by job search assistance and short- and longer term training measures. Public sector job creation schemes in contrast are found to be harmful for the employment prospects of participants in the short- to medium-run and ineffective in the long-run. Put more drastically, if one considers the initial program participation as investment into future labor market outcome, the return of participating in JCS is negative throughout the whole observation period of five years. Against this background it is surprising that during the current economic crisis policy makers still consider the temporary extension of these measure to counteract soaring levels of youth (long-term) unemployment rates (compare OECD, 2011). With respect to education outcomes we find that preparatory programs aimed at integrating youths into an apprenticeship are successful in doing so, but exhibit no long-term positive effects on employment outcomes.

With respect to effect heterogeneity, two interesting observations emerge: First, the relative benefit of longer-term training measures compared to wage subsidies is higher in West than East Germany. While youths in the East are characterized by high initial schooling levels, the provision of work experience by removing demand-side barriers seems to be the most important hurdle to integrating into the labor market. In contrast, unemployed youths in the West have much less favorable labor market characteristics and hence seem to benefit more from an improvement in human capital endowment. Second, we find that all programs except the JUMP wage subsidy particularly improve the labor market prospects of youths with high levels of pretreatment schooling. Furthermore, we find that youths who are assigned to the most successful employment measures have much better characteristics in terms of their pre-treatment employment chances compared to non-participants. Therefore, the program assignment process seems to favor individuals for whom the measures are most beneficial. This observed strong positive selection of youths into ALMP—in particular in the East—supports our interpretation of a systematic lack of ALMP alternatives that could benefit low-educated youths. Recent statistics on youth unemployment levels in Germany (and similarly in other European countries) show though that the probability to enter unemployment is significantly higher for low-educated than medium-educated youths, with a steadily increasing gap. Together with the expected shortage of labor in the medium-run the by far most vulnerable labor market group will be low-educated youths, making them the most important target of policy intervention. Our analysis provides evidence however, that the these youths are not sufficiently accommodated in the current policy set-up.

Therefore, the analysis indicates potential avenues for the improvement of ALMP for low educated youths. So far, none of the programs aimed at labor market integration increases the education participation of youths. By readjusting existing labor market programs to accommodate participation in further education or training as intermediate objective, the integration of low-educated youths into the labor market could be done in a more sustainable manner. Secondly, the analysis finds that wage subsidies of shorter duration work better for high-schooling youths, while wage subsidies with longer duration work equally well for low and high educated youths. This suggests that low educated youths require more time to turn the subsidized work experience into a stepping stone to a stable employment entry.

By extending the access to longer-term professional experience for these youths, an additional barrier of labor market integration for these could potentially be removed.

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Abbreviations

AIC	Akaike Information Criterion
ALMP	Active Labor Market Policy
ATT	Average Treatment Effect on the Treated
BA	Bridging Allowance
BIC	Bayesian Information Criterion
CIA	Conditional Independence Assumption
DID	Difference-in-Differences
EU	European Union
FEA	Federal Employment Agency
FT	Further Training
GDP	Gross Domestic Product
IAB	Institute for Employment Research
IEB	Integrated Employment Biographies
IPW	Inverse Probability Weighting
JCS	Job Creation Schemes
JS	Job Search
JUMP	Immediate Action Program for Lowering Youth Unemployment
JWS	JUMP Wage Subsidy
MAT	Matching
ME	Marginal Employment
MSB	Mean Standardized Bias
NP	Non-Participants
OECD	Organisation for Economic Co-operation and Development
OSATE	Optimal Subpopulation Average Treatment Effect
PS	Propensity Score
PT	Preparatory Training
RE	Regular Employment
SGB III	Social Act III
SOEP	German Socio-Economic Panel
SSC	Social Security Contributions

STT	Short-term Training
SUS	Start-up Subsidy
SUTVA	Stable Unit Treatment Value Assumption
UE	Unemployment
UI	Unemployment Insurance
WS	Wage Subsidy

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