Essays on
Job Search Behavior and Labor Market Policies

The Role of Subjective Beliefs, Geographical Mobility and Gender Differences

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Contents

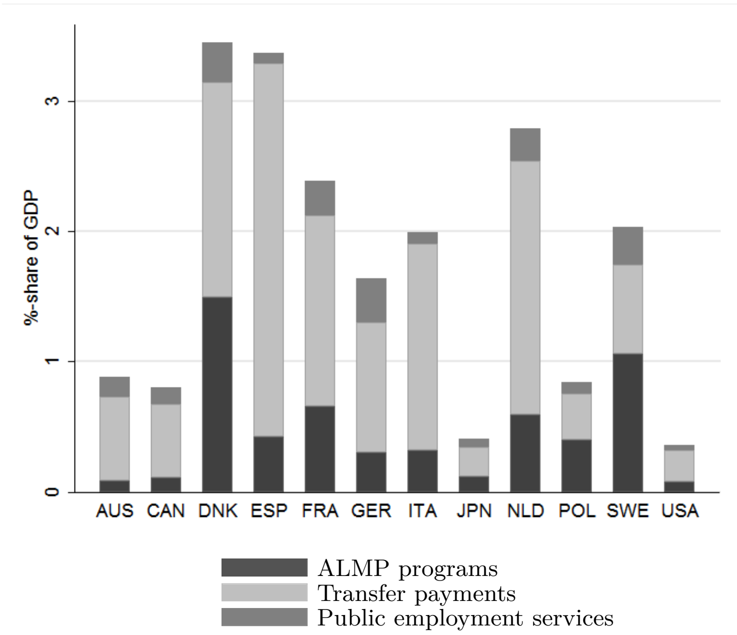
1 Introduction

Persistently high unemployment rates are a major threat to the social cohesion in many societies. To moderate the consequences of unemployment industrialized countries spend substantial shares of their GDP on labor market policies, while in recent years there has been a shift from passive measures, such as transfer payments, towards more activating elements which aim to promote the reintegration into the labor market. Although, there exists a wide range of evidence about the effects of traditional active labor market policies (ALMP) on participants' subsequent labor market outcomes, a deeper understanding of the impact of these programs on the job search behavior and the interplay with long-term labor market outcomes is necessary in order to improve the design of labor market policy schemes and the allocation of unemployed workers into specific programs. Moreover, previous studies have shown that many traditional ALMP programs, like public employment or training schemes, do not achieve the desired results which underlines the importance of understanding the effect mechanisms, but also the development of innovative programs that are more effective. This thesis extends the existing literature with respect to several dimensions. First, it analyzes the impact of job seekers' subjective beliefs about upcoming ALMPs programs on the effectiveness of realized treatments later during the unemployment spell. This provides important insights with respect to the job search process and relates potential anticipation effects (on the job seekers behavior before entering a program) to the vast literature evaluating the impact of participating in an ALMP program on subsequent labor market outcomes. Second, the thesis investigates the effects of a relatively new class of programs that aim to improve the geographical mobility of unemployed workers with respect to the job search behavior, the subsequent job finding prospects and the returns to labor market mobility. Third, it offers an empirical assessment with respect to the relevance of variables which have not been considered in previous evaluation studies. Finally, the thesis also examines the importance of gender differences in reservation wages which allows to assess the importance of special ALMP programs targeting women.

1.1 Motivation and Background

Unemployment represents one of the major challenges in modern welfare states. In 2013 OECD countries spent on average about 1.4% of their GDP on labor market policies dealing with the immediate consequences of unemployment, while actual strategies to moderate these consequences and to improve reemployment prospects of unemployed workers differ substantially across countries. Figure 1.1 shows that European countries typically spend substantially higher shares of the total GDP on labor market policies compared, to the US, Canada or Australia. Moreover, there are also substantial differences even within Europe which becomes clear when distinguishing between passive measures and activating elements. Although, transfer payments, such as unemployment benefits, typically account for the highest share of public spending, many European countries also spend large amounts of money on active labor market policies (ALMP) that aim to promote the reintegration of unemployed workers into the labor market.

Figure 1.1: Expenditures on Labor Market Policies Across Countries



Note: Depicted are national expenditures on three types of labor market policies in 2013 for selected OECD countries as the %-share of the GDP. *Source:* OECD database about public spending on labor markets.

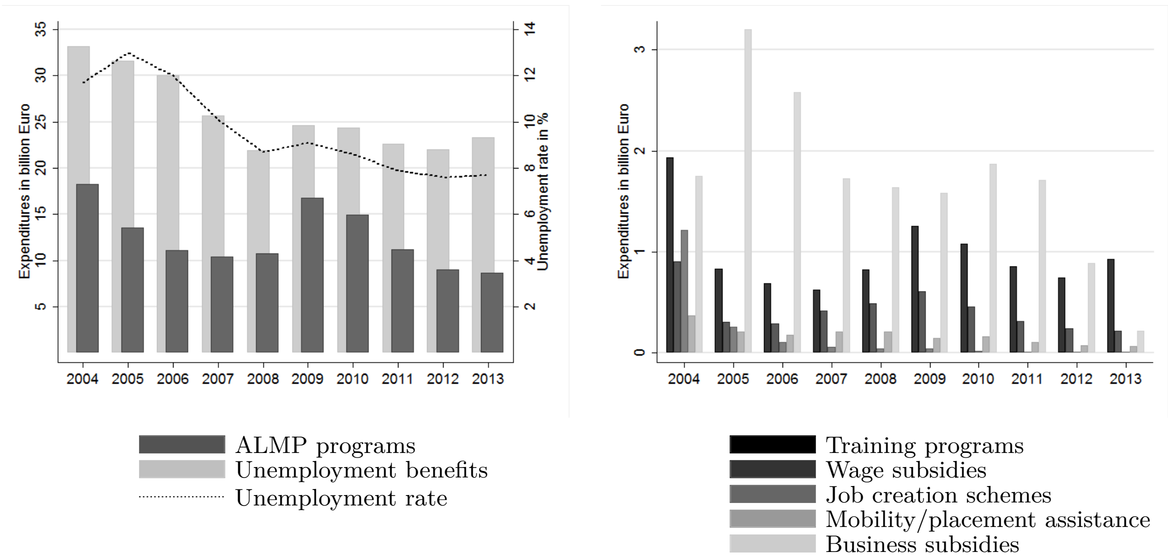
Traditionally, these ALMP programs can be divided into three categories. The first group provides onetime or temporary payments through wage subsidies to make specific job offers more attractive for certain workers (see e.g. Katz, 1998; Brouillette and Lacroix, 2010; Huttunen et al., 2013). The second group, public employment programs, involve the direct job formation for long-term unemployed, respectively hard-to-place workers, through job creation schemes (e.g. Caliendo et al., 2008) or workfare programs (e.g. Besley and Coate, 1992). Finally, the third, most commonly used, group of ALMP programs aims to increase the employability of the unemployed with training (see e.g. Biewen et al., 2014) or job search assistance (see e.g. Wunsch, 2013). A large strand of the economic literature analyzes the impact of these traditional ALMP programs on labor market outcomes, while there are several recent meta-analyses summarizing the results of international evaluation studies. The findings of Kluve (2010) and Card et al. (2010) suggest that these traditional ALMP programs only have limited success. Although, wage subsidies typically seem to increase the participants' reemployment prospects, job creation schemes are relatively ineffective, while training programs seem to create modest positive effects in the medium-run. In a recent follow-up study, Card et al. (2015) show that woman and long-term unemployed individuals tend to benefit more from ALMP programs in general. Moreover, the effects of training programs that aim to improve the accumulation of human capital are typically small or even negative in the short-run, but become more favorable in the medium- and long-run (2-3 years after completion of the program).

Although, these findings raise questions about the effect mechanisms of ALMP programs, most of the evaluation literature focuses on the effects of participating in one of these traditional programs on post-treatment labor market outcomes, but do not specify the consequences for the individual behavior before and during the treatment. However, as this can be expected to allow policy makers to improve the design and the allocation of labor market policies, the thesis uses Germany as a case study to provide new economic insights with respect to the effect mechanisms of ALMP programs. Relying on a unique set of administrative and survey data for unemployed workers allows me to extend the economic literature with respect to several dimensions. First, I consider the interplay of ALMP programs with job seekers' subjective beliefs and analyzes the consequences for the effectiveness of long-term training programs, as well as implications for the job search process. Second, the thesis provides first comprehensive evidence on the effects of an innovative class of programs which aim to increase the geographical mobility among unemployed workers. Since consequences for the job search behavior, as well as short- and long-term labor market outcomes are investigated, the analysis also provides evidence with respect to the underlying mechanisms of these programs. Third, it offers an empirical assessment of the relevance of usually unobserved variables, i.e. personality, expectations, social networks and intergenerational information, for the evaluation of ALMP programs. Since these variables can be expected to influence the selection into ALMP programs and labor market outcomes simultaneously the findings provide important insights with respect to the validity of previous evaluation studies. Finally, the analysis is complemented by an examination of the importance of gender differences in reservation wages among unemployed workers in explaining the realized wage gap between men and women which allows to assess the necessity of programs especially targeting the labor market participation of women.

Although, the expenditures in 2013 have been on a relatively low level compared to other Western European countries, Germany is a good example to study these effects since it has a long tradition of active labor market policies. It should be noted that after the implementation of a series of reforms between 2002 and 2005, the German labor market has undergone a positive development and has not shown a strong reaction to the Great Recession in 2008/09 compared to other countries (see e.g. Caliendo and Hogenacker, 2012) which results in relatively low expenditures on labor market policies. To consider the development over time, Figure 1.2a depicts the total expenditures for active and passive measure, as well as the corresponding unemployment rate for the period 2004-2013. The overall reduction of the unemployment rate, especially in the years after the implementation of the major labor market reforms in the early 2000s, is associated with a decline of total expenditures on unemployment benefits and ALMP programs. During the economic crisis, especially in 2009 and 2010, there have been higher expenditures on ALMP programs which have been associated only with a small increase of the unemployment rate.

Figure 1.2: Labor Market Policy Schemes in Germany

(a) Expenditures on active/passive measure (b) Expenditures on selected ALMP programs



Source: Statistic of the German Federal Employment Agency.
Note: (a) includes special programs for youth, disabled and elderly job seekers; (b) shows only expenditures on traditional active labor market policies targeting prime-age workers.

Moreover, Figure 1.2b shows the expenditures on selected ALMP programs representing different reintegration strategies which provides more detailed information about the design of the labor market policy system in Germany. It should be noted that there is a substantial variation over the ten-year period. On the one hand, this reflects differences with respect to the overall number of unemployed. On the other hand, this indicates also that the employment agency adjusts the policy mix, the allocation of the budget on different programs, over time.[1] For instance, job creation schemes have been popular in the past, but their usage was reduced tremendously as they turned out to be relatively ineffective (see e.g. Caliendo et al., 2008). On the other hand, the expenditures for subsidies that allow unemployed workers to start their own business have been increased dramatically in 2005. Although it has been shown that these programs are relatively successful (Caliendo and Künn, 2011, 2015), the legal claim for these subsidies has been suspended in 2011 which involved a substantial reduction of the expenditures. For traditional programs, such as training schemes or wage subsidies, there has been also a substantial variation with respect to the expenditures, however the fluctuations are far more moderate than for business subsidies. Finally, there exists a variety of subsidies which aim to directly support to the placement of unemployed workers, for instance by subsidizing costs associated to daily commuting, relocating, job applications and work equipment. The expenditures associated with these subsidies are in general relatively low compared to other programs and further declined since a reform in 2009.

Given these high expenditures on ALMP programs in general, a profound understanding of the underlying mechanisms is required to guarantee an effective usage of the financial means employed. However, beside numerous studies evaluating the effects of ALMP programs on subsequent labor market outcomes, only a small strand of the literature focuses on anticipation effects influencing the individual job search behavior (e.g. van den Berg et al., 2009) and actual labor market outcomes (e.g. Black et al., 2003; Rosholm and Svarer, 2008) before the realization of an ALMP program. These studies provide an initial step to gain a deeper understanding about the effect mechanisms and the selection of job seekers into ALMP programs. A theoretical framework to analyze these effects is given by job search models as discussed by Mortensen (1986). It is assumed that a job seeker chooses a search strategy by maximizing her own inter-temporal utility taking into account unemployment benefits, costs associated with the job search behavior and expected future returns given by the reemployment probability and the earned wage. Most of the studies exploiting a job search framework to analyze labor market policies focus on the impact of passive measures, either the level of unemployment benefits or the potential benefit duration. It is shown that a more generous system provides incentives to reduce the individual effort level (see Lichter, 2016; Marinescu, 2016) which implies a negative effect on the probability to leave unemployment (e.g. Chetty, 2008; Schmieder et al., 2012; Caliendo et al., 2013).

Moreover, traditional active labor market policies aim to increase the job seeker's employability by paying a direct wage subsidy, improving the skills of a job seeker or increasing the quality of job applications. If they are effective, these programs provide

[1] As discussed by Fertig et al. (2006) the policy mix does not only vary over time, but also on a regional level across local employment agencies in Germany.

incentives to spend more effort into job search activities as they increase the probability or the value of finding a new job. However, as discussed by van den Berg et al. (2009) job seekers who anticipate the upcoming program participation have incentives to adjust the job search behavior already before the treatment is realized, while the actual effect depends on the job seeker's perception about the value of the program. This includes the effect on the labor market performance, but also whether the she likes participation by itself. This is highly related to several studies illustrating that individuals select themselves into the treatment based on expectations about their future labor market performance (see Ashenfelter, 1978). For instance, individuals who expect that they will have low earnings in the future are more likely to participate since the income loss during the treatment is relatively low. For the US, it is shown by Heckman and Smith (1999) that participants in a training program experience an earnings dip already before the treatment has started. The self-selection into the program is likely to be associated with an adjustment of the individual behavior involving a reduction of the search intensity, and resulting in substantial locking-in effects (see van Ours, 2004; Lalive et al., 2008).

1.2 Contribution and Outline

Although, there exists comprehensive evidence on the effects of traditional labor market policies on participants' outcomes like reemployment prospects and wages, the previous findings suggest that there several aspects, especially with respect to the effect mechanisms of ALMP programs, that received only little attention so far. Based on theoretical considerations and an extensive empirical analysis of the German Labor Market, the thesis extends the existing literature into several dimensions. The structure of the thesis is highlighted in Table 1.1 which provides an overview about each chapter including research highlights, as well as the type of data used for the empirical analysis and additional information about the corresponding research paper, while the following section briefly discusses the content and findings of the subsequent chapters of the thesis.

The data basis of the empirical analysis is given by the administrative records for unemployed workers as provided by the Institute for Employment Research (IAB) (see Caliendo et al., 2011, for details) and covering information about employment subject to social security contributions, wages, unemployment benefits and ALMP participation. While the empirical analysis presented in Chapter 4 is directly based on these administrative data, the remaining chapters exploit survey information of the *IZA Evaluation Dataset* on individuals who enter unemployment between June 2007 and May 2008 (see Arni et al., 2014). These survey data cover a variety of non-standard questions, like information on personality traits, attitudes, job search behavior, expectations and preferences. Since the

Table 1.1: Overview of Thesis Chapters and Corresponding Research Papers

Chapter	Co-authors/ (declaration)	Data source	Research highlights	Publication status
2) Expectations and active labor market policies	—	Linked survey and administrative data[c)]	Expected participation probability has positive impact on effectiveness of realized training programs Related to greater support by caseworker and more flexible search strategy Early information treatments can increase program effectiveness	Prepared for submission
3) Mobility assistance and job search strategies	Marco Caliendo, Steffen Künn (p.253)	Survey data[b)]	Availability of mobility assistance encourages job seekers to increase search radius No impact on total number of job applications Increased search radius causes higher job finding rates and wages	Prepared for submission
4) The return to labor market mobility	Marco Caliendo, Steffen Künn (p.254)	Administrative data[a)]	Participants in relocation assistance earn higher wages and find more stable jobs Positive effects are the consequence of a better job match due to the increased search radius of participants	Accepted for publication in *Journal of Public Economics*
5) Usually unobserved variables and labor market policies	Marco Caliendo, Oscar A. Mitnik (p.255)	Linked survey and administrative data[c)]	Personality traits, attitudes, expectations etc. influence selection into ALMP programs Inclusion of these variables has no significant impact on estimated treatment effects Rich administrative data may be good enough to draw policy conclusions	Published in *Labour Economics*, 2017, 46, 14-25
6) The Gender wage gap and the role of reservation wages	Marco Caliendo, Wang-Sheng Lee (p.256)	Survey data[b)]	Reservation wages are an important determinant of the gender wage gap The gender wage gap disappears when controlling for reservation wages Reservation wage gap can arise from productivity differences, anticipated discrimination and job preferences	Published in *Journal of Economic Behavior and Organization*, 2017, 136, 161-173

Note: The table provides an overview about the chapters of the thesis and depicts related information about the corresponding research papers.
[a)] IZA/IAB Administrative Evaluation Dataset (see Eberle and Schmucker, 2015).
[b)] IZA Evaluation Dataset Survey (see Arni et al., 2014).
[c)] IZA/IAB Linked Evaluation Dataset (see Eberle et al., 2017).

dataset comprises four waves in total and therefore also contain information on realized labor market outcomes, it provides an ideal basis for analyzing the transition process from unemployment to employment. Moreover, it is also possible to directly merge the survey data, containing an extraordinary rich set of variables, and the administrative records, providing highly reliable information with respect to labor market outcomes and ALMP participation. This linked dataset is used for the analyses in Chapter 2 and 5. A detailed description of the used data is provided in each of the following chapters.

Expectations and Active Labor Market Policies

Chapter 2 analyzes the impact of job seekers' expectations about upcoming ALMP programs on the effectiveness of long-term training. In particular, I consider information on the expected participation probability and the expected treatment effect. Combining these subjective beliefs, measured at the entry into unemployment, and the realized treatment status later during the unemployment spell, allows me to derive new implications about the dynamics of the job search process and to link the traditional evaluation literature, focusing on post-treatment outcomes, to the recent studies analyzing the anticipation effects of ALMP programs. Based on theoretical considerations in the context of a job search model, it is shown that there exist several mechanisms that would imply an impact of the expected participation probability on labor market outcomes even after the actual treatment has taken place. First, as shown before, expectations about upcoming ALMP programs have an immediate impact on the search strategy (see e.g. van den Berg et al., 2009). If the choice of the search strategy in the current period is related to the optimal behavior in the future, this implies that job-seekers' pre-treatment expectations have also an impact on post-treatment labor market outcomes. Explanations for this inter-temporal relation of the search behavior can be manifold. For instance, there might be general efficiency effects, e.g. learning about own abilities or optimal search strategies, reference-dependent preferences or the appearance of additional costs associated with the adaption of new search methods during the treatment. A related explanation would connect the pre-treatment expectations to the choice of program providers or compliance with the program conditions.

A comprehensive analysis with respect to the impact of expectations on the program effectiveness shows that long-term training programs are indeed more effective when participants are aware of the treatment ex ante, while subjective beliefs about the effect of the program on the labor market performance are empirically unrelated to the actual program effectiveness. Moreover, it is shown that the finding is highly robust with respect to different types of observed and unobserved heterogeneity including the job seekers ability to correctly predict economic outcomes, individual motivation and the timing of the treatment. However, a further analysis of the job search behavior shows that expecting a participation is related to receive more support by the caseworker and a higher willingness to adjust the search behavior in association with a potential ALMP program which mirrors into the higher long-run employment rates. The empirical results provide important insights about the optimal policy of employment agencies when assigning unemployed workers to ALMP programs. The findings suggest that the low effectiveness of training programs can be partly explained by the fact that some job seekers have insufficient information about the potential future program participation which encourages them to choose search strategies that create high additional costs when they enter a program. This highlights the importance of the caseworker when providing unemployed workers relevant information about ALMP participation and developing the optimal search strategy along with the job seeker.

Mobility Assistance and the Return to Labor Market Mobility

The second major strand of this thesis analyzes the effects of a promising class of ALMP programs in Germany which had received only limited attention in the past. These so-called mobility programs aim to improve the geographical mobility by removing existing financial barriers for unemployed workers when searching for, respectively accepting, jobs in geographically distant regions. This includes travel costs to job interviews, daily commuting costs or the costs of a relocation. Based on a theoretical model which allows job seekers to search simultaneously in local and distant labor markets, it can be argued that the availability of the subsidies encourages job seekers to shift their search activities from local to distant regions. To analyze the causal impact of the subsidies on the job search behavior and subsequent labor market outcomes, an instrumental variable strategy is applied. Therefore, regional variation with respect to the local employment agencies' preferences towards mobility programs is used in order to create exogenous variation with respect to the probability that a job seeker receives knowledge about existence of the subsidies, as well as that an actual application will be approved. Therefore, it can be expected that job seekers living in local employment agency districts with a high preference for mobility programs are more likely to search for distant jobs, and finally also participate in the subsidy program

The empirical analysis of Chapter 3 exploits detailed survey data on individuals entering unemployment in Germany and investigates the impact of the mobility assistance scheme on the job search behavior and the subsequent transition from unemployment to employment. The findings show that the availability of the subsidies increases the likelihood to apply for distant vacancies and shifts the individuals' search effort from local to geographically distant regions without affecting the total number of job applications.

Moreover, the extended search radius causes higher reemployment probabilities, higher wages and a reduction of subsidized self-employment. The latter suggest that it apparently reduces the dependence on other forms of governmental support.

Related to these promising findings, Chapter 4 investigates the long-run effects of actually taking up a distant job for unemployed workers and receiving a relocation assistance. This specific mobility program, covers the moving costs, respectively the costs of renting a second flat at a new working location, to incentivize unemployed job seekers to search/accept jobs in distant regions. In general, all unemployed job seekers who are not able to find a job locally but in a distant region are eligible to the program. Thereby, it is required that the daily commuting time from the current place of residence to the location of the new job would exceed 2.5 hours. Based on detailed administrative records, it is shown that those individuals who take up subsidized employment in a distant labor market earn higher wages and have more stable jobs than individuals who find a job without the subsidy. It is also shown that the positive effects are the consequence of a better job match due to the increased search radius of participants rather than just a manifestation of regional differences with respect to price levels.

In summary, the findings of both chapters provide a very positive picture about the effectiveness of mobility assistance in Germany. This is especially important for European countries typically facing, on the one hand, high regional disparities in terms of unemployment rates, but, on the other hand, low geographical mobility among job seekers. It is shown that the introduction of subsidies which aim to support the acceptance of distant job offers can be a successful strategy to reintegrate unemployed workers into the labor market and that this is also associated with very positive long-term labor market outcomes if job seekers indeed move to distant labor markets. This is even more remarkable in the light of the relatively low program costs for the employment agency compared to traditional ALMP programs. For instance, vocational training creates costs that are about six times larger.

Usually Unobserved Variables and Labor Market Policies

Chapter 5 provides an empirical assessment of the importance of variables which are usually unobserved when evaluating traditional ALMP programs like training and wage subsidies. It addresses the key ever-present question, for non-experimental ALMP evaluations, whether the data can account fully for all the factors that explain both the participation in, and the outcomes of, a program. If this is not the case, estimators based on the unconfoundedness assumption, e.g. propensity score matching and weighting, become biased, either under- or overestimating the causal effects of a treatment. Although, in the last years the quality of administrative data has been improved and many countries now offer very informative and complete data including detailed information about labor market histories, at the same the economic literature has provided new evidence about the influence of variables such as personality traits or preferences on economic outcomes (see e.g. Heckman et al., 2006). Since these variables can be expected to influence also the job search behavior, as well as selection into specific ALMP programs, this raises concerns about the validity of the unconfoundedness assumption.

Relying on a unique combination of survey and administrative data, the chapter tackles this concerns explicitly. The data not only contain typical administrative-based information (similar to many other ALMP evaluations, particularly in Europe), but also information on characteristics usually not observed in the context of ALMP evaluations, like personality traits, attitudes, expectations, social networks and intergenerational information. Focusing on a class of estimators that are often used to evaluate ALMP programs and that rely on comparing treated and control individuals based on the propensity score, it is shown that these variables play a significant role for the selection into the treatment. However, differences with respect to the estimated treatment effects, when including or excluding these usually unobserved variables, are small and statistically insignificant. The results indicate that comprehensive control variables can operate as reasonable proxies and that rich administrative data, including detailed labor market histories, may be good enough to draw policy conclusions on the effectiveness of specific active labor market policies.

The Gender Wage Gap and the Role of Reservation Wages

The removal of gender differences in wages and labor market participation is often declared as a policy objective in modern societies. However, to design successful labor market policies that can achieve this goal, a deeper understanding of the driving mechanisms behind these gender differences is necessary. Therefore, Chapter 6 provides new evidence with respect to these mechanisms by examining the importance of differences in reservation wages for the gender wage gap. The reservation wage can be viewed as a measure of a person's eagerness or reluctance to accept employment and plays a key role in traditional job search theory (see Mortensen, 1986) by determining the unemployment duration.

The chapter focuses on the key research question whether any observed wage gap between men and women is simply an empirical realization of an initial gender gap in reservation wages. The novel contribution of the chapter is including the reservation wage into the decomposition of the gender gap in realized wages. By having survey data for a sample of newly unemployed individuals in Germany, including both reservation wages and realized wages on the same individual, it is possible to determine the extent to which gender differences in aspirations and expectations regarding wages can be a self-fulfilling prophecy and lead to gender differences in actual wages. As typical in the literature, the inclusion of standard explanatory variables, such as education, socio-demographics, labor market history and personality traits, reduces the gender gap in realized wages somewhat, but the gap remains statistically significant. However, the striking result implies that the inclusion of reservation wages halves the gender gap, making the remaining difference economically small and statistically insignificant.

2 Expectations and Active Labor Market Policies

It has been shown that unemployed workers who anticipate participation in an active labor market policy (ALMP) program adjust their job search behavior. Depending on the expected effect of such a program, they search more intensively to leave unemployment and prevent the treatment or reduce their effort to wait out until the program start. This chapter examines to what extent such expectations (with respect to the participation probability and the treatment effect) influence the effectiveness of realized ALMP programs. Theoretical considerations suggest that there is a causal effect if the job seekers' search strategy before participating is related to their behavior after the beginning of the program. Using a combination of German survey data and administrative records on newly unemployed job seekers, it is shown that participants in long-term training programs who are not aware of the treatment ex ante face significantly lower long-run employment rates compared to their participating counterparts expecting the treatment ex ante. A further analysis of the job search behavior is conducted to understand the effect mechanisms. It is shown that job seekers who do not expect a treatment also receive less support by their caseworker which results in a lower willingness to adjust their job search behavior in association with a treatment. The findings are highly robust with respect to differences in observed and unobserved characteristics, including expectations about the returns to treatment, motivation and the ability to forecast economic outcomes. It can be concluded that specific programs are more effective if potential participants were sufficiently informed about the treatment directly at entry into unemployment.

2.1 Introduction

Active labor market policies (ALMP) have a long tradition in many Western countries and represent one of the major instruments to reintegrate unemployed job seekers into the labor market. So far, the economic literature analyzing these programs typically focuses on evaluating the impact of a treatment on the subsequent labor market outcomes like employment prospects and earnings (see e.g. Card et al., 2010; Kluve, 2010, for an overview of international ALMP studies). Moreover, in the last decade several studies also show that the presence of ALMP programs has an impact on the job search behavior of unemployed workers even before they actually participate in a program (see e.g. Black et al., 2003; Rosholm and Svarer, 2008; van den Berg et al., 2009). This chapter links the two strands of the literature, by analyzing the relationship between job seekers beliefs about upcoming ALMP programs, obtained at the entry into unemployment, and the effects of a realized treatment on subsequent labor market outcomes. In particular, the chapter focuses on two dimensions of expectations which are essential in the context of ALMP programs: 1) the job seeker's perceived probability of participating in a program (given that the she remains unemployed) and 2) the expected returns to treatment with respect to the labor market performance, respectively the individual utility level.

From a theoretical perspective, it can be expected that the analysis of the program effectiveness with respect to these two measures provides important insights about the job search behavior and the effect mechanisms which would allow policy makers to improve the design and the allocation of ALMP programs. Previous studies have already shown that the expected participation probability has an impact on the job search behavior which crucially depends on the expected effect of the treatment (see van den Berg et al., 2009). For instance, the possibility of participating in a program which is expected to be beneficial provides incentives to reduce the search effort in order to remain unemployed until the treatment can be realized, while the opposite applies for a program which is expected to reduce the job seekers utility. These opposite effects are empirically documented by several studies exploiting specific eligibility criteria for ALMP programs. As shown by Black et al. (2003) for the US and Geerdsen (2006) or Rosholm and Svarer (2008) for Denmark, the presence of compulsory ALMP programs encourages job seekers to leave unemployment earlier to prevent participation. However, for the UK, van den Berg et al. (2014) show that the introduction of a multistage treatment encourages job seekers to reduce their search effort if they are close to reaching the eligibility criteria. Similar, Crépon et al. (2014) show that French job seekers who receive notifications about imminent training programs are less likely to leave unemployment.[2] Moreover, there exists two studies that explicitly utilize self-reported expectation measures in the context of ALMP programs. Van den Berg et al. (2009) show that job seekers who generally expect to participate in an ALMP program try to prevent participation by reducing reservation wages and searching harder than in the absence of the treatment, while Bergemann et al. (2011) show that these findings vary considerably among ethnic groups in Germany.

Given that this change of the pre-treatment job search strategy is related to the job seekers' actual behavior after the beginning of a program, this would imply that the expected participation probability would also have a causal impact on the program effectiveness. Related explanations connect the expected treatment probability to the compliance with program conditions, e.g. due to reference-dependent preferences (e.g. Kőszegi and Rabin, 2006), or the choice of different program providers. However, there might exist other observed and unobserved characteristics that are related to the job seekers pre-treatment expectations and have an impact on the labor market performance simultaneously. For instance, job seekers have specific beliefs about the effect of a treatment on their labor market performance. These beliefs are potentially correlated, on the one hand, with the expected participation probability (if job seekers assume that they can influence the probability of participating), and, on the other hand, also with the labor market outcomes (if the expected treatment effect is related to the actual treatment effect).

The literature is advanced with respect to several dimensions. First, the mechanism described above is incorporated into a job search model that accounts for uncertainty about upcoming ALMP participation (see van den Berg et al., 2009). Second, the implications of the model are tested empirically using a combination of extraordinary rich survey and administrative data that allows me to observe both dimensions of subjective beliefs, the expected participation rate and the expected treatment effect, for a sample of newly unemployed individuals in Germany. The chapter focuses on a specific program, long-term training, which generally requires a high level of participants' commitment, creates relatively large costs for the society compared to other ALMP programs and is frequently applied. It is analyzed how expectations with respect to the participation rate and the treatment effect, observed directly at the entry into unemployment, affect the labor market outcomes of actual participants (non-participants) in long-term training. Moreover, the data provide also detailed information with respect to the job search behavior and related expectation measures, e.g. income and reemployment prospects. This allows me to further

[2] Following the seminal work by Ashenfelter (1978), several studies show that individuals select themselves into the treatment based on expectations about their future labor market performance. For the US, Heckman and Smith (1999) show that there exists a substantial dip in pre-program earnings for participants in training programs using experimental data. The self-selection into the program might be associated with an adjustment of the individual search behavior during, respectively already before the program had started, typically involving higher reservation wages and a reduction of search intensities, and resulting in substantial locking-in effects (see van Ours, 2004; Lalive et al., 2008).

investigate the actual channels through which subjective beliefs affect the long-term labor market outcomes. Finally, a structural model is estimated which incorporates the fact that job seekers decide about their job search strategy and form expectations about a variety of outcomes, e.g. earnings, employment prospects, program participation and treatment effects, simultaneously.

The key finding is that long-term training programs are less effective when the job seekers are not aware of the treatment ex ante. However, participants' beliefs about the program effectiveness are empirically unrelated to the realized treatment effects. An extensive sensitivity analysis shows that the empirical results are highly robust with respect to several types of (un)observed heterogeneity including the job seekers ability to correctly predict future economic outcomes, differences with respect to motivation and the timing of the treatment. Moreover, the analysis of the search behavior shows that job seekers who do expect a treatment also receive more support by their caseworker, e.g. information about training programs or vacancies, which indeed results in a higher willingness to adjust their search behavior in association with a potential ALMP participation and mirrors into a positive effect on the long-run employment rates. The higher willingness to adjust the search behavior might be explained by the fact that such an adjustment requires the usage of different search methods which creates additional costs for individuals who have not been aware of the upcoming treatment. The estimates of the structural model suggest that these costs can be reduced by the caseworkers through information treatments and are directly related to the program effectiveness.

These findings provide important insights about the optimal assignment process of ALMP programs. Since previous papers often raise doubts about the efficiency of ALMPs (e.g. Card et al., 2010) and monitoring and sanctioning systems were suggested as a more efficient instrument to bring unemployed workers back to work (see Lalive et al., 2005), it is shown that these negative effects can be partly explained by the fact that job seekers have insufficient information about future program participation when entering unemployment. This highlights the importance of the caseworker, respectively the employment agency, when developing the optimal search strategy along with the job seeker.[3] Moreover, the chapter also contributes to the recent literature analyzing the consequences of individual perceptions and preferences on the search process during unemployment. For example, Dohmen et al. (2009) find systematical biases in the perception of job finding probabilities, while Spinnewijn (2015) shows that these biased beliefs affect savings decisions, the job

[3]This is in line with recent findings by Altmann et al. (2015), who show in a large-scale field experiment that informing job seekers about search strategies, the consequences of unemployment and labor market opportunities positively affects employment prospects and subsequent earnings, especially for those job seekers who at risk of being long-term unemployed. It is also related to several studies pointing out the importance of counseling unemployed workers (see e.g. Gorter and Kalb, 1996; Behaghel et al., 2014), analyzing the impact of caseworkers on job finding chances in general (see e.g. Behncke et al., 2010a,b), as well as their efficiency when assigning job seekers to ALMP programs Lechner and Smith (2007).

search behavior and have consequences for the unemployment insurance system. Results by Caliendo et al. (2015) indicate that job seekers who believe that their outcomes depend on their own actions (internal locus of control) search harder for new jobs, but also have higher reservation wages, while DellaVigna and Paserman (2005) show that workers who are more impatient search less intensively. Finally, recent findings by DellaVigna et al. (2017) show evidence for the presence reference-dependent search behavior with respect to previous income for unemployed job seekers in Hungary.

The rest of chapter is structured as follows. Section 2.2 presents the theoretical framework, the institutional details and the *IZA/IAB Linked Evaluation Dataset*. Section 2.3 introduces the expectation measures of interest and provides descriptive statistics, while Section 2.4 discussed the empirical strategy and shows the estimation results. Section 2.5 presents an empirical model of the expected search process, while Section 2.6 concludes.

2.2 Economic Framework, Data and Institutional Settings

To emphasize the underlying mechanisms that link the job seekers beliefs about ALMP programs to the effect of a realized treatment, the following section presents a job search model which incorporates expectations about future participation in an ALMP program. The model can be applied to a broad class of programs which requires a certain participation period, like training, workfare or job creation schemes. Based on a unique combination of survey and administrative data, the predictions of the model are tested empirically focusing on a specific ALMP program in Germany.

2.2.1 Baseline Model

It is assumed that an agent becomes unemployed in some period $t = 0$ and faces the possibility of participating in an ALMP program in the future. Conditioned on still being unemployed, the agents expected probability of entering the program in a certain period $t > 0$ is given as $\hat{\pi} \in [0, 1]$. It can be expected that this expectation is a function of the job seekers own characteristics, e.g. previous experience, but also information received by the caseworker during their regular meetings (see also van den Berg et al., 2009, who analyze a similar model of the search process). Moreover, in each period $t = 0, ..., T$, she has to decide about her job search strategy s_t. This search strategy potentially involves the level of search effort, the usage of different search methods, reservation wages or a decision on regions/firms where to apply. The choice of the search strategy has certain implications for the agents present and expected future utility. First, searching for a new job is associated with search costs $c_t = c(s_t)$. Moreover, the job finding rate in the current period is determined as a function of the search behavior: $\lambda_t = \lambda(s_t)$. If the agent finds a new job, she would earn a fixed wage ω which implies the utility $V^e(\omega)$, while, when not finding a job, the agent faces the possibility of entering a training program in the next period with probability $\hat{\pi}$. Therefore, for a given discount rate ρ, the inter-temporal value of being unemployed is characterized by:

$$V_t^u = -c_t + \rho \left\{ \lambda_t V^e(\omega) + (1 - \lambda_t)(V_{t+1}^u + \hat{\pi}(V_{t+1}^p - V_{t+1}^u)) \right\} \tag{2.1}$$

Once the agent enters an ALMP program, she faces different search costs $c_t^p = c^p(s_t)$ and a different job finding rate $\lambda_t^p = \lambda^p(s_t)$. This takes into account whether the agent likes to be in the program, respectively whether it is beneficial in terms of employment prospects. Since there is no longer the risk/possibility of being treated in the future, the inter-temporal value of participating in an ALMP program is given as:

$$V_t^p = -c_t^p + \rho \left\{ \lambda_t^p V^e(\omega) + (1 - \lambda_t^p) V_{t+1}^p \right\}. \tag{2.2}$$

As already shown by van den Berg et al. (2009), the possibility of participating in a program, indicated by the job seekers expected treatment rate $\hat{\pi}$, has implications for the optimal search strategy before a potential treatment has been realized depending on the value of the treatment $V^p - V^u$.[4] However, in order to emphasize the effect mechanisms it is useful to start with the case where the agent has already realized her actual treatment status in period $t+1$ and no longer faces the possibility of being treated in the future. This case does not consider the inter-temporal effects of subjective beliefs and represents a typical ex post comparison of participants and non-participants. Therefore, the optimal job search behavior s_{t+1}^* is characterized by the following first-order conditions:

$$\begin{aligned} \frac{\partial c(s_{t+1}^*)}{\partial s_{t+1}} &= \frac{\partial \lambda(s_{t+1}^*)}{\partial s_{t+1}} V^e(\omega) \quad \text{for non-participants,} \\ \text{and} \quad \frac{\partial c^p(s_{t+1}^*)}{\partial s_{t+1}} &= \frac{\partial \lambda^p(s_{t+1}^*)}{\partial s_{t+1}} V^e(\omega) \quad \text{for participants.} \end{aligned} \tag{\textit{IC1}}$$

Treated, as well as non-treated, agents equalize the marginal costs and the expected marginal returns of job search, while the difference between both groups is only determined by the search costs and the job offer arrival rate. However, without facing the possibility of being treated in the future, the expected treatment rate $\hat{\pi}$ has no impact on the actual behavior of participants, respectively non-participants, in the baseline model.

[4] For example, assuming that s denotes the search effort, there is typically a positive relationship between $\hat{\pi}$ and s if $V^p - V^u < 0$ and vice versa.

2.2.2 The Impact of Expectations on Job Seekers' Behavior

To establish a relationship between the job seekers' expectations measured at the entry into unemployment and the economic outcomes after the realization of an actual treatment it is necessary to impose additional assumptions about the determination of search strategies, as well as related behavior patterns and expectations. In the following, three potential explanations are discussed in more detail: 1) job search strategies are potentially related over time, 2) pre-treatment expectations can influence the job seekers preparation of the program and 3) there might be unobserved confounders that influence the agent's beliefs about and the long-term outcomes of the treatment simultaneously.

Inter-temporal Relation of Search Strategies The first mechanism requires that the agents search behavior in the current period t is related to the choice of her optimal search strategy in the previous period $t-1$. Therefore, it is assumed that the search costs are given as: $c_t = c(s_{t-1}, s_t)$. For instance, this inter-temporal relation of search costs can be explained by general efficiency effects, e.g. learning about their own abilities (see e.g. Falk et al., 2006), specific labor market and firm characteristics (see e.g. Morgan, 1985) or optimal search strategies, respectively reservation wages (see e.g. Krueger and Mueller, 2016). Moreover, changes of the search strategy between different periods can be expected to require the usage of new search methods which can be associated with additional costs since job seekers are not familiar with these new methods.[5]

The assumption implies that, based on Equation 2.1, the optimal search strategy of an agent who is still unemployed and has not yet been treated $\tilde{s}_t$ can be characterized by the following first-order condition:[6]

$$-\frac{\partial c(\tilde{s}_t)}{\partial s_t} + \frac{\partial \lambda(\tilde{s}_t)}{\partial s_t}\rho V^e(\omega) + \hat{\pi}\frac{\partial \hat{R}^p_{t+1}(\tilde{s}_t, \hat{s}_{t+1}(\hat{\pi}))}{\partial s_t} + (1-\hat{\pi})\frac{\partial \hat{R}^u_{t+1}(\tilde{s}_t, \hat{s}_{t+1}(\hat{\pi}))}{\partial s_t} = 0, \quad (IC2)$$

The condition visualizes several mechanisms determining the optimal behavior. The first term characterizes the direct marginal costs, e.g. associated with high levels of search effort, and the second term denotes marginal returns in form of reemployment probabilities. These two mechanisms are typical in the job search framework (see e.g. Mortensen, 1986) and similar to the baseline model discussed before. Moreover, there is a direct effect of $\hat{\pi}$ on the search behavior which depends on the expected utility difference between the treated and the non-treated situation $V^p - V^u$ (see e.g. van den Berg et al., 2009, for a detailed discussion). For instance, if $\hat{\pi}$ is large the agent has an incentive to increase the search

[5] Potential mechnisms are discussed in more detail in Section 2.5.

[6] For the ease of notation, I define $\hat{R}^u_{t+1}(s_t, \hat{s}_{t+1}(\hat{\pi})) = \rho(1-\lambda_t)V^u_{t+1}(s_t, \hat{s}_{t+1}(\hat{\pi}))$ and $\hat{R}^p_{t+1}(s_t, \hat{s}_{t+1}(\hat{\pi})) = \rho(1-\lambda_t)V^p_{t+1}(s_t, \hat{s}_{t+1}(\hat{\pi}))$ which can be interpreted as the expected discounted future value of a current investment in job search s_t when being unemployed, respectively participating in a program.

effort to prevent a treatment which is expected to reduce their utility level and vice versa. Finally, there is an additional indirect effect when the agent already anticipates that search strategies are inter-temporally related and she would be affected by spillover effects in the subsequent period. For instance, if the agent anticipates that she can gain from learning effects when not participating in a program in the next period, e.g. she would become more efficient in writing job applications when she has a high level of practice, she would spend more effort into her search activities when the expected treatment probability $\hat{\pi}$ is low. However, on the other hand the agent might also anticipate that she would need to use different methods when entering a program in the next period. Therefore, if $\hat{\pi}$ is high, she has an incentive to become familiar with these methods already before the treatment takes place to reduce the inter-temporal costs when participating in the next period.

Given that the search costs contain an inter-temporal component the fact that the pre-treatment search strategy is a function of the expected treatment rate $\tilde{s}_t(\hat{\pi})$ implies, according to *IC1*, also a relationship between $\hat{\pi}$ and the optimal search strategy s^*_{t+1} after the realization of the actual treatment status. This in turn has implications for the labor market outcomes of participants, respectively non-participants. For instance, given that the employment rate of participants is characterized by λ^p, the overall effect can be denoted as:

$$\frac{\partial \lambda^p}{\partial \hat{\pi}} = \frac{\partial \lambda^p}{\partial s_t} \times \frac{\partial s_t}{\partial s_{t-1}} \times \frac{\partial s_{t-1}}{\partial \hat{\pi}}, \tag{2.3}$$

while the sign of the effect depends on the functional form of the search costs and the job finding rates. The technical details are discussed in Appendix 2.7.1 and the empirical realization of the inter-temporal relationship is discussed in Section 2.5.

Program Compliance and Heterogeneous Providers A second potential mechanism directly relates the expected treatment rate $\hat{\pi}$ to the individual-specific job finding rate of participants λ^p. For instance, it could be assumed that participants who are assigned unexpectedly have a stronger distaste for the treatment compared to those participants who already expect to participate when entering unemployment. If this distaste for the treatment is associated with a reduced compliance with the program conditions, it can be expected that the treatment is less effective and participants who do not expect the treatment ex ante face lower employment rates in the long-run. Moreover, in Germany, potential participants in specific training programs typically receive a training voucher and can choose the actual provider of the program by themselves. If there is heterogeneity with respect to the quality of these providers, it can be expected that those job seekers who expect to participate have incentives to gather information in order to choose providers that positively affect their job finding prospects. Therefore, it can be assumed that there is direct positive effect of the expected treatment rate on the job finding probability of participants: $\partial \lambda^p / \partial \hat{\pi} > 0$.

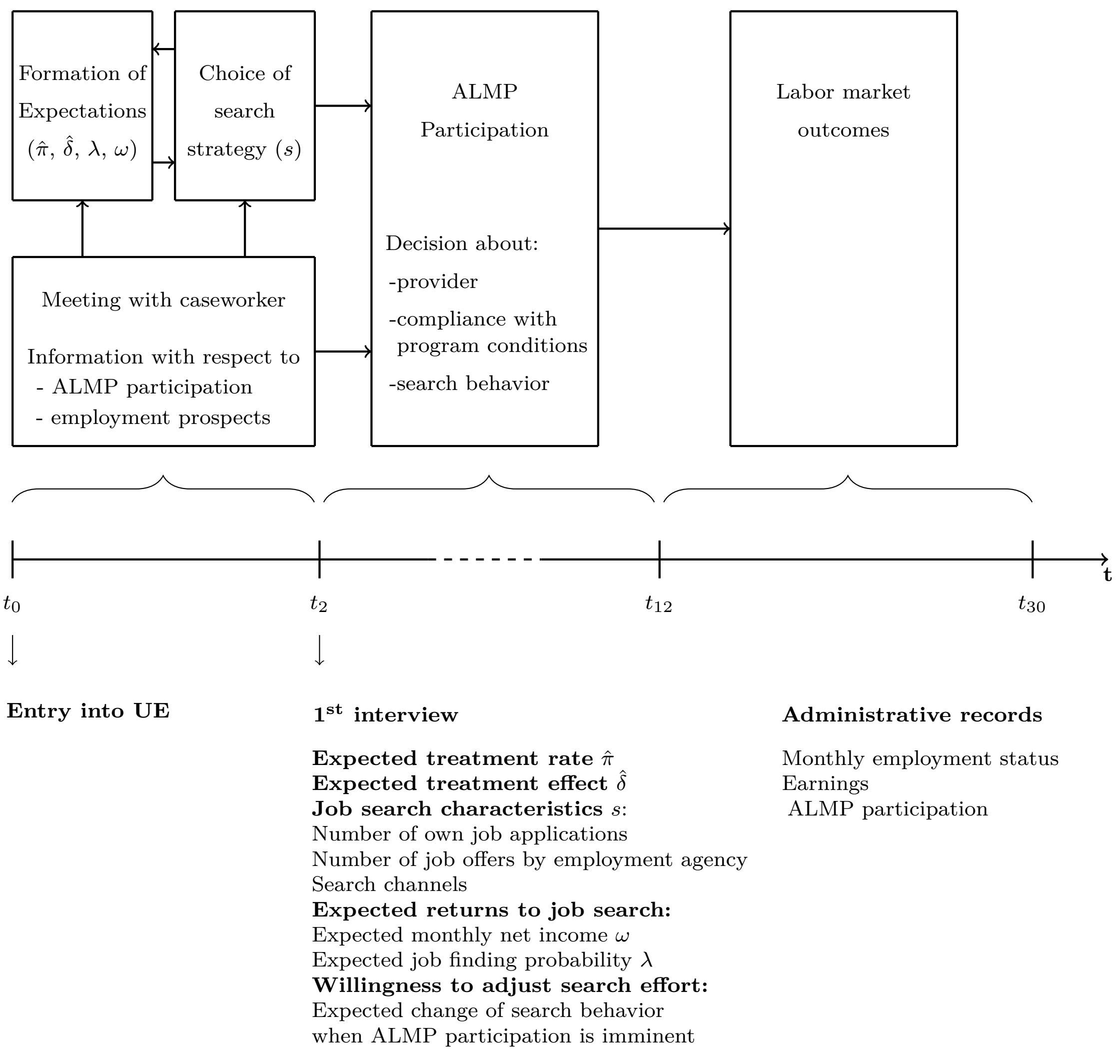
Alternative Explanations The previous mechanisms both imply a causal relationship between expected treatment probability $\hat{\pi}$ and the job finding rate of participants λ^p. However, it should be noted that there exists other, potentially unobserved, factors that might be related to expected treatment rate, the actual selection process into the program and the labor market outcomes simultaneously. For instance, it can be assumed that among job seekers who end up in an ALMP program those who already expect to participate when entering unemployment, i.e. $\hat{\pi}$ is high, simply have on average a higher ability in forecasting their treatment status. If this specific type of ability is related to the job seekers economic performance, it would imply a correlation between the expected treatment rate $\hat{\pi}$ and the actual labor market outcomes. It should be noted that a potential source of these unobserved differences might be established by the quality of the caseworker, as this might simultaneously affect the information that an agent receives about potential future treatments and therefore the likelihood to correctly predict the treatment status, but also her long-term labor market prospects. Finally, individuals have a specific perception of the effect of the treatment on their job finding prospects before entering the program: $\hat{\delta} = \lambda^p / \lambda$. If job seekers can (or expect to) influence whether they will participate, e.g. due to bargaining with their caseworker or by not redeeming training vouchers, it can be assumed that the expectations about individual-specific treatment effects mirrors in the expected treatment rate $\hat{\pi}$. When expected and actual treatment effects are (positively) correlated this would imply also a (positive) relationship between $\hat{\pi}$ and the effectiveness of the treatment.

2.2.3 Data and Institutional Setting

This chapter is based on the *IZA/IAB Linked Evaluation Dataset* which includes survey information on individuals who entered unemployment between June 2007 and May 2008 in Germany (see Eberle et al., 2017). About 17,400 individuals are interviewed shortly after the entry into unemployment (between 7 and 14 weeks). Besides the extensive set of individual-level characteristics (including socio-demographics and personality measure), as well as regional and seasonal information, the individuals are asked a variety of non-standard questions about their subjective assessments on future economic outcomes and job search characteristics. This includes expectations about ALMP participation (see Section 2.3 for details), the search intensity, the usage of different search channels, but also expectations about future earnings and employment prospects.

For the 88% of individuals who agreed, these survey data were then merged to administrative information from the *Integrated Employment Biographies* (IEB) provided by the Institute for Employment Research (IAB). The IEB integrates different sources, e.g., employment history, benefit recipient history, training participation history and job search history and therefore provides detailed information on labor market histories, as well as outcomes such as employment states, earnings, transfer payments and participation in active labor market policies for a period of 30 months after the entry into unemployment. Altogether, this amounts to a total of 15,274 realized interviews. The underlying economic and empirical framework is visualized in Figure 2.1.

Figure 2.1: Empirical Setting and Economic Framework



The combination of survey and administrative data provides an ideal setting to empirically analyze the mechanisms discussed before focusing on long-term training. On the one hand, the dataset includes expectation measures for long-term training, as well as information about the actual program participation. On the other hand, the program is frequently assigned to job seekers and requires a high level of participants' commitment since these programs typically last from several months up to one year, while for some degree courses participants might stay in the program for up to three years. The average program duration in the data set is about 6 months.

The program typically aims to improve occupational specific skills in order facilitate the reintegration into the labor market. Although, the usage of these long lasting and expensive measures was reduced related to the major labor market reform in the early 2000s, long-term training is still one of the most important ALMP programs in Germany. Previous studies find positive effects only in the very long-run (e.g. Fitzenberger et al., 2008; Lechner et al., 2011) or even partly negative effects on employment prospects (e.g. Lechner and Wunsch, 2008). In the short-run, these programs are expected to create a relative strong locking-in effect. From 2003 onwards, caseworkers no longer choose a specific course for the unemployed but hand out a training voucher to the job seeker. The caseworker defines the objective, the content and the maximum duration of the course, but the unemployed is allowed to find an appropriate provider for herself, respectively not to redeem the voucher (see Bernhard and Kruppe, 2012; Doerr et al., 2017). Moreover, it should be noted that there exist no explicit eligibility criteria for participating in a training program and participation is not mandatory in general. Therefore, the caseworker plays a crucial role. They are instructed to grant a voucher only if the probability that a job seeker will find employment immediately after finishing the program is estimated to be at least 70% (see Hipp and Warner, 2008). Hence, it can be expected that caseworkers are the main source of information for the unemployed job seeker. However, as described by Schütz et al. (2011), there exists a wide dispersion with respect to the quality of the job seekers counseling among caseworkers in Germany and the discussion, definition and adjustment of the job seekers targets is often rarely stringent.[7]

The estimation sample is restricted to all individuals who remain unemployed and do not participate in any ALMP program until the first interview takes place and report non-missing information for the relevant expectation measures discussed below. Job seekers are defined as participants if they attend long-term training within the first twelve months after the entry into unemployment. Moreover, I exclude all participants in short-term training measures. This is necessary since the dataset contains no expectation measures for those types of ALMP programs, but it could be expected that some of the participants relate

[7] Another important aspect of the German UI system with respect to formation of job seekers expectations is the so called integration agreement *(Eingliederungsvereinbarung)* (see e.g. Jacobi and Kluve, 2007; van den Berg et al., 2014). These compulsory agreements between the employment agency and the unemployed define the job seekers obligations and services that she received by the employment agency in a given period of unemployment, including search activities, as well as ALMP participation. Non-compliance could lead to a reduction of the unemployment benefits.

the corresponding questions about their expectations with respect to long-term training falsely to these short-term measures. Therefore, the final estimation sample contains 5,289 individuals, whereof 790 participate in long-term training and 4,499 individuals do not participate in any training program.

2.3 Measuring Expectations and Descriptive Statistics

2.3.1 Expected Treatment Rates

The key variables for the analysis are the actual treatment status and the expected participation probability in ALMP training programs $\hat{\pi}$. While the first information is obtained in the administrative records, the second is measured by answering the question how likely it is that long-term training participation occurs conditional on remaining unemployed in the upcoming three months. The answers range from 0 (very unlikely) up to 10 (very likely) in one digit steps. The distribution of this variable by the actual treatment status is depicted in the left column of Figure 2.2. In general, most individuals report either zeros, fives or tens, while there is a correlation between the expected and the actual treatment status. For example, about 36% of the participants report ex ante that it is very likely that they will participate, while only about 13% of the non-participants do so. In line with this, about 31% of the non-participants say ex ante that is very unlikely that they will participate, while only 17% of the participants report a zero.

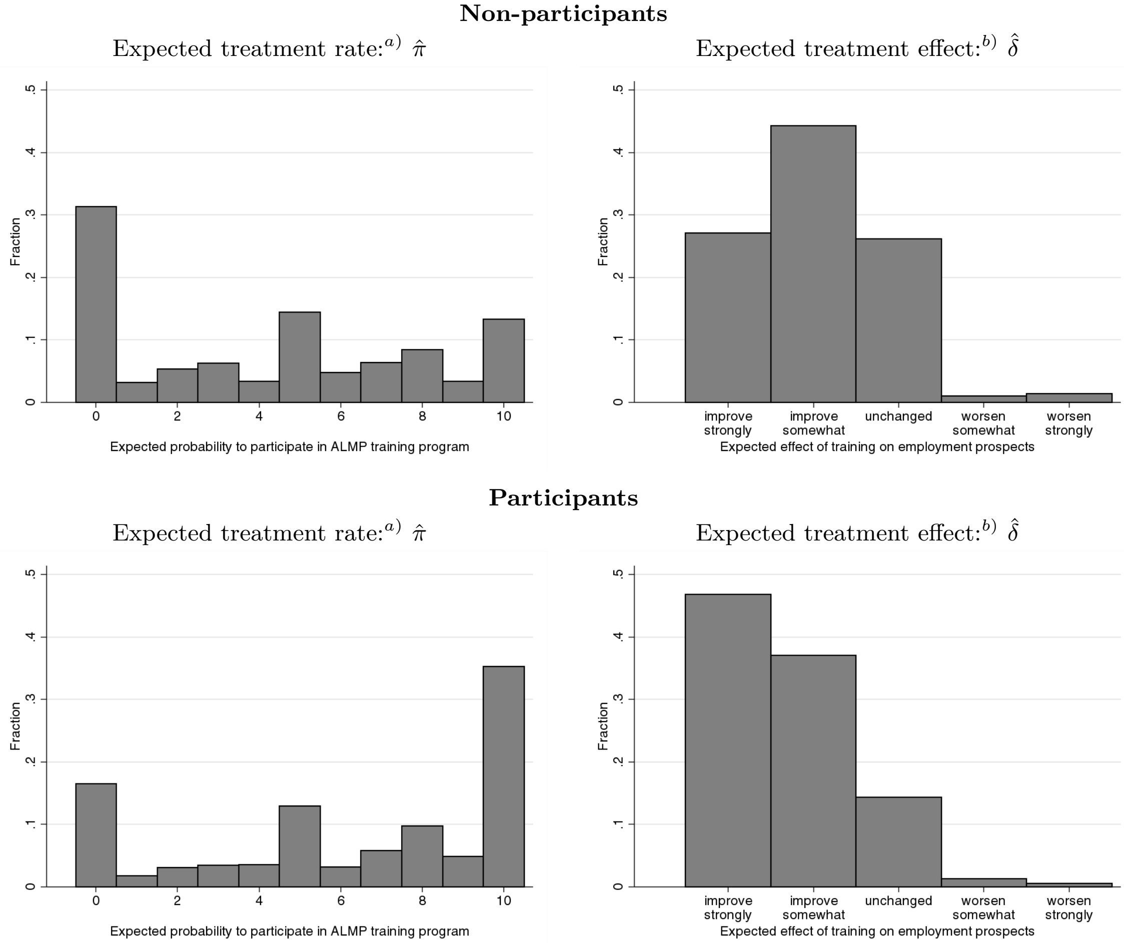
Based on this information on the expected participation probability, I construct a binary measure by summarizing the answers 0-4 ($\hat{\pi}$-low), respectively 5-10 ($\hat{\pi}$-high) (see van den Berg et al., 2009, who use the same variable without exploiting information on the realized participation).[8] Therefore, participants as well as non-participants are divided into two subgroups, those with low, respectively high expected treatment rates. This leads to four combinations of expected and actual treatment states which are exploited for the main analysis. A sensitivity analysis with respect to the group classification shows that there are only small differences with respect to main outcome variables within these four groups (see Appendix 2.7.2).

2.3.2 Expected Treatment Effects

The second important information refers to the expected effect of the treatment on the labor market prospects. The data provide a measure for the individual expectations about

[8] Note that for the ease of notation, I use the terms 'expecting a treatment', respectively 'not expecting a treatment', to describe individuals, who report expected participation probabilities between 5 and 10, respectively 0 and 4.

Figure 2.2: Distribution of Expectations by Actual ALMP Participation



[a)] Depicted are answers to the question: *"Assuming that you are still unemployed during the next 3 months. What is the probability that you will participate in a training scheme?"* 0 = very unlikely; 10 = very likely.
[b)] Depicted are answers to the question: *"In your opinion, to what extent would your chances of finding new employment be changed by participation in a training scheme?"* 1 = improve strongly, 2 = improve somewhat, 3 = remain unchanged, 4 = worsen somewhat, 5 = worsen strongly.

the effect of long-term training on their employment prospects which could be interpreted as the ratio of the two job finding rates: $\hat{\delta} = \lambda^p/\lambda$. Possible answers range from 'improve strongly' to 'worsen strongly'. As shown in the right column of Figure 2.2, in general, only a very few individuals expect these programs to worsen their labor market performance. However, those who participate, are also more likely to belief that the treatment will have a positive impact on their labor market outcomes. For example, only 27% of the non-participants think that training schemes will strongly improve their employment prospects, while 47% of the participants do. Again, both actual treatment groups are divided into two subgroups. For the main analysis, those individuals who report expected treatment effects in the highest category ('improve strongly') are denoted by $\hat{\delta}$-high while the remaining participants, respectively non-participants, are categorized as $\hat{\delta}$-low.

2.3.3 Observed Differences in Labor Market Outcomes

Table 2.1 provides first empirical evidence with respect to the importance of pre-treatment expectations, showing unconditional differences in labor market outcomes within the observation period of 30 months after the entry into unemployment. In particular, I focus on the employment status 12, respectively 30, months after the entry into unemployment, the total number of months in employment, as well as the total and average monthly earnings within the full observation period. From Panel A of Table 2.1 can be seen that non-participants who expect to participate in a training program face a higher employment probability of about 3-4 percentage points which is relatively constant from month 12 to 30 after the entry into unemployment and statistically significant at least at the 5%-level, while there are no significant differences with respect to earnings.

Table 2.1: Unconditional Differences in Labor Market Outcomes by Expectations and Actual Treatment Status

	Non-participants			Participants		
A. Expected treatment rate	$\hat{\pi}-$low	$\hat{\pi}-$high	$P-$value	$\hat{\pi}-$low	$\hat{\pi}-$high	$P-$value
No. of observations	2,222	2,277		223	567	
Regular employed in month						
t_{12}	0.469	0.510	0.006	0.238	0.358	0.001
t_{30}	0.540	0.571	0.035	0.480	0.589	0.005
Cumulated effect ($\sum_{t=0}^{30}$, months)	12.848	13.801	0.002	9.399	11.011	0.020
Cumulated earnings ($\sum_{t=0}^{30}$, in €)	17,580	18,134	0.300	14,104	15,052	0.398
Average earnings (€/month)	1,095	1,037	0.104	1,323	1,141	0.232
	Non-participants			Participants		
B. Expected treatment effect	$\hat{\delta}-$low	$\hat{\delta}-$high	$P-$value	$\hat{\delta}-$low	$\hat{\delta}-$high	$P-$value
No. of observations	3,244	1,208		416	367	
Regular employed in month						
t_{12}	0.483	0.505	0.187	0.312	0.335	0.499
t_{30}	0.559	0.547	0.474	0.553	0.561	0.813
Cumulated effect ($\sum_{t=0}^{30}$, months)	13.311	13.361	0.889	10.469	10.605	0.829
Cumulated earnings ($\sum_{t=0}^{30}$, in €)	17,788	17,848	0.921	15,231	14,133	0.275
Average earnings (€/month)	1,075	1,034	0.317	1,263	1,086	0.194

Note: Percentage share unless indicated otherwise. $P-$values measured based on two-tailed t-tests on equal means.

When considering participants, employment rates are substantially lower 12 months after the entry into unemployment compared to non-participants.[9] Moreover, there are also strong differences within the group of participants. Employment rates are between 10 and 12 percentage points higher for those participants who already expect the treatment when entering unemployment compared to those who are not aware of the treatment ex ante. The unconditional differences are statistically significant at least at the 5%-level, while, again, there are no significant differences with respect to earnings.

Moreover, Panel B depicts differences with respect to labor market outcomes between those participants, respectively non-participants, who expect training programs to have a strong positive effect on their labor market performance and those who do not. For none of the outcome variables there is statistically significant differences neither for non-participants nor participants. The latter provides first evidence that participants have only a very poor ability to predict the impact of a training program on their labor market outcomes and suggests that private information about the individual-specific program effectiveness are not the driving force of the observed differences with respect to the expected treatment rate.

2.4 Empirical Analysis

2.4.1 Estimation Strategy

The main objective is to analyze the effects of pre-treatment expectations on labor market outcomes of actual participants in long-term training, as well as non-participants. Therefore, I estimate treatment effects on the treated (ATT) using propensity score matching. The propensity score specification accounts for individual heterogeneity with respect to an extensive set of covariates including socio-demographics, household characteristics, labor market histories, regional and seasonal information, as well as personality traits.[10] Given the four combinations of expected and actual treatment states defined in Section 2.3.1, the estimated ATTs refer to the effect of expecting participation in long-term training ex ante ($\hat{\pi}$-high) compared to not expecting long-term training ($\hat{\pi}$-low) given the realized treatment status within 12 months. Moreover, a second set of ATTs is estimated which refers to the effect of expecting long-term training to be beneficial ($\hat{\delta}$-high) compared to

[9]These differences between participants and non-participants are not very surprising since long-term training programs last on average about 6 months and participants are generally expected to reduce their search effort during this period which would result in a locking-in effect. Moreover, it can be expected that there is a negative selection of individuals who stay unemployed until a treatment can be realized which might also contribute to the lower employment rate of participants in general.

[10]Descriptive statistics with respect to these observed characteristics are shown in Panel A of Table 2.14 in the Appendix.

a control group which expects the treatment to be less helpful ($\hat{\delta}$-low) using the categorization as defined in Section 2.3.2. Comparing the effect size of the two expectation measures $\hat{\pi}$ and $\hat{\delta}$ provides first evidence about the underlying mechanisms explaining the descriptive difference as shown in Section 2.3.3.

To provide a more profound understanding of the effect mechanisms, the second part of the empirical analysis investigates the relationship between the job seekers ALMP expectations and the job search strategy, as well as related expectation measures with respect to future economic outcomes. Since dataset includes information about the current search behavior when entering unemployment, but also the individuals' willingness to change the search behavior in connection with an upcoming ALMP participation, this allows direct conclusions with respect to the mechanisms discussed in Section 2.2.2. Finally, an extensive sensitivity analysis provides an empirical assessment of the robustness of the estimation results with respect to expectations being potentially endogenous. This comprises the application of a more sophisticated choice model in order predict the propensity score, a conditional difference-in-difference analysis and a placebo test using an alternative ALMP program (see Section 2.4.4).

2.4.2 The Impact of Expectations on Program Effectiveness

Table 2.2 presents the estimated ATTs for the two expectation measures separately for non-participants and participants referring to the matched difference between individuals reporting high and low expectation measures with respect to $\hat{\pi}$, respectively $\hat{\delta}$.[11] There are several possible estimators for the average treatment on the treated (ATT) parameters (e.g. Imbens and Wooldridge, 2009). For the sake of clarity, the main analysis focuses on a particular estimator, kernel matching with a bandwidth of 0.06, which is heavily used in evaluation studies. The propensity scores are estimated using separated pairwise logit models.[12]

Expected Treatment Rates Panel A of Table 2.2 shows the effect of expecting a treatment ex ante ($\hat{\pi}$-high v. $\hat{\pi}$-low) separated for non-participants, respectively participants. Column (1) and (3) refer to the unconditional differences (as already depicted in Table 2.1), while column (2) and (4) show the ATTs based on propensity score matching which

[11] In each case the group with the higher number of observations is used as the control group in order to minimize issues related to the common support condition. However, irrespective of the choice of treatment and control group, the depicted coefficients refer to the effect of reporting a high expected treatment rate (effect) compared to reporting a low expected treatment rate (effect). ATTs comparing alternative combinations of expected and actual treatment states are presented in Table 2.15 in Appendix 2.8.

[12] Marginal effects for these logit models are shown in Table 2.9 in the Appendix and the distribution of the estimated propensity scores is shown in Panel A of Figure 2.4. Estimation results for alternative matching algorithms are presented in Table 2.12.

accounts for differences with respect to observed characteristics. It is important to note that the matching estimates generally confirm the unconditional effects suggesting that differences with respect to socio-demographics, labor market histories or personality traits play only a minor role when explaining the long-term effect of the expected treatment rate $\hat{\pi}$. For non-participants, expecting participation is associated with a 3.3 percentage point higher employment rate 12 months after the entry into unemployment. The effect is statistically significant at the 5%-level and remains constant over time. It is still about 3.1 percentage points (significant at the 10%-level) 30 months after the entry, while there are no significant effects on earnings. The findings for non-participants are in line with the previous results by van den Berg et al. (2009) who show that job seekers who expect to participate in an ALMP program search harder and set lower reservation wages. This threat effect can be expected to result in higher job finding rates. Moreover, the effect seems to be persistent over time which suggest that the threat of being treated can create positive long-run employment effects.

When regarding participants in long-term training, there is a positive effect of $\hat{\pi}$-high compared to $\hat{\pi}$-low on the employment probability which is substantially larger than the effect for non-participants. One year after the entry into unemployment, the matched difference in employment rates between those participants expecting the treatment and those who do not is about 11.0 percentage points and statistically significant at the 1%-level. Again, the difference is relatively constant over the course of time. 30 months after the entry the effect is still about 9.2 percentage points and significant at the 5%-level. The lower employment probabilities of those who did not expect the treatment mirrors also in a lower cumulated effect over the full observation period of about 1.15 months. However, the effect is statistically insignificant at conventional levels. Again, there is no significant effect on earnings. Moreover, it should be noted that the estimated ATTs, that take into account differences with respect to an extensive set of control variables, are very similar to the unconditional difference. This can be interpreted as first evidence that the positive effect of the expected treatment rate on the program effectiveness cannot be explained by structural differences associated with $\hat{\pi}$.

Expected Treatment Effects Panel B of Table 2.2 shows the impact of the expected treatment effect $\hat{\delta}$ on the realized labor market outcomes separated for non-participants and participants according to the group classification discussed in Section 2.3.2. Again, column (1) and (3) show the unconditional effect of expecting the treatment to be beneficial, while column (2) and (4) show the matching estimates. In can be seen that there are no significant effects of the expected treatment effect for any of the labor market outcomes, neither for participants nor non-participants. Moreover, accounting for individual level characteristics has nearly no impact on the estimated differences. The findings for

Table 2.2: The Impact of Expectations on Program Effectiveness

	A. Expected treatment rate $\hat{\pi}$-high v. $\hat{\pi}$-low			
	Non-participants		Participants	
	(1)	(2)	(3)	(4)
Outcome variable				
Regular employed in month t_{12}	0.0409***	0.0330**	0.1204***	0.1097***
	(0.0149)	(0.0160)	(0.0396)	(0.0363)
Regular employed in month t_{30}	0.0313**	0.0306*	0.1092***	0.0924**
	(0.0148)	(0.0163)	(0.0411)	(0.0402)
Cumulated effect ($\sum_{t=0}^{30}$, months)	0.9532***	0.8795***	1.6115**	1.1456
	(0.3136)	(0.3382)	(0.7052)	(0.7195)
Cumulated earnings ($\sum_{t=0}^{30}$, in €)	554.0	826.9	948.1	875.4
	(534.7)	(556.6)	(1140.4)	(1222.0)
Average earnings (€/month)	-58.2	-52.9	-181.4	-131.3
	(35.8)	(37.2)	(151.7)	(233.2)
No. of observations	4,499	4,499	790	790
Treated off support		0		2
Mean standardized bias	7.73	0.68	9.65	1.44
	B. Expected treatment effect $\hat{\delta}$-high v. $\hat{\delta}$-low			
	Non-participants		Participants	
	(1)	(2)	(3)	(4)
Outcome variable				
Regular employed in month t_{12}	0.0222	0.0231	0.0226	0.0079
	(0.0168)	(0.0168)	(0.0335)	(0.0334)
Regular employed in month t_{30}	-0.0120	-0.0125	0.0084	-0.0042
	(0.0167)	(0.0176)	(0.0356)	(0.0354)
Cumulated effect ($\sum_{t=0}^{30}$, months)	0.0496	0.1215	0.1362	-0.0593
	(0.3550)	(0.3592)	(0.6309)	(0.6434)
Cumulated earnings ($\sum_{t=0}^{30}$, in €)	60.1	827.4	-1097.4	-876.1
	(603.0)	(593.1)	(1005.4)	(1058.8)
Average earnings (€/month)	-40.6	-4.2	-176.7	144.5
	(40.5)	(36.0)	(136.1)	(128.2)
No. of observations	4,499	4,499	790	790
Treated off support		0		0
Mean standardized bias	6.15	0.58	9.00	1.97
Control variables				
Socio-demographic characteristics	No	Yes	No	Yes
Household characteristics	No	Yes	No	Yes
Labor market histories	No	Yes	No	Yes
Regional and seasonal information	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes

Note: Depicted are average treatment effects on the treated (ATT) as the difference in mean outcomes between treated and matched controls using Epanechnikov kernel propensity score matching with bandwidth 0.06. Standard errors are in parentheses and based on bootstrapping with 399 replications. Treated and controls are defined based on $\hat{\pi}$, respectively $\hat{\delta}$, separated for non-participants and participants. $^{***}/^{**}/^{*}$ indicate statistically significance at the 1%/5%/10%-level.

non-participants are not very surprising given that the expectation measure refers to the impact of an event which does not take place. However, more surprisingly, the expected treatment effect is also unrelated to the actual treatment effect for those who start long-term training within 12 months which suggest that participants generally have a poor ability to predict the impact of the program.

Moreover, the results have also implications for the effect mechanism with respect to the expected treated rate $\hat{\pi}$. As discussed in Section 2.2.2, a potential concern might be that job seekers have private information about their individual-specific program effectiveness. On the one hand, this might lead to higher expected treatment rates since individuals anticipate that they can influence the likelihood of entering a program, e.g. due to bargaining with their caseworker or not redeeming training vouchers. On the other hand, this private information can be expected to be related to the actual treatment effects. The first is actually the case since $\hat{\pi}$-high and $\hat{\delta}$-high are positively correlated (the correlation coefficient is about 0.26). However, since expected and actual treatment effects are not related empirically, it can be concluded that subjective beliefs about the program effectiveness are not responsible for the differences in employment rates with respect to expected treatment rates $\hat{\pi}$. This is supported by the results presented in Table 2.16 in Appendix 2.8 showing that the positive effect is driven by those individuals who believe that the treatment has no or only a small effect on their labor market performance ($\hat{\delta}$-low), while there is no impact of the expected treatment rate $\hat{\pi}$ among those who expect a strong positive effect ($\hat{\delta}$-high).

2.4.3 Search Behavior and Related Expectations as Underlying Mechanism

As already discussed theoretically in Section 2.2, potential mechanisms that explain the positive impact of the expected treatment rate $\hat{\pi}$ on the effectiveness of training programs are related to the job seekers' behavior during unemployment, as well as during program participation. In order to identify these mechanisms empirically, Table 2.3 shows the matched differences between individuals reporting high and low expected treatment rates for several dimensions of job search characteristics, respectively expectations about future economic outcomes. All these variables are obtained during the first interview of the survey which takes place 7 to 14 weeks after the entry into unemployment, but before the actual treatment has been realized. In line with the baseline results, presented in Panel A of Table 2.2, the propensity score specification includes an extensive set of control variables and the ATTs of $\hat{\pi}$-high compared to $\hat{\pi}$-low are estimated separately for participants and non-participants.

Table 2.3: Matched Differences in Job Search Characteristics and Expected Returns

	Exp. treatment rates $\hat{\pi}$-high v. $\hat{\pi}$-low	
	Non-participants	Participants
	(1)	(2)
A) Own search strategies in t_0		
Average weekly number of own applications	0.0111	-0.4776
	(0.0765)	(0.4058)
Usage of search channels		
Total (10=high, 0=low)	0.2099***	-0.0215
	(0.0488)	(0.1603)
Involving others (4=high, 0=low)	0.1079***	0.1417**
	(0.0261)	(0.0685)
B) Contact to employment agency		
Utilizing caseworker as search channel	0.0644***	0.0911***
	(0.0112)	(0.0344)
Average weekly number of offers by employment agency	0.0176	0.0586***
	(0.0165)	(0.0287)
Information treatment received		
Training program	0.0434***	0.2650***
	(0.0088)	(0.0320)
Other ALMP program	0.0282***	0.0125
	(0.0086)	(0.0188
C) Adjustment of job search behavior		
Expected change of search behavior when ALMP program is imminent		
will increase search efforts	0.0984***	0.0979***
	(0.0139)	(0.0337)
will reduce search effort	-0.0034	0.0310**
	(0.0052)	(0.0148)
D) Expected returns to job search		
Log expected monthly net income	-0.0053	-0.0616*
	(0.0128)	(0.0326)
Will work for less than expected wage	0.0426***	0.0193
	(0.0136	(0.0386)
Expected employment probability within next 6 months		
very high	-0.0081	0.0035
	(0.0158)	(0.0377)
high	0.0631***	0.0656*
	(0.0145)	(0.0394)
Expected influence of employment agency on employment prospects		
will improve job finding chances	0.1348***	0.1337***
	(0.0139)	(0.0386)
will worsen job finding chances	-0.0113*	-0.0334**
	(0.0067)	(0.0159)
No. of observations	4,499	790
Control variables		
Socio-demographic characteristics	Yes	Yes
Household characteristics	Yes	Yes
Labor market histories	Yes	Yes
Regional and seasonal information	Yes	Yes
Personality traits	Yes	Yes

Note: Depicted are matched differences between treated/non-treated with $\hat{\pi}$-high and treated/non-treated with $\hat{\pi}$-low using Epanechnikov kernel propensity score matching with bandwidth 0.06. Standard errors are in parentheses and based on bootstrapping with 399 replications. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Own Search Strategy Panel A presents differences in variables characterizing the job seekers search strategies which are related to her own personal effort. These search strategies typically comprise several dimensions, like the individual search intensity or the usage of different search methods. The first observed variable characterizes the average weekly number of own job applications (measured between the entry into unemployment and the first interview). There are no significant differences with respect to $\hat{\pi}$ neither for participants nor non-participants which indicates that the search effort —measuring potential differences with respect to the job seekers motivation— is not a driving factor of the differences in long-term employment rates. A second set of variables describes the search channels that are utilized by the job seeker. The results show that, among the non-participants, those who expect a treatment in total use more different search methods than those who do not expect to participate, while there is no difference among the participants. In a second a step, I consider only those channels that involve the help of other people or institutions. This refers to agents of the employment agency, private agents (either with or without receiving a placement voucher by the employment agency), as well as contacting friends, acquaintances or family members. These channels are expected to create lower costs as the major part of the search effort is not raised by the job seeker herself. Irrespective of the actual treatment status individuals who expect to participate use significant more channels that involve other institutions.

Contact to Employment Agency Related to this previous finding, Panel B takes a closer look on variables that characterize the contact between the unemployed job seeker and the employment agency, respectively the responsible caseworker. First, it can be seen that much of the effect (60-64%) on search channels involving others is driven by the fact that those who expect a treatment more often utilize the caseworker as a search channel. Moreover, participants who expect a treatment also receive significantly more job offers by the employment agency, while there is no difference among non-participants. Moreover, especially participants who expect to participate more often received an information treatment about training programs.[13] Finally, for participants, there are no differences with respect to information treatments concerning other ALMP programs.

Adjustment of Search Behavior Panel C relates to differences with respect to the job seekers willingness to adjust their search behavior once the actual treatment is realized. Along with their expected participation probability, individuals are also asked how they will adjust their job search behavior when they would experience that the participation in

[13]These information treatments relate to a dummy variable which takes the value one if 1) the caseworker has already suggested to participate in a training program, 2) the caseworker has already suggested to hand out a training voucher or 3) the job seeker already received a training voucher before the first interview.

an ALMP program is imminent. Among both actual treatment states, those who expect to participate show a significantly higher willingness to increase their search effort compared to those who do not expect the treatment ex ante. Among participants, job seekers who are aware of the treatment are also more likely to decrease their search effort.

Expected Returns to Job Search Finally, Panel D shows difference with respect to the expected returns to job search, like future earnings and employment prospects, as well as the expected influence of the employment agency. The findings show that participants expect about 6% lower earnings if they expect to participate in a program, while there is no difference among non-participants. Moreover, non-participants reporting $\hat{\pi}$-high show a higher willingness to accept wage offers below the expected wage, while there are no significant differences among participants. Finally, expecting a treatment is generally associated with a high (but not a very high) expected reemployment probability and a more positive impression of the employment agencies capability of being helpful during the job finding process. The latter is probably related to the differences with respect to the number of job offers received by the employment agency and the usage of search channels as presented in Panel B.

In summary, the variables discussed in Panel A and B can be expected to characterize the search strategy of the unemployed worker (as denoted by the variable s in the model discussed in Section 2.2.1) before the actual treatment has been realized. First, the expected treatment rate $\hat{\pi}$ is not related to differences with respect to the agent's own effort. However, expecting a treatment is generally associated with having a closer relationship with the caseworker. Moreover, as shown in Panel C, this seems to translate into a higher willingness to adjust the search behavior when an ALMP program is imminent. It can be expected that for those individuals who end up in a program this adjustment has also implications for the participants' behavior during, respectively the preparation of, the treatment, as required by the causal mechanisms discussed in Section 2.2.2.

2.4.4 Sensitivity Analysis - Potential Endogeneity of Expectations

The baseline estimates presented in Table 2.2 already addressed the potential endogeneity of the expected treatment rate $\hat{\pi}$ in two ways. First, the propensity score specification includes a rich set of covariates which are potentially related to $\hat{\pi}$ and the labor market outcomes simultaneously. Second, the expected treatment effects $\hat{\delta}$ are explored as a potential driving factor. Although, the baseline estimates suggest that the results are robust with respect to these potential confounders, there might be concerns that expected treatment rates are related to other unobserved factors that also influence (non-)participants

Table 2.4: Addressing the Potential Endogeneity of Expectations

	Exp. treatment rates $\hat{\pi}$-high v. $\hat{\pi}$-low	
	Non-participants	Participants
	(1)	(2)
Regular employed in month t_{30}		
Baseline effect	0.0306* (0.0163)	0.0924** (0.0411)
Sensitivity analysis		
1) Nested logit model		
Type I	0.0278* (0.0151)	0.0843** (0.0375)
Type II	0.0341** (0.0161)	0.1024*** (0.0387)
Type III	0.0363** (0.0165)	0.1004*** (0.0384)
2) Dynamic treatment assignment		0.0671* (0.0384)
3) Conditional DID with reference level...		
...avg. employment rate last 2 years	0.0209 (0.0175)	0.0879** (0.0437)
...avg. employment rate last 10 years	0.0233 (0.0158)	0.1049*** (0.0402)
...expected reemployment probability	0.0343* (0.0204)	0.0886 (0.0558)
4) Placebo test$^{(a)}$		-0.0035 (0.0231)
No. of observations	4,499	790
Control variables		
Socio-demographiccharacteristics	Yes	Yes
Household characteristics	Yes	Yes
Labor market histories	Yes	Yes
Regional and seasonal information	Yes	Yes
Personality traits	Yes	Yes

Note: Depicted are average treatment effects on the treated (ATT) as the difference in mean outcomes between treated and matched controls using epanechnikov kernel propensity score matching with bandwidth 0.06. Standard errors are in parentheses and based on bootstrapping with 399 replications. Treated and controls are defined based on $\hat{\pi}$ separated for non-participants and participants. ***/**/* indicate statistically significance at the 1%/5%/10%-level.
$^{(a)}$The placebo test refers to participants in short-term training ($N = 1,681$).

long-term labor market outcomes. In the following, I discuss several additional robustness checks and the corresponding results considering the employment status 30 month after the entry into unemployment are presented in Table 2.4.

Nested Logit Model First, instead of using reduced-form logit models to estimate the propensity score, I implement a nested logit model that allows to relax the independence assumption by accounting for the correlation between states which are associated with common unobserved characteristics (for details see McFadden, 1978; Louviere et al., 2000; Hensher and Greene, 2002). Given that in the baseline setting individuals can either be treated or non-treated and face two potential levels of expected treatment rates ($\hat{\pi}$-high and $\hat{\pi}$-low), there are in total four potential states characterizing combinations of the expected and actual treatment status. In the following case, it is plausible categorize these states based on their correlation with the job seekers ability to predict the future treatment status, respectively her own economic outcomes in general denoted by U. For instance, it is assumed that an individual who reports high values of $\hat{\pi}$ and participates in the program later has on average a higher level of U compared to an actual participant who reports low values of $\hat{\pi}$ ex ante. However, for non-participants comparing the expected and actual treatment status does not create a valid proxy for the ability level U since job seekers might simply leave unemployment before the treatment could be realized. Therefore, I exploit the distance between the expected fob finding probability and the actual realization of this outcome as an alternative measure to proxy U for non-participants.[14] The basic idea is to exploit for both groups —participants, respectively non-participants— expectations about the specific event —entering a program, respectively finding a job— which terminates the initial unemployment spell to proxy their level of unobserved abilities. The underlying nesting structure is presented in Table 2.5.

However, as shown in Table 2.4 the implementation of this specific nested logit model (see Type I) has only a very small impact on estimated ATTs. This suggests that the unobserved ability to forecast the individual labor market status does not play an important role for the effect of $\hat{\pi}$. It should be noted that the estimation results of the nested logit model clearly depend on the assumed number of choices, as well as the imposed nesting structure. To test the sensitivity of the findings, two alternative models are imposed. First, individuals with the same actual treatment status (either non-participants or participants) are nested into one group with similar unobserved characteristics. This nesting structure is denoted as Type II and considers, for instance, unobserved heterogeneity with

[14]Expected job finding rates within the next 6 months are given as a four-point item ranging from 'very unlikely' to 'very likely'. It is assumed that a correct answer is given if the individual actually finds a job within the subsequent 6 months and reports beforehand the expected job finding rate to be 'likely' or 'very likely', respectively she actually does not find a job and reports the expected job finding rate to be 'unlikely' or 'very unlikely' and vice versa.

Table 2.5: Tree Structure Specified for Nested Logit Model Type I

Level 1	**Level 2**				
	Treatment status		Job finding		
Ability level U	Actual	Expected	Actual[a)]	Expected[b)]	Obs.
Low	1)Non-participants	$\hat{\pi}$-low	no yes	(very) likely (very) unlikely	1,093
	2)Non-participants	$\hat{\pi}$-high	no yes	(very) likely (very) unlikely	1,021
	3)Participants	$\hat{\pi}$-low			223
High	4)Non-participants	$\hat{\pi}$-low	yes no	(very) likely (very) unlikely	1,184
	5)Non-participants	$\hat{\pi}$-high	yes no	(very) likely (very) unlikely	1,201
	6)Participants	$\hat{\pi}$-high			567

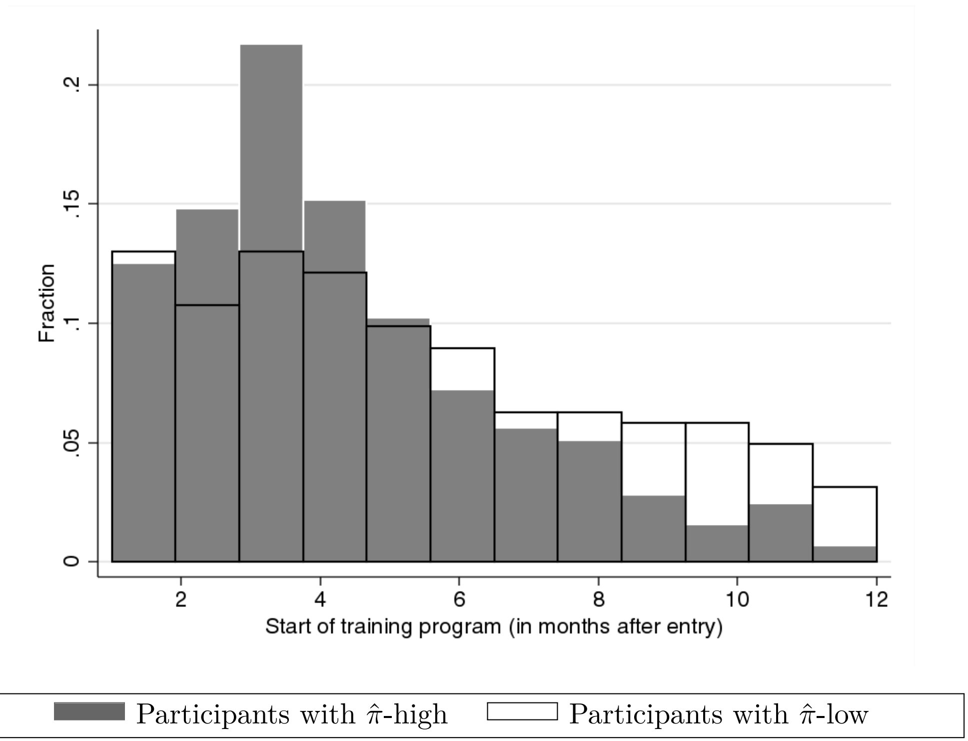
Note: Depicted is the decision tree structure for the nested logit model Type I. Each individual can end up in one of the six potential states depicted on the second hierarchy level. These six potential states are nested (into two nests) based on their relationship to the individual ability of correctly predicting future economic outcomes.
[a)] Refers to the actual transition from unemployment to regular employment within 6 months after the first wave.
[b)] Refers to the survey question: *"When you think of the future, how likely it is from your perspective that you will find a job within the next 6 months? 1=very likely; 2=likely; 3=unlikely; 4=very unlikely"*

respect to the caseworker's willingness to assign certain job seekers to a program. Finally, individuals with the same expected treatment status (either $\hat{\pi}$-high or $\hat{\pi}$-low) are nested (see Type III) which accounts for unobserved differences that are related to $\hat{\pi}$ like the job seekers level of motivation. Again, the results based on these two alternative nesting structures (Type II and III) are very similar and the estimated ATTs $\hat{\pi}$ are even larger for participants, as well as non-participants.

Accounting for the Elapsed Unemployment Duration As shown in Figure 2.3, there are significant differences between participants with $\hat{\pi}$-high and $\hat{\pi}$-low with respect to the timing of the program start. On average, participants who expect a treatment start the training program about one month earlier compared to those who do not expect the treatment (4.2 compared to 5.2 months after the entry). These delayed program starts of participants with $\hat{\pi}$-low potentially translate into lower long-term employment rates even if both groups would, apart from that, behave completely identical. Moreover, the time spend in unemployment is potentially related to individual unobserved ability. Those job seekers who remain unemployed longer and are therefore at the risk of being treated in later periods are likely to represent a negatively selected group of individuals.

Therefore, exact matching on the elapsed unemployment duration might be important to account for these potentially confounding factors (e.g. Biewen et al., 2014). This is implemented by adopting a dynamic matching approach. For each period t after the entry into unemployment treatment effects are calculated separately comparing individuals who enter the program in that specific period and those who are not treated up to t but who are still at risk of being treated, i.e. they are still unemployed in period t. Afterwards, the weighted average of the estimated treatment effects is calculated where weights are obtained from the share of treated in each period (see Sianesi, 2004, for details). Since the two groups of participants contain only a limited number of observations, the group of non-participants with low expected treatment rates is chosen as the unique reference group for both treatment groups and differences with respect to the estimated ATTs are calculated.

Figure 2.3: Distribution of Start Dates in Training Programs



Note: Depicted are months of program starts t_p for participants in long-term training separated by the expected treatment status $\hat{\pi}$. Mean values: $\bar{t}_p$($\hat{\pi}$-high)= 4.198; $\bar{t}_p$($\hat{\pi}$-low)= 5.220; $p-$value= 0.000. Two-sample Kolmogorov-Smirnov Test: $D = 0.157$; $p-$value= 0.001.

The estimation results show that accounting for the elapsed unemployment duration reduces the estimated coefficient for participants in long-term training about 2.5 percentage points, however the remaining effect is still large and statistically significant at the 10%-level. Moreover, it should be noted that, from an economic perspective, there are two mechanisms that potentially relate the expected participation probability $\hat{\pi}$ and the duration until the program start. On the one hand, individuals might already anticipate the exact timing of the program start at the moment of the interview and adjust the expected treatment rate accordingly. As this would not imply a behavioral change due to differences with respect to job seekers expectations but rather a correlation due to related unobserved factors it would be important to account for the unemployment duration to avoid a bias from these potential confounders. However, on the other hand, the duration until the beginning of the treatment might already be a function of the expected participation rate $\hat{\pi}$. For instance, individuals who expect the treatment might prepare themselves in a different way before the actual program start or choose different providers which could lead to differences with respect to the timing of the treatment. However, as this would be a causal consequence of the expected treatment rate $\hat{\pi}$, the dynamic matching procedure will underestimate the actual effect of the pre-program expectations $\hat{\pi}$. The fact that the estimated coefficient is still positive and statistically significant indicates that pre-treatment expectations indeed have an impact on long-term labor market outcomes beyond the effect induced by delayed program starts.

Conditional Difference-in-Difference Moreover, to investigate the importance of the potential endogeneity of the expected treatment rate, three conditional difference-in-difference models are estimated. The reference levels are given as 1) the average employment rate within the last 2 years before the entry into unemployment, 2) the last 10 years before the entry into unemployment and 3) the expected reemployment probability measured at the first interview.[15] All models are expected to account for time-constant unobserved factors which also affect the labor market outcomes. While the first two models account for factors which are related to realized employment probabilities in the short-, respectively long-run, history, the third model relates to unobserved private information that job seekers already take into account when reporting the expected reemployment probability. The DID estimates are very close to the baseline effect, indicating that neither past employment experiences (measured before the entry into unemployment) nor private information about reemployment prospects (measured directly after the entry into unemployment) are related to the labor market performance of participants.

Placebo Test Finally, I also conduct a placebo test considering participants in alternative training programs. These short-term courses last from two days up to eight weeks.[16] By estimating the effect of $\hat{\pi}$, which denotes the expected participation probability in long-term training, on the treatment effect for participants in short-term training, it is

[15] The reverence level is given by a dummy variable which takes the value one if the individual report the expected reemployment probability to be very high within the next 6 months.

[16] The total time spent in short-term training programs is limited to twelve weeks. In contrast to traditional long-term measures the courses aim to improve general employability or serve a test of occupation-specific abilities, including job search assistance, computer or language classes and special programs for certain hard-to-place workers.

possible to analyze the importance of specific types of unobserved heterogeneity which are independent of the program type. For instance, if high values of $\hat{\pi}$ are associated with higher levels of motivation, the positive effect of the expected treatment rate would also appear among participants in short-term training. However, when the differences in employment rates would be induced by a change of participants' behavior, which depends on specific program characteristics, e.g. the treatment duration, $\hat{\pi}$ would be unrelated to the treatment effects of short-term training. The empirical analysis shows that for participants in short-term training, the expected treatment rate does not have an impact on the employment probability. Therefore, it can be concluded that the positive effect of $\hat{\pi}$ for participants in long-term training is directly related to the treatment and is not induced by unobserved characteristics that are generally related to $\hat{\pi}$.

2.5 Implications for Expected Value Functions

So far, the empirical analysis established two important results. First, there is a positive effect of the expected treatment rate $\hat{\pi}$ on the long-run employment rates of participants in long-term training, respectively non-participants. This effect is robust with respect several types of observed and unobserved heterogeneity. Second, the expected treatment rate is related to differences with respect to the pre-treatment search strategy —mainly induced by the caseworker— and the willingness to adjust the search behavior once an actual ALMP participation is realized. In line with the theoretical considerations of Section 2.2, these behavioral differences provide a plausible explanation for the positive effect of $\hat{\pi}$ on the employment probability. However, when forming expectations about expected treatment rates $\hat{\pi}$, job seekers might simultaneously consider expectations with respect to other economic outcomes, i.e. the treatment effect, earnings and reemployment prospects, which are also potentially correlated with the labor market performance. In order to account for this fact and directly link the empirical analysis to the theoretical considerations, the following section presents an empirical model of the job seekers process of forming expectations based on the expected value functions discussed in Section 2.2. Moreover, the estimated parameters are related to the realized treatment effects presented in Section 2.4.

2.5.1 Empirical Model

The aim of the model is to predict the formation process of the job seekers expected treatment rate $\hat{\pi}$ at the entry into unemployment taking into account the agent's realized search behavior, as well as her expectations about future labor market outcomes measured at the first interview. Therefore, I do not consider realized outcomes, e.g. program participation or employment, in subsequent periods. Given the available data, it is reasonable to consider a three-period version of the model proposed in Section 2.2.1. It is assumed that the search strategy during unemployment s_u is characterized by the average weekly number of own job applications measured at the first interview. In the current period t_0, this search strategy implies costs of the form: $c_u(s_u) = \kappa_u s_u^2$ (see e.g. van den Berg and van der Klaauw, 2006). Moreover, the agent forms expectations about her labor market status in the subsequent period t_1 facing three options. First, she expects to find a new job with probability λs_u which is associated with the utility $\log(\hat{\omega})$,[17] with ω denoting the expected monthly net income. It is assumed that the agent expects to keep the job in the final period t_2 and receives the same level of utility. Second, when not finding a job with probability $(1 - \lambda s_u)$, she expects to participate in a training program with probability $\hat{\pi}$ which would imply a different search strategy s_p due to a different search costs $c_p(s_p) = \kappa_p s_p^2$ and the agent expects that the treatment influences the returns in t_2 which is given by the factor $\hat{\delta}$. Third, with probability $(1 - \lambda s_u)(1 - \hat{\pi})$ the agent expects to remain unemployed without entering a program. Therefore, the search costs and expected returns would be the same as in the initial period.

Behavioral Adjustment and Status Quo Bias As discussed in Section 2.2, the crucial assumption of the model implies that the perceived treatment probability $\hat{\pi}$ measured in t_0 is related to the agent's behavior in period t_1 after realizing her actual treatment status. Therefore, it is assumed that expecting participation in t_1 is associated 1) with the expectation that the agent needs to adjust her own behavior between t_0 and t_1 and 2) with the prospect that this adjustment creates additional costs. The first assumption seems to be reasonable given that the treatment reduces the time that is available for job search activities which requires an adjustment of the search strategy. However, the crucial question is whether the adjustment of the search behavior creates additional costs going beyond the direct impact of the new search strategy.[18] From a theoretical perspective there are several reasons that will justify this assumption. First, the adjustment of the search strategy is likely to be associated with a change of the search methods which can be assumed to create actual costs since job seekers might be less effective when they are not familiar with these methods.

Moreover, a related explanation could be derived from prospect theory (see Kahneman and Tversky, 1979; Samuelson and Zeckhauser, 1988). If the search strategy, and therefore also the amount of leisure, in the initial period t_0 defines an agent's reference

[17] It is assumed that the expected job finding probability λ contains an expected baseline rate which is estimated within the model and an individual-specific part which is predicted from an ordered probit estimation (see Table 2.17) of X on the expected job finding rate within the next 6 months measured on a scale from 1 ('very likely') to 4 ('very unlikely'). The distribution of the expected job finding rate and the expected earnings is depicted in Figure 2.5b and 2.5c.

[18] This direct impact of the new search strategy in t_1 for those expecting a treatment is indicated by $\kappa_p s_p^2$ (for the search costs) and $\lambda_p s_p$ (for the job finding prospects).

point for her future behavior, it can be expected that an increase of the search effort creates additional costs —beyond the costs that are directly associated with higher effort— when the agent has a preference to maintain the status quo. However, agents who already anticipate the treatment face the possibility of adjusting their behavior already in advance. A similar argument might also relate the expected treatment rate, respectively the agent's behavior in t_0, to the compliance with the program conditions and the effort that the agent will spend into the treatment activities. Assuming that program compliance is time-consuming, training participation is likely to reduce the leisure time which has a negative impact on the job seeker's utility. However, this effect can be expected to be stronger for those individuals who do not anticipate the treatment ex ante and therefore have no possibility to adjust their behavior in t_0 accordingly.

In the context of the empirical model, it is assumed that the adjustment of the agent's behavior between t_0 and the t_1 can be approximated by the information whether the agent will adjust her search behavior when the treatment is imminent. Although, the survey question implies that the agent adjusts the search behavior already before the treatment, this variable can be expected to provide a valid proxy for the flexibility of the agent's search strategy, respectively her overall behavior, in association with expected future ALMP programs. An alternative interpretation would imply that this variable measures the agent's willingness to avoid the imminent treatment. However, it should be noted that, due to the voucher system, job seekers always face the option not to redeem the voucher in order to avoid participation. Moreover, a comparison of the willingness to adjust the search behavior and expected treatment effects shows that those job seekers who would have incentives to wait out until the treatment do expect to increase their search effort (see Appendix 2.7.2 for details). This suggests that the variable proxies the flexibility of the agent's search strategy rather than her willingness to avoid the treatment.[19]

As this is crucial for the model, I propose two alternative ways to determine the expected change of the search strategy and corresponding adjustment costs: $\kappa_a\eta$. First, the effort adjustment is estimated by using information from the second wave of the survey (which takes place about 12 months after the entry). Changes of effort levels are obtained for 438 individuals who participate in a training program between the first and the second interview and predictions for the full sample are generated based on OLS estimates.[20] The adjustment costs are given as $\kappa_a\eta = \kappa_a(s_p - s_u)^2$. This refers to the baseline model in the following. As an alternative, I exploit only the information whether

[19] It should be noted that even if the alternative interpretation would be true, it would not query the validity of the empirical model. This is the case since the model does not account for actual program participation but rather relates $\hat{\pi}$ to an expected adjustment of the search behavior which could take place either before or after the actual program start.

[20] See also Table 2.17 for the results of the corresponding OLS estimation and Figure 2.5d for the distribution of the expected effort change based on these estimates.

the individual will the increase search effort and estimate a parameter η which characterizes the magnitude of this adjustment. Hence, the search effort during the treatment is defined as $s_p = s_u + \eta$. It should be noted that, for this alternative model, the adjustment costs occur only if the agent expects to increase the search effort. This reflects the fact that an effort reduction (which is associated with more leisure time) is unlikely to imply the application of new search methods or to affect the agent's utility negatively due to reference-dependent preferences.

Estimation of Structural Parameters Given the empirical model and the discount rate ρ, the expected utility difference between treated and non-treated situation over the three-period horizon is given as

$$\Delta\hat{V}(s_u, s_p) = \rho(1 - \lambda s_u)\left\{\rho\lambda \log(\hat{\omega})(\hat{\delta}s_p - s_u) - (\kappa_p s_p^2 - \kappa_u s_u^2 + \kappa_a \eta)\right\}. \tag{2.4}$$

The factor before the brackets denotes the discounted probability that the agent is still unemployed in period t_1 and therefore faces the possibility of being treated. The first term inside the brackets denotes the expected discounted utility difference between the treated and non-treated situation with respect to labor market returns, while the last term characterizes differences with respect to search costs. In order to estimate the expected utility difference, it is assumed that the expected treatment rate $\hat{\pi}$ is an ordinal outcome variable that takes values $j = 1, ..., J$ when $\zeta_{j-1} \leq \Delta\hat{V}(s_u, s_p) < \zeta_j$ (see e.g. Cunha et al., 2007; Greene and Hensher, 2010). Therefore, the log-likelihood is characterized by:

$$\ln \mathcal{L} = \sum_{i=1}^{N}\sum_{j=1}^{J} \hat{\pi}_{ij} \ln\left\{\Phi(\zeta_j - \Delta\hat{V}_i(\kappa_u, \kappa_p, \kappa_a, \lambda, \hat{\delta})) - \Phi(\zeta_{j-1} - \Delta\hat{V}_i(\kappa_u, \kappa_p, \kappa_a, \lambda, \hat{\delta}))\right\}, \tag{2.5}$$

where Φ denotes the cdf of the normal distribution. Moreover, I allow for individual heterogeneity with respect to the parameters κ_u and κ_p depending on the observed characteristics X (see Table 2.18 for an overview of variables included in X):

$$\kappa_u = \gamma_u X + \varepsilon_u \qquad \text{and} \qquad \kappa_p = \gamma_p X + \varepsilon_p. \tag{2.6}$$

In total, I estimate four different versions of the model. In the baseline model, the level of search effort during the treatment is imputed from wave 2 information for those who participate in a training program, while in the alternative model an additional parameter η is estimated which denotes the magnitude of the adjustment of the search effort for those who report that they will change their search behavior when entering a program. Moreover, for both versions of the model, I additionally include unobserved heterogeneity by allowing for two different types of agents with high, respectively low, levels of utility cutoffs ζ.

Finally, it is assumed that, during the first meetings with the caseworker, the job seeker receives specific information about potential future program participation and employment prospects in general. This set of information Z is assumed to influence the agent's specific search strategy and therefore also the level of the adjustment costs that will arise once she enters a program:

$$\kappa_a = \gamma_a Z_i + \varepsilon_a. \tag{2.7}$$

The vector Z contains several variables indicating whether the job seeker utilizes the caseworker as a search channel, the number of job offers she received from the agency, as well as indicators for whether she received an information treatment with respect to training programs, respectively other ALMP programs, or a job offer for full- or part-time employment.[21]

It is important to note that the model is estimated based only on the agent's search behavior and expectations measured during the first interview and does not rely on realized labor market outcomes or the actual program participation. Therefore, the estimated parameters refer to the agent's expectations about search costs, reemployment probabilities and treatment effects. The main objective of the estimation procedure is to identify the set of parameters κ_a that can be interpreted as agent's expected costs of adjusting the search behavior when entering a training program, respectively the impact of the employment agency on these expected costs. It can be argued that these parameters are relevant in the context of the search model proposed in Section 2.2 since the agent's decision about the behavior during the treatment is taken only based on her expectations about costs that will arise in the future.

2.5.2 Empirical Results

For the estimation of the parameters, I assume that there exists $J = 3$ potential levels of expected treatment probabilities ($\hat{\pi}_1 = \mathbb{1}\{\hat{\pi} \in (0,3)\}$, $\hat{\pi}_2 = \mathbb{1}\{\hat{\pi} \in (4,6)\}$ and $\hat{\pi}_3 = \mathbb{1}\{\hat{\pi} \in (7,10)\}$). This reflects the fact that the empirical distribution of $\hat{\pi}$ has three

[21] Descriptive statistics with respect to these information treatments can be obtained in Panel B of Table 2.14 in Appendix 2.8.

peaks at zero, five and ten (see Figure 2.2).[22] The main focus of the analysis is on the identification of the set of parameters κ_a indicating the job seekers expected adjustment costs. The estimates of the constant suggest that a job seeker who had no contact to the employment agency (until the first interview) expects to face substantial costs when adjusting the search strategy in association with an imminent training program. Although, the estimated coefficients are about three times larger in the alternative (compared to the baseline) models, the effect is statistically significant in all four cases. Moreover, the findings show that specific information treatments that the job seeker received by the employment agency affect these adjustment costs. Most importantly, informing the job seeker about the availability of training programs reduces the level of the adjustment costs significantly. For the two alternative models this reduction is even larger than the constant indicating that job seekers who have been already informed about these programs, e.g. by receiving a training voucher, do not expect any adjustment costs.

Moreover, since the parameters κ_u and κ_p denoting the search costs during unemployment, respectively the treatment, depend on individual characteristics X, average values for these two parameters are depicted in Table 2.6.[23] For all models, the expected search costs are on average higher during the program which seems to reasonable given that participating in a training program is time-consuming and there is less time available for job search. It should be noted that there might exists external factors, e.g. the influence of the caseworker or the threat of sanctions, which would encourage the agent to spend effort into job search activities even if the expected returns in terms of future earnings and employment prospects are relatively low. As I do not take these external factors into account explicitly, they are captured implicitly by probably lower search cost parameters.

Considering the expected returns to job search all estimated parameters have the sign as expected and are of reasonable size. The baseline hazard rate λ^{base} characterizes the expectation of an average agent that she would find a new job between two periods given that she sends out one application per week.[24] Moreover, the estimates for the expected treatment effects δ_1, respectively δ_2, suggest that those agents who expect the training program to have a (very) positive effect expect a utility increase of about 54% (107%) in

[22]Since the analysis of the expected value functions can be conducted without conditioning on the actual treatment status, it is possible to allow for a finer segregation of $\hat{\pi}$ compared to the analysis of Section 2.4, with only two levels of expected treatment rates $\hat{\pi}$-high/$\hat{\pi}$-low. Moreover, this allows also to include individuals who participate in short-term training within the first 12 months after the entry. The findings are qualitatively similar when excluding those individuals. Results are available upon request. Finally, individuals from the highest/lowest percentile of the expected income distribution are excluded in order to avoid a strong impact of a few individuals who report implausible high/low values for this variable.

[23]Full estimation results are shown in Table 2.18 in the Appendix.

[24]It should be noted that in this context the definition of a period refers to respondents interpretation of the survey question on the expected treatment rate as depicted in Figure 2.2.

Table 2.6: Structural Parameters of Expected Value Function

		Baseline Model		Alternative Model	
		(1)	(2)	(3)	(4)
Parameters of Search Cost Function					
Adjustment costs	κ_a				
Utilizing caseworker as search channel		-0.1079	-0.1478	-0.1120	-0.0548
		(0.0682)	(0.1153)	(0.1286)	(0.1586)
Information treatment received					
Training program		-0.1305**	-0.2258**	-0.7681***	-1.0685***
		(0.0664)	(0.1091)	(0.2250)	(0.3031)
Other ALMP program		-0.0692	-0.1556	-0.3644	*-0.6329*
		(0.1004)	(0.2023)	(0.2245)	(0.3397)
Job offer received					
Full-time employment		0.1954***	0.2062**	0.1190	0.2055
		(0.0644)	(0.1026)	(0.1279)	(0.1700)
Part-time employment		0.0373	0.2283*	-0.2249	-0.2986
		(0.0833)	(0.1261)	(0.1816)	(0.2519)
Avg. weekly no. of offers by caseworker		-0.2743***	-0.6398***	-0.1980	-0.6609*
		(0.1023)	(0.2179)	(0.2164)	(0.3435)
Constant		0.2098***	0.2680**	0.6146***	0.7839***
		(0.0678)	(0.1063)	(0.1566)	(0.1785)
Search costs in unemployment (avg.)	κ_u	0.1079	0.1835	0.0903	0.0468
		(0.0699)	(0.1138)	(0.1296)	(0.1684)
Search costs in training program (avg.)	κ_p	0.1543**	0.2431**	0.1423	0.1059
		(0.0680)	(0.1138)	(0.1296)	(0.1684)
Parameters of Expected Return Function					
Expected baseline hazard	λ^{base}	0.0752***	0.0912***	0.1018***	0.0950***
		(0.0114)	(0.0047)	(0.0065)	(0.0047)
Expected treatment effect					
positive	δ_1	0.5434***	0.6125***	0.5362***	0.7007***
		(0.0780)	(0.0615)	(0.0435)	(0.0614)
very positive	δ_2	1.0777***	1.8021***	0.9707***	1.6899***
		(0.1090)	(0.1251)	(0.0548)	(0.1371)
Expected change of search effort	μ			0.6000***	0.6585***
				(0.1405)	(0.1379)
Utility cutoff 1	ζ_1	-0.0117		0.0828***	
		(0.0218)		(0.0253)	
	ζ_1^{low}		-0.4729***		-0.3206***
			(0.0575)		(0.0547)
	ζ_1^{high}		2.5005***		2.4947***
			(0.1654)		(0.1762)
Utility cutoff 2	ζ_2	0.6183***		0.7226***	
		(0.0229)		(0.0263)	
	ζ_2^{low}		0.4394***		0.5731***
			(0.0371)		(0.0389)
	ζ_2^{high}		5.8692***		6.4322
			(1.5989)		(5.9892)
Share of high cutoff individuals	q^{high}		0.7368***		0.7514***
			(0.0174)		(0.0174)
Discount factor (fixed)	ρ	0.9500	0.9500	0.9500	0.9500
No. of observations		6,239	6,239	6,239	6,239
log-Likelihood		-6,404.9	-6,369.4	-6,314.7	-6,295.2
LR test (χ^2)		320.5	391.6	500.9	1948.4
		{0.000}	{0.000}	{0.000}	{0.000}
Hitrate					
absolute		0.8345	0.8326	0.8611	0.8545
difference		0.0538	0.0519	0.0804	0.0738
Unobserved heterogeneity		No	Yes	No	Yes

Note: Depicted are Maximum-Likelihood Estimates. The LR-test and the hitrate difference refer to a comparison to an ordered probit model based on covariates X. The hitrate is defined as: $\frac{1}{NJ}\sum_{i=1}^{N}\sum_{j=1}^{J}\hat{\pi}_{ij}\mathbb{1}\left\{P_{ij} \geq \bar{P}_j\right\} + (1-\hat{\pi}_{ij})\mathbb{1}\left\{P_{ij} < \bar{P}_j\right\}$. Standard errors are shown in parenthesis; $p-$values in brackets. $^{***}/^{**}/^{*}$ indicate statistically significance at the 1%/5%/10%-level.

the baseline model without unobserved heterogeneity. While the estimates are similar for the alternative model, allowing for unobserved heterogeneity suggests even larger expected benefits of the treatment. At first sight this effect appears large, however it should be noted that this might also capture whether the individual expects to like participation per se. Moreover, it can also be expected that the caseworker affects the job seeker's perception of the program positively. Finally, the alternative model provides also an estimate for the parameter η which denotes the magnitude of the expected effort adjustment during the program which is about 0.60 to 0.67 applications per week. This is substantially larger than the corresponding prediction for the baseline specification exploited from participants observed changes with respect to the search behavior between wave 1 and 2 which is about 0.21 applications per week.

Model Fit When interpreting the results, it must be considered that all parameters refer to the job seekers perception of search costs, respectively their expectations about labor market outcomes. Therefore, it is difficult to evaluate whether the estimated parameters are realistic or not. In order assess the quality of the model, two measures are presented that allow the comparison to the predictions of an ordered probit model estimating the effect of the covariates X on the ordinal variable $\hat{\pi}_j$. First, a likelihood ratio test shows that the ordered probit model is rejected in favor of all four versions of the proposed model. Moreover, I calculate a hitrate which refers to share of correctly predicted values.[25] Again, all four versions of the proposed model predict the observed outcomes substantially better than the ordered probit model. The hitrate increases between 5 and 8 percentage points, while it is larger for the alternative compared to the baseline model.

2.5.3 Adjustment Costs and Labor Market Outcomes

The estimated parameters of the model show that, when deciding about their search strategy, job seekers consider the appearance of adjustment costs once they enter a training program during the unemployment spell. Moreover, these costs can be directly influenced by the caseworker through various information treatments. However, an open question remains whether this adjustment of the search behavior is actually related to the program effectiveness. This is particularly important as it would provide the employment agency the possibility to effectively improve the labor market performance of participants in long-term training programs. In order to test the underlying mechanism empirically the estimated model is utilized to generate predictions about the level of the adjustment costs κ_a, given the set of available information Z. Based on these predictions a dummy variable indicating

[25] It is given as the mean of a variable which takesfor each individual-choice combination the value one if the actual value is one (zero) and the predicted value of the model is greater or equal (smaller) than the sample average and zero otherwise : $\frac{1}{NJ}\sum_{i=1}^{N}\sum_{j=1}^{J}\hat{\pi}_{ij}\mathbb{1}\left\{P_{ij} \geq \bar{P}_j\right\} + (1-\hat{\pi}_{ij})\,\mathbb{1}\left\{P_{ij} < \bar{P}_j\right\}$.

whether the adjustment costs are above/below the sample median is defined and each of the four groups analyzed in Section 2.4 —given by the combinations of expected and actual treatment states— is divided into a subgroup with a high, respectively low, level of adjustment costs.

Table 2.7: Adjustment Costs, Expectations and Program Effectiveness

	Exp. treatment rates $\hat{\pi}$-high v. $\hat{\pi}$-low			
	Non-participants		Participants	
	κ-low (1)	κ-high (2)	κ-low (3)	κ-high (4)
Regular employed in month $t+30$				
A. Unconditional	-0.0194 (0.0204)	0.0562*** (0.0170)	0.0091 (0.0754)	0.1221*** (0.0452)
No. of observations	1,539	2,960	262	528
B. Baseline Model	0.0331* (0.0187)	0.0270 (0.0242)	-0.0021 (0.0543)	0.2265*** (0.0604)
No. of observations	2,579	1,920	519	271
C. Alternative Model	0.0193 (0.0227)	0.0301 (0.0212)	0.0173 (0.0626)	0.1495*** (0.0579)
No. of observations	2,080	2,419	446	344
Control variables				
Socio-demographic characteristics	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Labor market histories	Yes	Yes	Yes	Yes
Regional and seasonal information	Yes	Yes	Yes	Yes
Personality traits	Yes	Yes	Yes	Yes

Note: Differences in ATTs between treated/non-treated with $\hat{\pi}$-high and treated/non-treated with $\hat{\pi}$-low separated by the level of predicted adjustment costs. Epanechnikov kernel propensity score matching with bandwidth 0.06. Standard Errors in parenthesis are obtained based on bootstrapping with 399 replications. ***/**/* indicate statistically significance at the 1%/5%/10%-level. The adjustment costs κ_a are obtained from the ML estimation depicted in Table 2.6 for the baseline, respectively the alternative model. κ_a-low (κ_a-high) denote prediction below (above) the sample median. For the unconditional case, κ_a-low (κ_a-high) characterizes individuals who report that they will (not) adjust their search behavior when the treatment is imminent.

Table 2.7 shows ATTs for the outcome variable regular employed in month 30 separated for these subsamples with low/high levels of expected adjustment costs. Since the number of observations for the groups of participants becomes relatively small due to the additional sample split, the group of non-participants with a low expected treatment rates ($\hat{\pi}$-low) is used as the unique reference group and differences with respect to the estimated ATTs are calculated. In the first specification ('unconditional'), the adjustment cost κ_a-low, respectively κ_a-high, are assumed to characterize individuals who report that they will, respectively will not, adjust their search behavior when the treatment is imminent. This specification provides a reference level where adjustment costs are endogenously predicted based on the observed willingness to adjust the search behavior. In the second, respectively third specification, the adjustment costs are predicted based on the baseline, respectively the alternative, model allowing for unobserved heterogeneity.

For non-participants, there is no clear pattern (except for the unconditional case) which is not very surprising, given that the effects of $\hat{\pi}$ are generally small and non-participants do not actually have to adjust their behavior. More interestingly, for all three specifications the positive effect of $\hat{\pi}$ on the employment probability of participants is completely driven by those individuals who are assumed to have a high level of adjustment costs. Although, this pattern is more pronounced when using the baseline model, in all cases the difference between those participants $\hat{\pi}$-high and $\hat{\pi}$-low is large and statistically significant for individuals with high adjustment costs, while it is close to zero and insignificant for individuals with low adjustment costs. For instance, using the alternative model the estimated difference is about eight times larger for the group with high adjustment costs compared to the group with low adjustment costs.

2.5.4 Discussion of Economic Implication

In summary, the findings indicate that job seekers expect significant additional costs when changing the search behavior in association with an ALMP program. This encourages those participants who correctly forecast the future treatment to choose search patterns that are more efficient when the treatment is realized and mirrors into higher long-term employment rates. Moreover, the level of the adjustment costs can be influenced by the caseworker due to different types of information treatments which implies that employment agencies can directly improve the effectiveness of long-term training programs. An important question remains, how caseworkers can influence the job seekers' expectations about future treatments, respectively reduce the expected costs of adjusting the search behavior.

It should be noted that, in 2003, Germany already introduced a reform which can be expected to affect the job seekers perception of the individual-specific treatment probability by switching to a voucher system. Before 2003, participants had been assigned to a specific training program by their caseworker, while after the reform, which has been introduced in the context of the Hartz reforms (see e.g. Jacobi and Kluve, 2007; Caliendo and Hogenacker, 2012, for an overview), job seekers are free to choose a training provider in the market. Therefore, potential participants receive a training voucher which defines the maximum duration, the target of the program and its costs. The voucher is valid for up to 3 months. However, since job seekers are free not to redeem the voucher, the new system can be expected to reduce the difference between the job seeker's perceived and the actual treatment probability and therefore increases the likelihood that potential participants choose the optimal search strategy given the future treatment status. This argument is supported by the fact that previous studies find a positive effect of the introduction of the voucher system on labor market outcomes (see e.g. Rinne et al., 2013).

However, in the present sample only about 23% of the participants already received a voucher between the entry into unemployment and the first interview, while another 15% of the participation have been informed about the possibility of participating in a training scheme by their caseworker. These numbers indicate that the majority of participants already spent several months into job search activities before discussing a participation in a training programs with the caseworker. It can be expected that the presence of this time lag reduces their willingness to adjust their search behavior.

2.6 Conclusion

The aim of the chapter was to investigate the impact of job seekers expectations about future ALMP participation on (long-term) labor market outcomes after the actual treatment status has been realized. In particular, the importance of two measures is analyzed in the context of long-term training programs in Germany: 1) the expected probability to participate in the near future and 2) the expected effect of the treatment on the labor market performance. The results show that the expected participation probability (measured at the entry into unemployment) has a positive impact on the long-term employment probabilities of individuals who experience a treatment later during the unemployment spell. However, participants' ex ante beliefs about the program effectiveness are empirically unrelated to the actual treatment effects.

Theoretical considerations suggest that the positive relationship of the expected treatment rate on the program effectiveness could have a causal interpretation if the job seekers' beliefs about the future treatment have an impact on the participants' behavior once the actual treatment has been realized. For instance, this would comprise the job search behavior during the treatment, the compliance with program conditions or the choice of program providers. The latter might be of special relevance in the case of Germany since unemployed workers have a high degree of autonomy when choosing providers for long-term training programs. However, an alternative explanation would imply that the expected participation probability is related to unobserved characteristics (other than the expected treatment effect) that in turn would have an impact on the labor market performance.

In order to understand the effect mechanisms, a further analysis of the job search behavior and related expectation measures is conducted. The findings show that the expected treatment rate is indeed related to the job search strategy. Although there are no differences with respect to search effort, unemployed workers who expect a treatment are more likely to exploit the help of their caseworker, i.e. they receive more job offers from the employment agency and more often receive information about training programs.

Moreover, it turned out that expecting a treatment is indeed associated with a higher willingness to adjust the search behavior when an ALMP program is imminent. Moreover, an extensive sensitivity analysis shows that the results are highly robust with respect to several types of unobserved heterogeneity including the job seekers ability to correctly predict future economic outcomes, the timing of the treatment and motivation. Moreover, a placebo test, considering participants in short-term training courses, shows that the finding is strongly related to a specific treatment with a sufficient program duration. Altogether, the findings indicate that pre-treatment expectations indeed have a causal effect on the participants' behavior in conjunction with the actual treatment.

Finally, the last part of the chapter presents an empirical model that predicts the job seekers process of forming expectations considering the job search strategy, as well as subjective beliefs with respect to treatment effects, reemployment probabilities and future earnings. The findings show that an adjustment of the search strategy in connection with the treatment is expected to create additional costs. Since these costs can be directly influenced by the caseworker due to different information treatments, the impact of pre-treatment expectations on the effectiveness of long-term training is directly associated with high levels of individual adjustment costs.

The findings of the chapter give new insights into the job search process of unemployed workers and indicate that the German System of ALMP programs provides substantial room for improvement when assigning job seekers to ALMP programs. For very costly long-term training programs, it seems to be important that potential participants receive the information about upcoming treatments very early during the unemployment spell. For instance, informing the job seeker about the possibility of a future treatment or awarding a training voucher, reduces the degree of uncertainty, allows the job seeker to choose the optimal search pattern and therefore increases the effectiveness of a realized treatment. However, it should be also noted that facing the threat of being treated might also encourage job seekers to leave unemployment early which implies that some degree of uncertainty could also have positive implications for the welfare state.

2.7 Technical Appendix

2.7.1 Details on the Job Search Model

Expected Search Strategy in $t+1$**:** Based on the inter-temporal value function of unemployment, given by Equation 2.1, the expected search strategy in the next period $t+1$ is characterized by the following condition:

$$(1-\hat{\pi})\frac{\partial V_{t+1}^{u}(s_t, \hat{s}_{t+1})}{\partial s_{t+1}} + \pi\frac{\partial V_{t+1}^{p}(s_t, \hat{s}_{t+1})}{\partial s_{t+1}} = 0. \tag{2.8}$$

Hence, the expected search strategy for the next period $\hat{s}_{t+1}$ can be expressed as a function of the expected participation probability and the search strategy of the current period. In general, it is true that $\hat{s}_{t+1}(s_t, \hat{\pi}) \neq s_{t+1}^{*}(s_t)$ which means the expected search strategy differs from that optimally strategy once the true treatment status would be realized depending on the expected treatment rate $\hat{\pi}$. Assuming that

$$\frac{\partial c}{\partial s_t} > 0, \quad \frac{\partial^2 c}{\partial s_t^2} > 0, \quad \frac{\partial \lambda}{\partial s_t} > 0 \quad \text{and} \quad \frac{\partial^2 \lambda}{\partial s_t^2} < 0, \tag{2.9}$$

and s characterizes the search effort, it would follow that the expected effort level $\hat{s}_{t+1}$ would be high if $\hat{\pi}$ is high, respectively the marginal returns to job search are large when being in a program in the next period compared to being unemployed, respectively if $\hat{\pi}$ is low and the marginal returns to job search are large when being unemployed compared to being in a program in the next period, and vice versa.

From 2.9 follows that

$$(1-\pi)\frac{\partial^2 V_{t+1}^{u}(s_t, \hat{s}_{t+1})}{\partial s_{t+1}^2} + \pi\frac{\partial^2 V_{t+1}^{p}(s_t, \hat{s}_{t+1})}{\partial s_{t+1}^2} < 0, \tag{2.10}$$

is sufficient to ensure that 2.8 characterizes a maximum. Therefore, the effect of the expected treatment rate $\hat{\pi}$ on the expected search strategy for the subsequent period $t+1$ can be derived as:

$$\frac{\partial \hat{s}_{t+1}}{\partial \hat{\pi}} = -\frac{\frac{\partial V_{t+1}^{p}(s_t,\hat{s}_{t+1})}{\partial s_{t+1}} - \frac{\partial V_{t+1}^{u}(s_t,\hat{s}_{t+1})}{\partial s_{t+1}} + (1-\hat{\pi})\frac{\partial^2 V_{t+1}^{u}(s_t,\hat{s}_{t+1})}{\partial s_{t+1}\partial \hat{\pi}}}{\hat{\pi}(1-\hat{\pi})\frac{\partial^2 V_{t+1}^{u}(s_t,\hat{s}_{t+1})}{\partial s_{t+1}^2} + \frac{\partial^2 V_{t+1}^{p}(s_t,\hat{s}_{t+1})}{\partial s_{t+1}^2}} \tag{2.11}$$

Given condition 2.10, the denominator of Equation 2.11 is negative and therefore

$$\frac{\partial V_t^{p}(s_t, \hat{s}_{t+1})}{\partial s_{t+1}} > \frac{\partial V_t^{u}(s_t, \hat{s}_{t+1})}{\partial s_{t+1}} - (1-\hat{\pi})\frac{\partial^2 V_t^{u}(s_t, \hat{s}_{t+1})}{\partial s_{t+1}\partial \hat{\pi}} \tag{2.12}$$

is sufficient to ensure that $\dfrac{\partial \hat{s}_{t+1}}{\partial \hat{\pi}} > 0$.

Assuming that ${\partial c}/{\partial s_t} > 0$, ${\partial^2 c}/{\partial s_t^2} > 0$, ${\partial \lambda}/{\partial s_t} > 0$, ${\partial^2 \lambda}/{\partial s_t^2} < 0$, the search strategy of an agent i in period t when the actual treatment status has not yet been realized is given as $s_i > s_j$ if $\hat{\pi}_i > \hat{\pi}_j$ and ${\partial R^p_{t+1}}/{\partial s_t} > {\partial R^u_{t+1}}/{\partial s_t} - (1-\hat{\pi}){\partial^2 R^u_{t+1}}/{\partial s_t \partial \hat{\pi}}$ and vice versa.

Actual Search Strategy in t**:** For *IC2* characterizing a maximum with respect to the inter-temporal utility it is sufficient that:

$$\frac{\partial^2 c(\tilde{s}_t)}{\partial s_t^2} + \frac{\partial^2 \lambda(\tilde{s}_t)}{\partial s_t^2} + \hat{\pi}\frac{\partial^2 R^p_{t+1}(\tilde{s}_t, \hat{\pi})}{\partial s_t^2} + (1-\hat{\pi})\frac{\partial^2 R_{t+1}(\tilde{s}_t, \hat{\pi})}{\partial s_t^2} < 0. \tag{2.13}$$

Based on *IC2*, the effect of the expected participation probability on the search behavior in the initial period can be derived as:

$$\frac{\partial \tilde{s}_t}{\partial \hat{\pi}} = -\frac{\frac{\partial R^p_{t+1}(\tilde{s}_t,\hat{\pi})}{\partial s_t} - \frac{\partial R_{t+1}(\tilde{s}_t,\hat{\pi})}{\partial s_t} + (1-\hat{\pi})\frac{\partial^2 R_{t+1}(\tilde{s}_t,\hat{\pi})}{\partial s_t \partial \hat{\pi}}}{\frac{\partial^2 c(\tilde{s}_t)}{\partial s_t^2} + \frac{\partial^2 \lambda(\tilde{s}_t)}{\partial s_t^2} + \hat{\pi}\frac{\partial^2 R^p_{t+1}(\tilde{s}_t,\hat{\pi})}{\partial s_t^2} + (1-\hat{\pi})\frac{\partial^2 R_{t+1}(\tilde{s}_t,\hat{\pi})}{\partial s_t^2}}, \tag{2.14}$$

where condition 2.13 guarantees that the denominator is negative and the following condition

$$\frac{\partial R^p_{t+1}(\tilde{s}_t, \hat{\pi})}{\partial s_t} > \frac{\partial R_{t+1}(\tilde{s}_t, \hat{\pi})}{\partial s_t} - (1-\hat{\pi})\frac{\partial^2 R_{t+1}(\tilde{s}_t, \hat{\pi})}{\partial s_t \partial \hat{\pi}} \tag{2.15}$$

ensures that $\dfrac{\partial \tilde{s}_t}{\partial \hat{\pi}} > 0$.

Assuming that ${\partial R^u_{t+1}}/{\partial s_t} \neq {\partial R^p_{t+1}}/{\partial s_t}$ and ${\partial^2 R^p_{t+1}}/{\partial s_t \partial s_{t+1}} \neq 0$, an agents search behavior during a treatment is given a function of her expected participation rate before the treatment has been realized: $s^*_{t+1} = g(\hat{\pi}, \tilde{s}_t(\hat{\pi}))$.

2.7.2 Sensitivity of Empirical Findings

Group Classification with respect to Expected Treatment Rates: For the main analysis in Section 2.4.2, two groups ($\hat{\pi}-$low and $\hat{\pi}-$high) are defined, while $\hat{\pi} \geq 5$ is chosen as the threshold that distinguish those individuals expecting the treatment from thos who do not expect the treatment. Although the same threshold is used in previous studies (see van den Berg et al., 2009), this is an arbitrary choice and it is important to analyze the sensitivity of the findings with respect to the categorization based on $\hat{\pi}$. Therefore, Table 2.8 shows estimation results for an alternative classification that allows for five (instead of two) levels of expectations for each treatment group. Three groups are given by the points zero, five and ten which include the majority of the reported perceived treatment rates, while two additional categories summarize the intermediate answers 1-4, respectively 6-9. It should be noted that the size of the five groups of participants becomes relatively small. Therefore, non-participants who report a very low expected treatment rate ($\hat{\pi} = 0$) are used as the unique reference group and ATTs on the labor market outcomes, as well as the willingness to increase the search effort, are estimated with respect to this reference group. When considering participants, there is huge difference with respect to the estimated ATTs between those individuals with $\hat{\pi} < 5$ and $\hat{\pi} \geq 5$, while the effects within the two baseline groups ($\hat{\pi}-$low and $\hat{\pi}-$high) are relatively similar. Therefore, it can be concluded that for participants 5 is indeed the relevant threshold. It should be noted that the overall pattern suggest the presence of non-linear, especially driven by individuals who report expected treatment rates $6 \geq \hat{\pi} \geq 9$. However, due to the small sample size, this has to be interpreted with caution.

Comparison of Alternative Choice Models: The following section provides additional information with respect to the estimation of the propensity scores that are utilized in order to estimate the average treatment effects on the treated as presented in Section 2.4. Marginal effects are presented in Table 2.9 for the sequence of the reduced-form logit models and in Table 2.10 for the three nested logit model type I. It should be noted that the effects are not directly comparable. Due to its binary nature, for the reduced-form logit models, it depicts the marginal effect of X on the probability of switching from the reference group to the treatment group of interest. However, for the nested logit model, it expresses the marginal effect of X on the likelihood of choosing the specific group of interest instead of one of all the other groups.

Moreover, in order to provide a more profound understanding of the selection process, additional evidence with respect to the predicted propensity scores and the corresponding rank distribution is presented for the two models. Figure 2.4 shows the relevant propensity score distribution for three treatment groups and the corresponding reference group for each of the choice models. It can be seen that, except for a few individuals at the at the limit, there is huge overlap in the distribution. To compare the estimated propensity scores of the two choice models, Table 2.11 shows three correlation coefficients for the estimated propensity score, respectively the corresponding relative ranks. The small correlation coefficients, especially for participants, indicate that using a different choice model has a strong impact on the score distribution and can be interpreted as evidence the underlying types of unobserved heterogeneity (imposed by the nesting structures, e.g. depicted in Table 2.5) are relevant for the expected and actual selection into the treatment. However, given the small differences with respect to the estimated ATTs, these unobserved characteristics seem to have only a limited impact on the labor market outcomes.

Altering Matching Algorithms: In order to analyze the sensitivity of the estimation results with respect to the choice of the matching procedure, Table 2.12 shows estimated ATTs based on alternative estimators. In particular, I focus four types of kernel matching with different bandwidths (0.006, 0.02, 0.06 and 0.2), two types of radius matching with a caliper of 0.02, respectively 0.1, and one-to-four nearest neighbor matching. Moreover, the results are presented for propensity scores obtained based on the reduced-form logit and the nested logit model type I. It can be seen that, irrespective of the choice of the matching procedure, there is positive and statistically significant effect of expecting a treatment on the long-term employment probability (30 months after the entry into unemployment) of actual participants varying between 8 and 10 percentage points. For non-participants the effect is smaller (2.5 to 3.0 percentage points) but also significant at least at the 10%-level in 11 out of 14 cases. When considering the cumulated effect over the observation period of 30 months, it should be noted that the estimates become somewhat imprecise. However, when using propensity scores based on the nested logit model the effect is statistically significant at the 10%-level in all cases and varies between 1.1 and 1.4 months. The effect for non-participants is slightly smaller but, due to the larger sample size, highly significant for all estimators.

Effort Adjustment and Expected Treatment Effects: The estimation results presented in Section 2.4.3 show that job seekers who expect a treatment show a higher willingness to adjust their search behavior when an ALMP program is imminent. This variable is expected to proxy the flexibility of the job seekers search strategy in association with ALMP programs and is used in order to predict the expected behavior after the treatment has been realized in Section 2.5.1. However, given the wording of the corresponding survey question it is unclear whether this interpretation is justified. As the question implies that the job seekers actually adjust their search behavior before the beginning of the ALMP program it could be the case that the answer only reflects the agent's willingness to actually enter the program, respectively to avoid the treatment, by changing the search effort correspondingly.

It should be noted that for the plausibility of the empirical model neither the timing of nor the reason for the expected effort adjustment plays a crucial role, as long as the adjustment creates additional costs and is related to the expected treatment rate $\hat{\pi}$. However, in order to explain the different long-term employment rates of participants with respect to $\hat{\pi}$, the adjustment has to have an impact on the agent during the actual treatment. This assumption could be potentially violated if the agent expects to adjust the search behavior only in the interim period between the notification about an upcoming treatment and the actual program start, while she expects to fall back on the initial search strategy without any costs during the treatment. If this would be the case, it can be assumed that the willingness to adjust the search effort reflects the agent's presumption about the treatment effects. However, as shown in Table 2.13, this is actually not the case since job seekers who expect the treatment to be beneficial (indicated by $\hat{\delta}$-high) show a higher willingness to increase the search effort when the treatment is imminent, while those with a less positive perception of the treatment effect expect to keep their search effort constant. This finding contradicts the view that the variable characterizes only the agents expected behavior between the notification and the actual treatment (in other words the size of the threat effect), but rather suggests that it proxies the agent's overall flexibility of the job search strategy.

Table 2.8: Sensitivity Analysis: Alternative Categorization with Respect to Expected Treatment Rate $\hat{\pi}$

		Non-participants			Participants		
		ATT	SE	Obs.	ATT	SE	Obs.
A. Regular employed in month t_{12}							
Expected treatment rate							
very low:	$\hat{\pi} = 0$	ref.		1,411	-0.2313***	(0.0437)	130
medium low:	$\hat{\pi} \in (1, 4)$	-0.0110	(0.0204)	811	-0.2421***	(0.0524)	93
medium:	$\hat{\pi} = 5$	-0.0159	(0.0253)	650	-0.1150**	(0.0578)	102
medium high:	$\hat{\pi} \in (6, 9)$	0.0412*	(0.0220)	1,027	-0.1684***	(0.0406)	186
very high:	$\hat{\pi} = 10$	0.0263*	(0.0260)	600	-0.0983***	(0.0346)	279
B. Regular employed in month t_{30}							
Expected treatment rate							
very low:	$\hat{\pi} = 0$	ref.		1,411	-0.0501	(0.0484)	130
medium low:	$\hat{\pi} \in (1, 4)$	-0.0009	(0.0230)	811	-0.0751	(0.0484)	93
medium:	$\hat{\pi} = 5$	0.0052	(0.0250)	650	0.0405	(0.0566)	102
medium high:	$\hat{\pi} \in (6, 9)$	0.0427*	(0.0221)	1,027	0.0182	(0.0435)	186
very high:	$\hat{\pi} = 10$	0.0026	(0.0267)	600	0.0426	(0.0359)	279
C. Cumulated effect ($\sum_{t=0}^{30}$, months)							
Expected treatment rate							
very low:	$\hat{\pi} = 0$	ref.		1,411	3.6468***	(0.8645)	130
medium low:	$\hat{\pi} \in (1, 4)$	-0.1110	(0.4503)	811	-3.5267***	(1.0492)	93
medium:	$\hat{\pi} = 5$	0.0466	(0.5057)	650	-2.0435*	(1.0801)	102
medium high:	$\hat{\pi} \in (6, 9)$	1.2576***	(0.4477)	1,027	-2.9553***	(0.8405)	186
very high:	$\hat{\pi} = 10$	0.3763	(0.5570)	600	-1.7988***	(0.6798)	279
D. Cumulated earnings ($\sum_{t=0}^{30}$, in €)							
Expected treatment rate							
very low:	$\hat{\pi} = 0$	ref.		1,411	-4888.0***	(1430.6)	130
medium low:	$\hat{\pi} \in (1, 4)$	-478.2	(792.7)	811	-1867.7	(1903.8)	93
medium:	$\hat{\pi} = 5$	-1156.6	(774.7)	650	-2741.6*	(1633.1)	102
medium high:	$\hat{\pi} \in (6, 9)$	1184.5*	(706.7)	1,027	-3533.4***	(1228.7)	186
very high:	$\hat{\pi} = 10$	124.7	(838.0)	600	-2017.2*	(1080.1)	279
E. Average earnings (€/month)							
Expected treatment rate							
very low:	$\hat{\pi} = 0$	ref.		1,411	344.6	(310.2)	130
medium low:	$\hat{\pi} \in (1, 4)$	-76.0	(53.9)	811	43.5	(160.8)	93
medium:	$\hat{\pi} = 5$	-168.4***	(54.3)	650	28.8	(126.4)	102
medium high:	$\hat{\pi} \in (6, 9)$	-59.6	(54.4)	1,027	38.1	(121.8)	186
very high:	$\hat{\pi} = 10$	-58.0	(60.8)	600	81.3	(81.1)	279
F. Will increase search effort when ALMP program is imminent							
Expected treatment rate							
very low:	$\hat{\pi} = 0$	ref.		1,411	-0.0259	(0.0390)	130
medium low:	$\hat{\pi} \in (1, 4)$	0.0145	(0.0212)	811	-0.0194	(0.0514)	93
medium:	$\hat{\pi} = 5$	0.0598***	(0.0231)	650	0.0480	(0.0562)	102
medium high:	$\hat{\pi} \in (6, 9)$	0.0951***	(0.0225)	1,027	0.0516	(0.0397)	186
very high:	$\hat{\pi} = 10$	0.1154***	(0.0264)	600	0.0939***	(0.0345)	279

Note: Depicted are average treatment effects on the treated (ATT) as the difference in mean outcomes between treated and matched controls using Epanechnikov kernel propensity score matching with bandwidth 0.06. In each case the control group contains non-participants with very low expected treatment rates $\hat{\pi} = 0$ (with $N = 1,411$), while the depicted number of observations refers to the corresponding treatment group only. Standard errors are in parentheses and based on bootstrapping with 399 replications. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Table 2.9: Estimation of Propensity Scores: Marginal Effects of Pairwise Logit Models for $\hat{\pi}$

	$(D_0\|\hat{\pi}$-high) v. $(D_0\|\hat{\pi}$-low)	$(D_1\|\hat{\pi}$-low) v. $(D_0\|\hat{\pi}$-low)	$(D_1\|\hat{\pi}$-high) v. $(D_0\|\hat{\pi}$-low)	$(D_1\|\hat{\pi}$-low) v. $(D_0\|\hat{\pi}$-high)	$(D_1\|\hat{\pi}$-high) v. $(D_0\|\hat{\pi}$-high)	$(D_1\|\hat{\pi}$-low) v. $(D_1\|\hat{\pi}$-high)
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0052	0.0086	0.0152	0.0089	0.0221	0.0077
Age (Ref.: 16-24 years)						
25-34 years	-0.0550**	0.0243	0.0545*	0.0350*	0.0750***	-0.0203
35-44 years	-0.1093***	-0.0042	0.0967***	0.0229	0.1498***	-0.1301**
45-55 years	-0.1314***	0.0189	0.0558*	0.0572*	***0.1242***	-0.0300
School leaving degree (Ref.: None)						
Lower sec. degree	-0.0170	0.0150	0.0426	0.0165	0.0473	0.0357
Middle sec. degree	-0.0247	0.0117	0.0606	0.0165	0.0693	-0.0001
(Spec.) Upper sec. degree	-0.0927*	0.0193	0.0149	0.0574	0.0683	0.0936
Higher education (Ref.: None)						
Internal/external prof. training	-0.0055	-0.0019	-0.0028	0.0018	0.0002	-0.0170
University degree	-0.0667**	-0.0252	-0.0313	-0.0076	0.0159	-0.0648
German citizenship	-0.0224	0.0448	-0.0140	0.0522	-0.0058	0.1646
Migration background	0.0853***	0.0446*	0.0514*	0.0247	-0.0120	0.0553
Married or cohabiting	-0.0106	-0.0292**	-0.0146	-0.0218*	-0.0098	-0.0306
Children (Ref.: None)						
One child	0.0050	0.0205	0.0125	0.0181	0.0087	0.0391
Two children or more	0.0235	0.0374	0.0333	0.0270	0.0156	0.0344
Problems with childcare	0.0107	-0.0213	0.0019	-0.0194	-0.0134	-0.0362
Partner is full-time employed	0.0220	-0.0066	0.0200	-0.0114	-0.0040	-0.0408
Searching for full-time employment	0.0393**	0.0111	-0.0196	-0.0043	-0.0517***	0.0740**
Region (Ref.: West & UE rate 0-6%)						
West & UE rate 6+%	0.0322*	0.0163	-0.0026	0.0071	-0.0284	0.0462
East & UE rate 9-14%	-0.0201	-0.0028	-0.0586**	0.0048	-0.0481*	0.0677
East & UE rate 15+%	-0.0589**	0.0283	-0.0463**	0.0578**	-0.0130	0.1312**
Entry into unemployment (Ref.: 2nd quarter 2007)						
3rd quarter 2007	0.0166	-0.0534***	-0.0028	-0.0559***	-0.0134	-0.1062
4th quarter 2007	0.0011	-0.0350	0.0188	-0.0279	0.0178	*-0.1022*
1st quarter 2008	0.0293	-0.0385	0.0340	-0.0422*	0.0083	-0.0997
2nd quarter 2008	0.0071	-0.0196	0.0364	-0.0182	0.0264	-0.1002
Time to interview (Ref.: 7 weeks)						
8 weeks	0.0796	-0.0181	0.0766	-0.0527	-0.0027	-0.1082
9 weeks	0.1151*	-0.0168	0.1093	-0.0622	0.0055	-0.1381
10 weeks	0.0570	-0.0120	0.0959	-0.0367	0.0350	-0.0864
11 weeks	0.0879	-0.0236	0.0994	-0.0612	0.0064	-0.1132
12 weeks	0.0393	-0.0206	0.0465	-0.0463	-0.0190	-0.0587
13 weeks	0.1046	-0.0528	0.1457	-0.0917**	0.0357	***-0.2591***
14 weeks or more	0.0843	-0.0439	0.0763	-0.0746*	0.0116	-0.1472
Unemployment benefit recipient	0.0273	-0.0114	0.0505**	-0.0202	0.0336	-0.1089***
Last daily income in €	-0.0005*	0.0000	-0.0004	0.0002	-0.0001	0.0005
Employment status before unemployment (Ref.: Other)						
Regular employment	-0.0017	0.0190	-0.0125	0.0233	-0.0074	*0.0790*
Subsidized employment	-0.0023	0.0085	-0.0308	0.0138	-0.0442	0.1062
Last job was full-time employment	-0.0076	0.0052	0.0273	0.0073	0.0433	0.0081
Months in employment						
in last year	0.0011	-0.0022	-0.0008	-0.0026	-0.0028	-0.0031
in last 5 years	0.0008	0.0004	0.0023*	-0.0001	0.0020	-0.0034
in last 10 years	0.0003	0.0003	-0.0008	0.0003	-0.0010	0.0023
Months in unemployment						
in last year	-0.0008	-0.0056	-0.0114**	-0.0053	-0.0114**	0.0021
in last 5 years	-0.0014	0.0017*	0.0017	0.0019**	0.0031**	0.0011
in last 10 years	-0.0022**	-0.0007	-0.0037***	0.0001	-0.0023**	0.0028
Openness (standardized)	0.0328***	-0.0016	0.0040	-0.0121*	0.0192**	-0.0028
Conscientiousness (standardized)	0.0094	0.0018	0.0223**	-0.0021	*0.0150*	-0.0277
Extraversion (standardized)	-0.0054	-0.0135**	-0.0158*	-0.0146**	-0.0124	-0.0122
Neuroticism (standardized)	-0.0120	-0.0023	-0.0031	-0.0021	0.0042	-0.0062
Locus of control (standardized)	-0.0064	-0.0009	-0.0077	0.0010	-0.0061	0.0054
Observations	4499	2445	2789	2500	2844	790
Hitrate	0.6012	0.5992	0.6131	0.6404	0.5960	0.6392
log-Likelihood	-2976.9	-721.8	-1344.0	-702.1	-1359.2	-432.3

Note: Depicted are average marginal effects for a sequence of logit models comparing each combination of expected and actual treatment states. D_0 indicates non-participants; D_1 indicates participants. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Table 2.10: Estimation of Propensity Scores: Marginal Effects of Nested Logit Model Type I

	Non-participants				Participants	
Expected treatment rate	$\bar{\pi}$−low	$\bar{\pi}$−low	$\bar{\pi}$−high	$\bar{\pi}$−high	$\bar{\pi}$−low	$\bar{\pi}$−high
Job finding (expected ≡ actual)	yes	no	yes	no		
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.0091	-0.0032	-0.0045	-0.0064	-0.0041	0.0092
Age (Ref.: 16-24 years)						
25-34 years	0.0372*	0.0446***	-0.0511***	-0.0242	0.0159	-0.0223
35-44 years	0.0387*	0.0796***	-0.0894***	-0.0395*	0.0190*	-0.0084
45-55 years	0.0435*	0.1227***	-0.1504***	-0.0353	0.0253*	-0.0059
School leaving degree (Ref.: None)						
Lower sec. degree	0.0202	-0.0028	-0.0082	-0.0139	0.0079	-0.0032
Middle sec degree	0.0174	-0.0066	-0.0068	-0.0074	0.0144	-0.0109
(Spec.) Upper sec. degree	0.0279	0.0211	-0.0377	-0.0200	0.0184	-0.0097
Higher education (Ref.: None)						
Internal/external prof. training	0.0397*	-0.0386**	0.0277	-0.0326*	-0.0112	0.0150
University degree	0.0719**	-0.0339	0.0095	-0.0591***	-0.0122	0.0238
German citizenship	0.0176	0.0117	-0.0233	-0.0036	0.0276	-0.0300
Migration background	-0.0161	-0.0422**	0.0426***	0.0268	-0.0056	-0.0054
Married or cohabiting	-0.0012	0.0014	-0.0019	0.0046	0.0017	-0.0046
Children (Ref.: None)						
One child	-0.0141	0.0073	0.0010	0.0013	-0.0013	0.0057
Two children or more	-0.0095	-0.0031	-0.0094	0.0166	-0.0041	0.0095
Problems with childcare	0.0011	-0.0052	0.0072	-0.0072	-0.0148	0.0188
Partner is full-time employed	-0.0177	-0.0085	0.0201	0.0043	-0.0061	0.0080
Searching for full-time employment only	-0.0026	-0.0183	0.0315**	-0.0074	-0.0042	0.0011
Region (Ref.: West & UE rate 0-6%)						
West & UE rate 6+%	-0.0249*	-0.0027	-0.0054	0.0373**	0.0011	-0.0055
East & UE rate 9-14%	0.0013	0.0191	-0.0284***	0.0039	0.0087	-0.0047
East & UE rate 15+%	-0.0013	0.0527***	-0.0848***	0.0305*	0.0143	-0.0114
Entry into unemployment (Ref.: 2nd quarter 2007)						
3rd quarter 2007	0.0156	-0.0165	0.0108	-0.0163	-0.0185**	0.0248
4th quarter 2007	0.0386	-0.0314	0.0060	-0.0256	-0.0011	0.0135
1st quarter 2008	0.0366	-0.0379	0.0302	-0.0321	-0.0055	0.0087
2nd quarter 2008	0.0318	-0.0355	0.0209	-0.0228	0.0010	0.0046
Time to interview (Ref.: 7 weeks)						
8 weeks	-0.0691	-0.0110	0.0237	0.0481	-0.0169	0.0252
9 weeks	-0.0867**	-0.0290	0.0287	0.0696	-0.0196	0.0370
10 weeks	-0.0753*	0.00698	-0.0108	0.0518	-0.0126	0.0399
11 weeks	-0.0977**	0.00819	-0.00239	0.0768	-0.0207	0.0358
12 weeks	-0.0737	0.0345	-0.0288	0.0700	-0.0166	0.0146
13 weeks	-0.0651	-0.0379	-0.0137	0.0974	-0.0361**	0.0554
14 weeks or more	-0.0922*	0.0166	-0.0403	0.1142*	-0.0286	0.0306
Unemployment benefit recipient	0.0055	-0.0113	0.0133	-0.0174	-0.0078	0.0177
Last daily income in €	0.0006***	-0.0001	0.0000	-0.0006***	0.0001	0.0000
Employment status before unemployment (Ref.: Other)						
Regular employment	0.0041	0.0047	-0.0100	0.0034	0.0094	-0.0115
Subsidized employment	-0.0065	0.0095	-0.0122	0.0269	0.0174	-0.0351**
Last job was full-time employment	-0.0046	-0.0190	0.0225	-0.0161	-0.0103	0.0275
Months in employment						
in last year	-0.0015	-0.0004	0.0013	0.0006	-0.0010	0.0011
in last 5 years	-0.0005	0.0001	0.0008	0.0001	-0.0000	-0.0004
in last 10 years	0.0002	-0.0008	0.0005	-0.0001	-0.0002	0.0005
Months in unemployment						
in last year	0.0016	0.0034	0.0046	-0.0008	-0.0023	**-0.0065**
in last 5 years	0.0015	-0.0014	0.0003	-0.0026**	0.0008*	*0.0013*
in last 10 years	0.0006	0.0022***	-0.0018***	0.0006	-0.0001	-0.0016***
Openness (standardized)	-0.0222***	0.0011	0.0055	0.0186***	-0.0013	-0.0016
Conscientiousness (standardized)	-0.0026	-0.0060	0.0048	0.0039	-0.0019	0.0017
Extraversion (standardized)	-0.0010	0.0046	-0.0050	0.0029	0.0022	-0.0037
Neuroticism (standardized)	0.0092	-0.0076	0.0065	-0.0105*	-0.0025	0.0048
Locus of control (standardized)	0.0071	-0.0011	0.0010	-0.0060	-0.0000	-0.0010
No. of observations by group	1,184	1,093	1,201	1,021	223	567
Hitrate	0.6037	0.6105	0.5745	0.5798	0.5849	0.5733
Category 1st hierarchy level (ability)	High	Low	High	Low	Low	High

Note: Depicted are average marginal effects for the nested logit model according to the tree structure depicted in Table 2.5. ***/**/* indicate statistically significance at the 1%/5%/10%-level. $N_{\text{total}} = 5,289$; $P-$ value (LR-test for IIA) = 0.000; log-Likelihood= $-8,564.2$.

Table 2.11: Propensity Score and Rank Correlation

	Non-participants	Participants
	(1)	(2)
Propensity score correlation		
Pearson's r	0.9179	0.4267
Rank correlation		
Spearman's rho	0.9193	0.4165
Kendall's tau	0.7503	0.2868

Note: Depicted are correlation coefficients comparing the propensity score distribution of expecting the treatment $\hat{\pi}$-high when using the reduced-form logit model (see Table 2.9), respectively the nested logit model type I (see Table 2.10 and Table 2.5).

Table 2.12: Sensitivity Analysis: Alternative Matching Algorithms

	Exp. treatment rates $\hat{\pi}$-high v. $\hat{\pi}$-low			
	Logit model		Nested logit model type I	
	Non-participants	Participants	Non-participants	Participants
	(1)	(2)	(3)	(4)
A. Regular employed in month t_{30}				
Kernel matching ($bw = 0.006$)	0.0275*	0.1009**	0.0233	0.0823*
	(0.0154)	(0.0460)	(0.0154)	(0.0421)
Kernel matching ($bw = 0.02$)	0.0302**	0.0801*	0.0270*	0.0799**
	(0.0152)	(0.0434)	(0.0154)	(0.0393)
Kernel matching ($bw = 0.06$)	0.0306*	0.0924**	0.0278*	0.0843**
	(0.0163)	(0.0411)	(0.0151)	(0.0375)
Kernel matching ($bw = 0.2$)	0.0254*	0.0958**	0.0274*	0.0906**
	(0.0150)	(0.0390)	(0.0150)	(0.0363)
Radius matching ($c = 0.02$)	0.0278*	0.0956**	0.0271*	0.0879**
	(0.0150)	(0.0398)	(0.0151)	(0.0364)
Radius matching ($c = 0.1$)	0.0303**	0.0773*	0.0281*	0.0800**
	(0.0151)	(0.0430)	(0.0153)	(0.0388)
Nearest neighbor matching (1:4)	0.0246	0.1020**	0.0236	0.0886**
	(0.0177)	(0.0497)	(0.0153)	(0.0450)
B. Cumulated effect ($\sum_{t=0}^{30}$, months)				
Kernel matching ($bw = 0.006$)	0.8923***	1.2314	0.8289*	1.3717*
	(0.3393)	(0.8080)	(0.3301)	(0.7489)
Kernel matching ($bw = 0.02$)	0.9173***	0.9517	0.9107***	1.2997*
	(0.3331)	(0.7641)	(0.3306)	(0.6917)
Kernel matching ($bw = 0.06$)	0.8795***	1.1456	0.8802***	1.1610*
	(0.3382)	(0.7195)	(0.3316)	(0.6576)
Kernel matching ($bw = 0.2$)	0.8115**	1.2867*	0.8351**	1.2068*
	(0.3262)	(0.6644)	(0.3262)	(0.6375)
Radius matching ($c = 0.02$)	0.8449***	1.1765*	0.8406**	1.1498*
	(0.3291)	(0.6847)	(0.3298)	(0.6376)
Radius matching ($c = 0.1$)	0.9154***	0.9413	0.9227***	1.3180*
	(0.3331)	(0.7566)	(0.3307)	(0.6783)
Nearest neighbor matching (1:4)	0.8943**	1.0930	0.8163**	1.3789*
	(0.3782)	(0.8609)	(0.3599)	(0.8184)
No. of observations	4,499	790	4,499	790
Control variables				
Socio-demographic characteristics	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Labor market histories	Yes	Yes	Yes	Yes
Regional and seasonal information	Yes	Yes	Yes	Yes
Personality traits	Yes	Yes	Yes	Yes

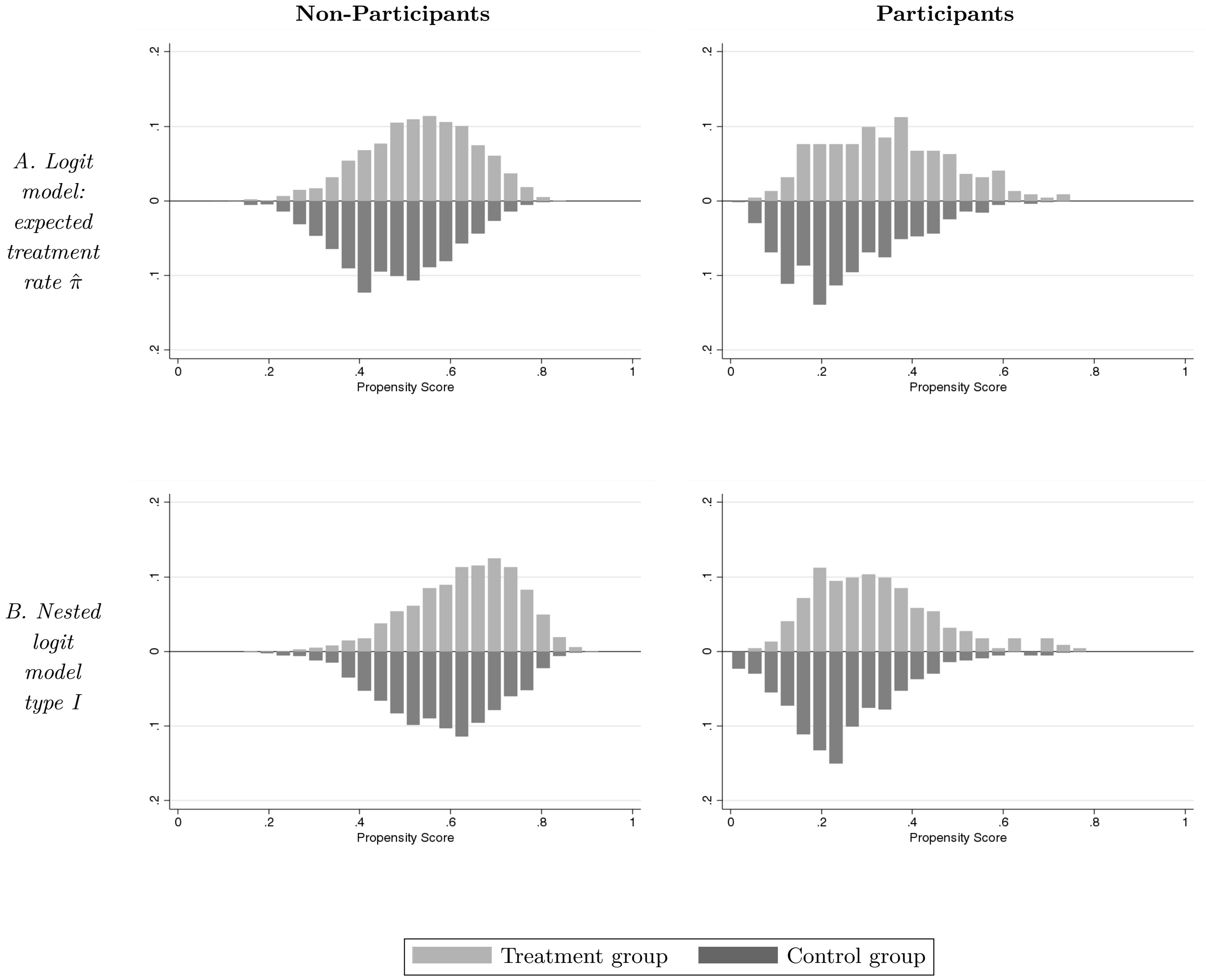
Note: Depicted are average treatment effects on the treated (ATT) as the difference in mean outcomes between treated and matched controls using alternative matching algorithms: Epanechnikov kernel propensity score matching with bandwidth (*bw*) 0.006, 0.02, 0.06 and 0.2; radius matching with a caliper (*c*) of 0.02 and 0.1; one-to-four nearest neighbor matching. Standard errors are in parentheses and based on bootstrapping with 399 replications. Treated and controls are defined based on $\hat{\pi}$ separated for non-participants and participants. Italic numbers: ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Table 2.13: Willingness to Adjust Search Effort and Expected Treatment Effects

	$\hat{\delta}-$low	$\hat{\delta}-$high	$P-$value
A. Non-participants			
No. of observations	3,291	1,208	
Expected adjustment of search behavior			
will increase search effort	0.27	0.41	0.00
will keep search effort constant	0.70	0.55	0.00
will decrease search effort	0.03	0.04	0.31
B. Participants			
No. of observations	423	367	
Expected adjustment of search behavior			
will increase search effort	0.22	0.35	0.00
will keep search effort constant	0.74	0.58	0.00
will decrease search effort	0.04	0.07	0.06

Note: Depicted are answers to the question: *"To what extent would your search activities change when you know that you could/must participate in an ALMP program within the next 2 months?"* Percentage share unless indicated otherwise. $P-$values measured based on two-tailed t-tests on equal means.

Figure 2.4: Propensity Score Distributions



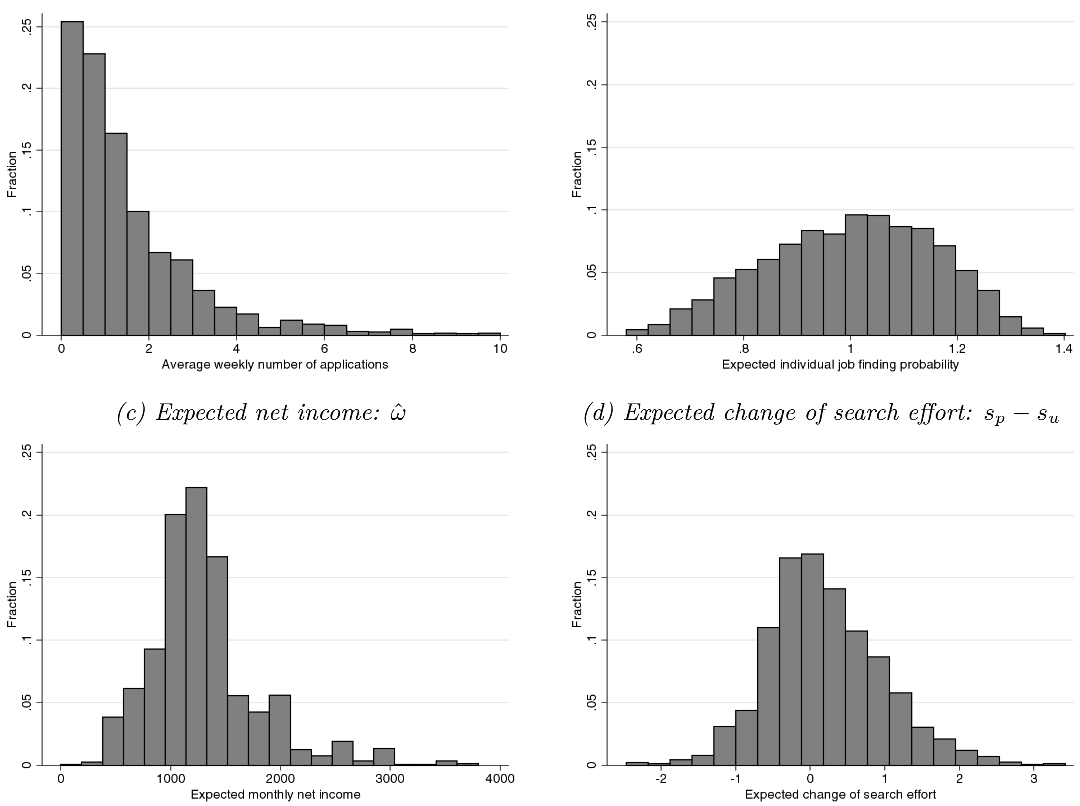
Note: Depicted are propensity score distributions for separated for treated and controls using different choice models. The group with the larger sample size is always defined as the control group (non-participants: $\hat{\pi}$-low; participants: $\hat{\pi}$-high), while the group with the smaller sample size is defined as the treatment group (non-participants: $\hat{\pi}$-high; participants: $\hat{\pi}$-low).

2.8 Supplementary Figures and Tables

Figure 2.5: Distribution of Search Characteristics and Expectations

(a) Realized search effort at first interview: s_u *(b) Expected job finding rate (individual-specific)*

(c) Expected net income: $\hat{\omega}$ *(d) Expected change of search effort:* $s_p - s_u$



Note: Depicted are the distributions of the main search characteristics and expectation measures utilized for the estimation of the expected value functions. While search effort (a) and the expected income (c) are directly observed in the data, the predictions of the expected job finding rates (b) and the expected change of the search effort (d) are obtained from estimates depicted in Table 2.17.

Table 2.14: Descriptive Statistics by Expectations and Treatment Status

	Non-participants			Participants		
Expectations	$\hat{\pi}-$low	$\hat{\pi}-$high	$P-$value	$\hat{\pi}-$low	$\hat{\pi}-$high	$P-$value
No. of observations	2,222	2,277		223	567	
A. Baseline control variables X						
Socio-demographic characteristics						
Female	0.48	0.47	0.52	0.46	0.55	0.03
Age in years	36.64	33.69	0.00	37.05	36.89	0.84
A-level qualification	0.31	0.23	0.00	0.30	0.27	0.36
University degree	0.25	0.17	0.00	0.21	0.21	0.83
German citizenship	0.96	0.94	0.00	0.97	0.94	0.09
Migration background	0.12	0.18	0.00	0.15	0.17	0.61
Searching for full-time employment	0.65	0.68	0.05	0.70	0.60	0.01
Household characteristics						
Married (or cohabiting)	0.41	0.36	0.00	0.36	0.44	0.05
Two children or more	0.13	0.13	0.95	0.14	0.18	0.19
Partner is full-time employed	0.45	0.44	0.54	0.40	0.50	0.02
Substantial problems with childcare	0.09	0.09	0.97	0.07	0.11	0.05
Labor market history						
UI benefit recipient	0.76	0.78	0.17	0.76	0.81	0.07
Last daily income in €	49.17	46.18	0.00	50.96	47.84	0.27
Employment status before unemployment						
Regular employed	0.65	0.63	0.12	0.72	0.66	0.14
Subsidized employed	0.07	0.06	0.20	0.07	0.06	0.56
Last job was full-time	0.95	0.96	0.19	0.96	0.95	0.95
Months regular employed						
in last year	7.71	8.25	0.00	7.88	8.34	0.22
in last 5 years	32.69	34.95	0.00	33.83	36.15	0.14
in last 10 years	48.72	50.08	0.11	51.50	53.39	0.42
Months unemployed						
in last year	1.25	1.03	0.00	1.17	0.83	0.04
in last 5 years	9.56	7.17	0.00	10.77	7.65	0.00
in last 10 years	12.76	9.69	0.00	13.17	10.07	0.00
Regional and seasonal information						
Region						
West-Germany and local UE rate ≤6%	0.24	0.25	0.33	0.21	0.29	0.09
West-Germany and local UE rate >6%	0.40	0.47	0.00	0.43	0.44	0.64
East-Germany and local UE rate ≤12%	0.15	0.13	0.02	0.12	0.11	0.54
East-Germany and local UE rate >12%	0.21	0.15	0.00	0.24	0.16	0.01
Time between entry into UE and interview						
8 weeks	0.26	0.26	0.62	0.26	0.25	0.76
⋮						
14 weeks	0.06	0.06	0.89	0.04	0.05	0.64
Personality traits						
Openness	4.96	5.14	0.00	4.88	4.99	0.25
Conscientiousness	6.23	6.28	0.03	6.21	6.34	0.04
Extraversion	5.15	5.24	0.01	4.95	5.11	0.07
Neuroticism	3.75	3.72	0.46	3.75	3.80	0.58
Locus of Control	5.04	5.06	0.44	5.00	5.01	0.88
B. Information variables Z						
Utilizing caseworker as search channel	0.64	0.72	0.00	0.62	0.71	0.02
Average weekly number of job offers by employment agency	0.21	0.25	0.00	0.14	0.20	0.13
Information treatment received						
Training program[a)]	0.12	0.20	0.00	0.23	0.49	0.00
Other ALMP program[b)]	0.06	0.10	0.00	0.05	0.07	0.49
Job offer received						
Full-time employment	0.32	0.37	0.00	0.29	0.36	0.05
Part-time employment	0.10	0.12	0.07	0.08	0.10	0.26

Note: Percentage share unless indicated otherwise. $P-$values measured based on two-tailed t-tests on equal means. Personality traits are measured with different items on a 7-Point Likert-Scale.

[a)] Includes application training, programs to improve employment prospects and training vouchers (either received or offered).

[b)] Includes workfare programs, job creation schemes and start-up subsidies to become self-employed.

Table 2.15: Sensitivity Analysis: Alternative Treatment/Control Groups

	Group 1			
	Non-participants		**Participants**	
	$\hat{\pi}$-low	$\hat{\pi}$-high	$\hat{\pi}$-low	$\hat{\pi}$-high
Group 2				
Non-participants				
$\hat{\pi}$-low	0.5401 (0.4985)			
$\hat{\pi}$-high	0.0306* (0.0163)	0.5714 (0.4950)		
Participants				
$\hat{\pi}$-low	-0.0636* (0.0360)	-0.0983*** (0.0353)	0.4798 (0.5007)	
$\hat{\pi}$-high	0.0356 (0.0239)	0.0035 (0.0240)	0.0924** (0.0411)	0.5891 (0.4924)
No. of observations	2,277	2,222	223	567
Control variables				
Socio-demographiccharacteristics	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Labor market histories	Yes	Yes	Yes	Yes
Regional and seasonal information	Yes	Yes	Yes	Yes
Personality traits	Yes	Yes	Yes	Yes

Note: Depicted are average treatment effects on the treated (ATT) as the difference in mean outcomes between group 1 depicted on the vertical axis and group 2 depicted on the horizontal axis using Epanechnikov kernel propensity score matching with bandwidth 0.06. The diagonal line shows the mean outcome of the corresponding group. The number of observations refers only to the corresponding group 1 depicted on the horizontal axis. Standard errors are in parentheses and based on bootstrapping with 399 replications. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Table 2.16: Simultaneous Impact of Expected Treatment Rates ($\hat{\pi}$)/Effects ($\hat{\delta}$) on ATTs

	Non-participants			Participants		
	ATT	SE	Obs.	ATT	SE	Obs.
A. Regular employed in month t_{30}						
Expected treatment rate/effect:						
(1) $\hat{\pi}$-low /$\hat{\delta}$-low	ref.		1,884	-0.0810*	(0.0443)	166
(2) $\hat{\pi}$-high/$\hat{\delta}$-low	0.0441**	(0.0183)	1,407	0.0649*	(0.0332)	257
(3) $\hat{\pi}$-low /$\hat{\delta}$-high	-0.0090	(0.0306)	338	-0.0087	(0.0671)	57
(4) $\hat{\pi}$-high/$\hat{\delta}$-high	0.0118	(0.0219)	870	0.0122	(0.0302)	310
Differences:						
(1) v. (2)				0.1459***	(0.0493)	
(3) v. (4)	0.0208	(0.0338)		0.0208	(0.0759)	
B. Cumulated effect ($\sum_{t=0}^{30}$, months)						
Expected treatment rate/effect:						
(1) $\hat{\pi}$-low /$\hat{\delta}$-low	ref.		1,884	-3.7672***	(0.7037)	166
(2) $\hat{\pi}$-high/$\hat{\delta}$-low	0.9662***	(0.3922)	1,407	-1.7711***	(0.6303)	257
(3) $\hat{\pi}$-low /$\hat{\delta}$-high	-0.1684	(0.6715)	338	-2.9964**	(1.2284)	57
(4) $\hat{\pi}$-high/$\hat{\delta}$-high	0.7931*	(0.4568)	870	-2.3822***	(0.6204)	310
Differences:						
(1) v. (2)				1.9961**	(0.9049)	
(3) v. (4)	0.9616	(0.7096)		0.6142	(1.3603)	
Control variables						
Socio-demographic characteristics	Yes			Yes		
Household characteristics	Yes			Yes		
Labor market histories	Yes			Yes		
Regional and seasonal information	Yes			Yes		
Personality traits	Yes			Yes		

Note: Individuals are categorized based on combinations of actual treatment status, expected treatment rates and expected treatment effects. Depicted are average treatment effects on the treated (ATT) as the difference in mean outcomes between the 7 treatment groups and the reference group of non-participants with $\hat{\pi}$-low and $\hat{\delta}$-low using epanechnikov kernel propensity score matching with bandwidth 0.06. Standard errors are in parentheses and based on bootstrapping with 399 replications. Standard errors for the differences in ATT's are based on bootstrapped robust Hausman tests with 399 replications (see Cameron and Trivedi, 2010, for details). ***/**/* indicate statistically significance at the 1%/5%/10%-level.

Table 2.17: Estimation Results: Expected Change of Search Effort and Job Finding Prospect

	OLS		Ordered Probit	
	Expected change of search effort[a]		Expected job finding probability[b]	
	Coef.	SE	Coef.	SE
Female	0.0684	(0.2341)	-0.2451***	(0.0361)
German citizenship	-0.6638	(0.5924)	-0.1034	(0.0822)
Migration background	0.4237	(0.4086)	-0.1116**	(0.0524)
Age (Ref.: 16-24 years)	ref.		ref.	
25-34 years	-0.3520	(0.3898)	0.1523***	(0.0520)
35-44 years	-0.5802	(0.4169)	0.0647	(0.0582)
45-55 years	-0.6903*	(0.4132)	-0.2509***	(0.0600)
School leaving degree (Ref.: None)	ref.		ref.	
Lower sec. degree	0.9883	(0.6197)	0.0536	(0.0983)
Middle sec. degree	0.5751	(0.6182)	0.0508	(0.0989)
(Spec.) Upper sec. degree	0.8901	(0.6652)	0.1360	(0.1029)
Higher education (Ref.: None)	ref.		ref.	
Internal/external prof. training	-0.0164	(0.4317)	-0.0021	(0.0547)
University degree	-0.0401	(0.5187)	0.0365	(0.0678)
Married or cohabiting	-0.4394*	(0.2631)	-0.2067***	(0.0388)
Children (Ref.: None)	ref.		ref.	
One child	0.0532	(0.2817)	0.0105	(0.0428)
Two children or more	0.6591*	(0.3886)	-0.0323	(0.0536)
Problems with childcare	-0.0132	(0.3670)	-0.1368**	(0.0586)
Partner is full-time employed	-0.2665	(0.2392)	0.0630*	(0.0348)
Searching for full-time employment only	-0.0267	(0.2421)	0.2907***	(0.0367)
Region (Ref.: West-Germany & UE rate 0-6%)	ref.		ref.	
West-Germany & UE rate 6+%	0.3281	(0.2617)	-0.0991***	(0.0382)
East-Germany & UE rate 9-14%	0.4421	(0.3408)	-0.1219**	(0.0523)
East-Germany & UE rate 15+%	0.2488	(0.3193)	-0.2485***	(0.0488)
Entry into unemployment (Ref.: 2nd quarter 2007)	ref.		ref.	
3rd quarter 2007	0.5349	(0.5514)	-0.0582	(0.0664)
4th quarter 2007	0.3095	(0.5429)	0.1281**	(0.0650)
1st quarter 2008	-0.0553	(0.5714)	0.0624	(0.0722)
2nd quarter 2008	0.6438	(0.5509)	0.0719	(0.0707)
Time between UE and interview (Ref.: 7 weeks)	ref.		ref.	
8 weeks	-0.6349	(0.7258)	-0.1675	(0.1228)
9 weeks	-0.9016	(0.7518)	-0.1480	(0.1252)
10 weeks	-0.6497	(0.7602)	-0.2472*	(0.1278)
11 weeks	-0.5647	(0.7822)	-0.2371*	(0.1303)
12 weeks	0.1751	(0.8483)	-0.1983	(0.1350)
13 weeks	-1.6069*	(0.9425)	-0.1140	(0.1484)
14 weeks or more	-0.6067	(0.9074)	-0.1849	(0.1407)
Unemployment benefit recipient	-0.1073	(0.2721)	0.0549	(0.0412)
Last daily income in €	0.0021	(0.0036)	0.0007	(0.0006)
Employment status before unemployment (Ref.: Other)	ref.		ref.	
Regular employment	-0.1163	(0.2851)	0.1509***	(0.0423)
Subsidized employment	0.9244*	(0.4704)	0.1103*	(0.0662)
Months in employment				
in last year	0.0270	(0.0338)	0.0168***	(0.0049)
in last 5 years	-0.0103	(0.0169)	-0.0003	(0.0024)
in last 10 years	0.0104	(0.0100)	-0.0002	(0.0015)
Months in unemployment				
in last year	-0.0386	(0.0614)	0.0231***	(0.0086)
in last 5 years	-0.0388**	(0.0153)	-0.0068***	(0.0024)
in last 10 years	0.0209**	(0.0102)	-0.0005	(0.0018)
Openness (standardized)	0.0282	(0.1096)	0.0578***	(0.0166)
Conscientiousness (standardized)	-0.2242*	(0.1172)	-0.0000	(0.0169)
Extraversion (standardized)	0.0158	(0.1110)	0.0460***	(0.0172)
Neuroticism (standardized)	0.0634	(0.1091)	-0.0276*	(0.0165)
Locus of control (standardized)	0.1221	(0.1055)	0.1149***	(0.0168)
Constant	0.3406	(1.3248)		
cut 1			-1.9565***	(0.1967)
cut 2			-1.2507***	(0.1952)
cut 3			0.0008	(0.1948)
No. of Observations	438		6,037	
(Pseudo-)R^2	0.1436		0.0614	
log-Likelihood	-897.9		-5974.5	
No. of observations	438		6,037	

Note: Italic numbers: ***/**/* indicate statistically significance at the 1%/5%/10%-level.
[a] The expected change of search effort is given as the trend adjusted difference between the average weekly number of own job applications in wave 1 and wave 2 for those actually participating in an ALMP program in between.
[b] The expected job finding probability is given as a four-point item ranging from ‘very unlikely’ to ‘very likely’.

Table 2.18: Estimation of Search Cost Parameters

	Baseline I		Baseline II		Alternative I		Alternative II	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Search costs in unemployment: κ_u								
Female	-0.0430*	(0.0245)	-0.0540	(0.0443)	-0.0817*	(0.0442)	-0.0534	(0.0538)
Migration background	-0.0329	(0.0279)	-0.0598	(0.0481)	-0.0516	(0.0557)	-0.0909	(0.0735)
Age (Ref.: 16-24 years)	ref.		ref.		ref.		ref.	
25-34 years	-0.1126***	(0.0355)	-0.1630**	(0.0638)	-0.0067	(0.0621)	0.0035	(0.0783)
35-44 years	-0.0417	(0.0403)	-0.0319	(0.0738)	0.0747	(0.0723)	0.0981	(0.0912)
45-55 years	-0.0368	(0.0437)	0.0124	(0.0767)	0.1322	(0.0860)	0.1861*	(0.1043)
School leaving degree (Ref.: None)	ref.		ref.		ref.		ref.	
Lower sec. degree	-0.1054	(0.0912)	-0.2186	(0.1482)	-0.0102	(0.1201)	-0.1024	(0.1473)
Middle sec. degree	-0.0843	(0.0922)	-0.2190	(0.1520)	0.0606	(0.1215)	-0.0545	(0.1478)
(Spec.) Upper sec. degree	-0.0204	(0.0956)	-0.1332	(0.1568)	0.0864	(0.1295)	-0.0124	(0.1571)
Higher education (Ref.: None)	ref.		ref.		ref.		ref.	
Internal/external prof. training	-0.0090	(0.0314)	-0.0173	(0.0675)	-0.0367	(0.0702)	0.0682	(0.0931)
University degree	-0.0532	(0.0535)	-0.0553	(0.0945)	-0.0967	(0.0940)	-0.0223	(0.1182)
Married or cohabiting	0.0729**	(0.0289)	0.0960*	(0.0496)	-0.0041	(0.0462)	0.0046	(0.0576)
Unemployment benefit recipient	0.0317	(0.0283)	0.0672	(0.0521)	0.0733	(0.0550)	0.0933	(0.0689)
Last daily income in €	-0.0000	(0.0004)	0.0004	(0.0008)	-0.0001	(0.0008)	0.0008	(0.0009)
Months in employment								
in last year	-0.0013	(0.0031)	0.0027	(0.0058)	-0.0029	(0.0053)	-0.0007	(0.0069)
in last 5 years	-0.0010	(0.0017)	-0.0025	(0.0030)	0.0037	(0.0030)	0.0037	(0.0037)
in last 10 years	0.0002	(0.0012)	0.0009	(0.0021)	-0.0035	(0.0021)	-0.0045*	(0.0026)
Time to interview (Ref.: 7 weeks)	ref.		ref.		ref.		ref.	
8 weeks	-0.0797	(0.0758)	-0.0843	(0.1134)	-0.0329	(0.1453)	-0.0501	(0.1630)
9 weeks	-0.0296	(0.0764)	0.0230	(0.1240)	-0.0150	(0.1466)	-0.0269	(0.1640)
10 weeks	-0.0186	(0.0774)	0.0178	(0.1184)	0.0130	(0.1488)	0.0159	(0.1667)
11 weeks	-0.0732	(0.0755)	-0.0998	(0.1106)	-0.0827	(0.1500)	-0.0849	(0.1695)
12 weeks	-0.0955	(0.0763)	-0.2220*	(0.1148)	0.0381	(0.1586)	-0.0784	(0.1824)
13 weeks	0.0654	(0.1025)	0.3276*	(0.1854)	0.0583	(0.1855)	0.0489	(0.2134)
14 weeks or more	-0.1300	(0.0838)	-0.2031	(0.1318)	-0.0990	(0.1621)	-0.1544	(0.1844)
Openness (standardized)	-0.0139	(0.0121)	-0.0110	(0.0238)	-0.0200	(0.0230)	-0.0162	(0.0289)
Conscientiousness (standardized)	0.0174	(0.0108)	0.0256	(0.0173)	0.0157	(0.0217)	0.0234	(0.0259)
Extraversion (standardized)	0.0120	(0.0125)	0.0224	(0.0239)	-0.0289	(0.0247)	-0.0423	(0.0304)
Neuroticism (standardized)	0.0220*	(0.0130)	0.0416	(0.0263)	0.0106	(0.0224)	0.0404	(0.0313)
Locus of control (standardized)	0.0155	(0.0131)	0.0402*	(0.0227)	0.0490**	(0.0243)	0.0696**	(0.0327)
Constant	0.2983**	(0.1243)	0.4338**	(0.1898)	0.1033	(0.1960)	0.0011	(0.2240)

Continued on next page.

Continued Table 2.18.

Search costs in training program: κ_p								
Female	-0.0545**	(0.0232)	-0.0721*	(0.0408)	-0.0910**	(0.0394)	-0.0665	(0.0466)
Migration background	-0.0535**	(0.0254)	-0.0974**	(0.0422)	-0.0670	(0.0476)	-0.1079*	(0.0633)
Age (Ref.: 16-24 years)	ref.		ref.		ref.		ref.	
25-34 years	-0.0787**	(0.0337)	-0.1020*	(0.0592)	0.0373	(0.0536)	0.0572	(0.0668)
35-44 years	-0.0155	(0.0382)	0.0117	(0.0690)	0.1018	(0.0639)	0.1240	(0.0792)
45-55 years	-0.0117	(0.0417)	0.0496	(0.0733)	0.1477*	(0.0775)	0.1890**	(0.0913)
School leaving degree (Ref.: None)	ref.		ref.		ref.		ref.	
Lower sec. degree	-0.1230	(0.0943)	-0.2663*	(0.1467)	-0.0267	(0.1057)	-0.1242	(0.1303)
Middle sec. degree	-0.1114	(0.0957)	-0.2818*	(0.1506)	0.0188	(0.1068)	-0.1039	(0.1305)
(Spec.) Upper sec. degree	-0.0522	(0.0983)	-0.1902	(0.1541)	0.0553	(0.1148)	-0.0401	(0.1392)
Higher education (Ref.: None)	ref.		ref.		ref.		ref.	
Internal/external prof. training	0.0003	(0.0284)	0.0136	(0.0576)	-0.0184	(0.0577)	0.0876	(0.0786)
University degree	-0.0269	(0.0509)	-0.0021	(0.0857)	-0.0568	(0.0810)	0.0099	(0.1011)
Married or cohabiting	0.0622**	(0.0277)	0.0774*	(0.0470)	-0.0195	(0.0400)	-0.0189	(0.0495)
Unemployment benefit recipient	0.0258	(0.0264)	0.0609	(0.0481)	0.0603	(0.0479)	0.0758	(0.0598)
Last daily income in €	-0.0001	(0.0004)	0.0003	(0.0007)	-0.0001	(0.0007)	0.0006	(0.0008)
Months in employment								
in last year	-0.0008	(0.0029)	0.0035	(0.0054)	-0.0016	(0.0046)	0.0004	(0.0061)
in last 5 years	-0.0017	(0.0016)	-0.0038	(0.0029)	0.0026	(0.0026)	0.0025	(0.0032)
in last 10 years	0.0003	(0.0011)	0.0012	(0.0020)	-0.0030	(0.0019)	-0.0038*	(0.0023)
Time to interview (Ref.: 7 weeks)	ref.		ref.		ref.		ref.	
8 weeks	-0.0678	(0.0674)	-0.1018	(0.0985)	-0.0415	(0.1268)	-0.0870	(0.1390)
9 weeks	-0.0304	(0.0680)	-0.0060	(0.1096)	-0.0393	(0.1276)	-0.0722	(0.1392)
10 weeks	-0.0161	(0.0694)	0.0017	(0.1042)	-0.0046	(0.1297)	-0.0258	(0.1408)
11 weeks	-0.0707	(0.0672)	-0.1183	(0.0952)	-0.0917	(0.1313)	-0.1167	(0.1459)
12 weeks	-0.0741	(0.0666)	-0.2094**	(0.0957)	0.0338	(0.1374)	-0.0925	(0.1556)
13 weeks	0.0707	(0.1039)	0.3249*	(0.1846)	0.0142	(0.1571)	-0.0024	(0.1746)
14 weeks or more	-0.1051	(0.0757)	-0.1674	(0.1170)	-0.0865	(0.1417)	-0.1325	(0.1573)
Openness (standardized)	-0.0135	(0.0114)	-0.0228	(0.0224)	-0.0204	(0.0197)	-0.0248	(0.0242)
Conscientiousness (standardized)	0.0152	(0.0097)	0.0174	(0.0152)	0.0131	(0.0189)	0.0141	(0.0221)
Extraversion (standardized)	0.0071	(0.0118)	0.0164	(0.0223)	-0.0296	(0.0216)	-0.0444*	(0.0267)
Neuroticism (standardized)	0.0303**	(0.0126)	0.0600**	(0.0250)	0.0197	(0.0195)	0.0500*	(0.0271)
Locus of control (standardized)	0.0231*	(0.0124)	0.0563***	(0.0211)	0.0532***	(0.0217)	0.0775***	(0.0287)
Constant	0.3609***	(0.1231)	0.5495***	(0.1792)	0.1867	(0.1737)	0.1263	(0.1958)

Note: Depicted are parameters of individual characteristics on search costs in unemployment, respectively training programs, based on Maximum Likelihood estimation. ***/**/* indicate statistically significance at the 1%/5%/10%-level.

3 Mobility Assistance and Job Search Strategies

The appealing idea of geographically relocating unemployed job seekers from depressed to prosperous regions and hence reducing unemployment, leads industrialized countries to offer financial support to unemployed job seekers when searching and/or accepting distant jobs. In this chapter, we investigate the impact of the existence of these so-called mobility programs on the job search behavior of unemployed workers and how this affects their labor market outcomes. While job search theory predicts a shift in individuals' search effort from local to distant labor markets, consequences for other dimensions of the search behavior, e.g. reservation wages or the overall search effort, and job finding probabilities remain theoretically ambiguous. We use survey data on German unemployed job seekers and apply an instrumental variable approach to identify empirically the causal impact of distant job search, as triggered by the availability of mobility programs, on job search strategies and subsequent labor market outcomes. The results show that the existence of mobility programs shifts the individuals' search effort from local to geographically distant regions without affecting the total number of job applications. The increase in search radius causes higher employment probabilities and wages.[26]

[26] This chapter is based on joint work with Marco Caliendo and Steffen Künn (Caliendo et al., 2017a, see).

3.1 Introduction

In macroeconomic models, geographical mobility of labor is considered to be one of the most efficient adjustment mechanisms to equal regional disparities in terms of unemployment rates (e.g. Borjas, 2006; Blanchard et al., 1992). The re-allocation of labor within the economy is particularly expected to improve job match quality and relax labor market tightness in certain regions (Taylor and Bradley, 1997; Giannetti, 2002). Therefore, some industrialized countries spend financial resources on increasing the job seekers' willingness to search and accept distant jobs. For instance, the German active labor market policy (ALMP) offers a wide range of financial support for distant job search activities, e.g., subsidies for travel costs to distant job interviews, daily commuting costs or even the costs of a relocation. These programs aim to remove existing financial barriers[27] for unemployed individuals in order to extend their search radius. Earlier studies have shown the effectiveness of such programs to improve the labor market outcomes of actual participants (e.g. Caliendo et al., 2017b).

However, it has not been examined yet whether the pure existence of mobility programs (as part of the national ALMP) already affects the individuals' search behavior and has consequences for subsequent job finding prospects. Based on a theoretical search model which allows for simultaneous job search in local and distant labor markets (e.g. Damm and Rosholm, 2003; Arntz, 2005), it can be argued that the pure availability of mobility programs affects the equilibrium levels of distant and local job search effort by changing the associated costs for searching and accepting distant jobs. In other words, job seekers might increase their search radius just because they are (made) aware of the possibility of receiving a subsidy for travel or relocation costs, which makes distant job search relatively cheaper compared to local job search. However, it remains theoretically unclear whether job seekers increase their overall search effort or substitute local for distant job search, and whether this increases the job finding rate, e.g., due to higher job offer arrival rates or a better job match quality. We now empirically investigate the mechanisms and aim to answer the following questions: Does the existence of mobility programs affect individuals' willingness to search for distant jobs and if so, what is the impact on other job search characteristics, such as local search effort and reservation wage? Finally, we examine the impact of the altered job search strategy on subsequent labor market outcomes.

Given these research questions, we contribute to the economic literature on the determinants, as well as the returns to geographical mobility. With respect to the determinants, it has been shown that a generous welfare system (De Giorgi and Pellizzari, 2009), strong

[27] We find suggestive evidence for the existence of financial barriers in our estimation sample. For instance, households of recipients of mobility programs are more likely to depend on welfare payments and are less likely to being able to pay off their debt than non-subsidized job seekers.

social ties (Rainer and Siedler, 2009; Belot and Ermisch, 2009), homeownership (Battu et al., 2008; Caliendo et al., 2015) and risk-aversion (Jaeger et al., 2010; Bauernschuster et al., 2014) reduce internal migration, while educational mismatch (Borjas et al., 1992) and regional disparities in terms of prices (Giannetti, 2003), income (Kennan and Walker, 2011) and labor demand (Wozniak, 2010) positively affect geographical mobility in the labor market. Moreover, studies on individual characteristics find that migrants are rather young and have higher levels of human capital (e.g. Pekkala and Tervo, 2002; Hunt, 2006; Dustmann and Preston, 2007). In this chapter, we analyze to what extent the existence of a governmental subsidy program affects individuals' decision to search for distant jobs and hence geographical mobility. Furthermore, we contribute to the literature on the return to geographical mobility, especially in the context of governmental subsidy programs. So far, it exists evidence only for the US (Briggs and Kuhn, 2008; Mueller, 1981), Germany (Caliendo et al., 2017b) and Romania (Rodríguez-Planas and Benus, 2010) showing that relocation assistance programs improve the labor market outcomes (employment and income) of participants. In contrast to the existing literature, we now analyze whether the pure existence of mobility programs, independent of actual participation, has an impact on the job seekers' labor market outcomes (via changes in job search behavior).

We use rich survey data on unemployed job seekers in Germany and apply an instrumental variable approach exploiting regional variation among German local employment agencies (LEA) with respect to their preferences towards mobility programs. Therefore, we take advantage of the fact that each LEA has a high degree of autonomy when deciding on its own policy mix, i.e., which share of its budget to spend on which ALMP program. This autonomy leads to regional differences in terms of the intensity at which mobility programs are offered to job seekers (conditional on local labor market conditions). We use this regional variation as an instrumental variable which exogenously affects the individual probability to search for distant jobs. Therefore, job seekers living in a LEA district with a high intensity of mobility programs also face a higher probability to receive knowledge about the existence of the mobility programs (via the caseworker) which is expected to increase their willingness to search for distant jobs. This exogenous variation in the first stage allows us then to estimate the causal effect of searching for distant jobs on other search characteristics and subsequent labor market outcomes. Based on this IV setting, our results can be interpreted as the local average treatment effect (LATE, see Imbens and Angrist, 1994) on those job-seekers who start searching for distant jobs due to the LEA's support of mobility programs. We argue that this parameter is of high interest for policy makers as they have full control over the instrumental variable, the (regional) intensity of mobility programs.

The results show that regional variation in the availability of mobility programs impact the individuals' willingness to search for distant jobs. In fact, job seekers living in LEA districts which offer mobility programs more intensively, shift their search effort from local to distant job search, while the total effort level remains unchanged, while the extended search radius leads to a higher probability to move to a distant region, as well as higher job finding probabilities and wages. Moreover, we conduct an extensive sensitivity analysis in order to rule out that unobserved regional differences bias our estimation. In particular, we exploit an alternative instrument and account for region-specific preferences of the working population with respect to geographical mobility. The remainder of the chapter is organized as follows. Section 3.2 explains the institutional settings in Germany, outlines the theoretical job search model and introduces the data. Section 3.3 explains the econometric identification strategy. Section 3.4 presents the estimation results and shows the robustness of these results, especially with respect to unobserved regional heterogeneity. Section 3.5 concludes.

3.2 Institutional Settings, Economic Framework and Data

3.2.1 Mobility Programs in Germany

Mobility programs as part of ALMP have been initially introduced in 1998 in Germany to encourage the geographical mobility among unemployed job seekers. Mobility programs encompass in total six separate programs ranging from reimbursement for distant job interviews to relocation assistance. While the use of such programs was only modest right after its introduction in 1998, it increased remarkably with the implementation of a major labor market reform, the so-called “Hartz Reform”, between 2003 and 2005 (see, e.g., Caliendo and Hogenacker, 2012, for details). For instance, 84,000 job seekers received a mobility assistance in 1999, while it increased to 375,000 participants in 2008.

In general, mobility programs are directly linked to a transition to employment, i.e., in order to being eligible the job seeker has to have a concrete job offer (respectively a job interview in order to receive travel cost assistance). It is further important to know that out of the six separate programs that are summarized under the term mobility programs, only four programs are actually directly linked to the geographical mobility of unemployed job-seekers. The two “unrelated” subsidy programs are called equipment and transition assistance. The equipment assistance financially supports the acquisition of work clothes and working tools up to an amount of 260 €, while the transition assistance offers an interest-free loan up to 1,000 € to bridge the period until the first wage payment arrives. Both programs aim to increase the job seekers’ overall flexibility to overcome financial barriers to the new job, but not necessarily the geographical mobility. Nevertheless, they are categorized as mobility programs due to administrative reasons.

In contrast, the other four programs are directly aiming at the geographical mobility. First, the travel cost assistance reimburses expenses for distant job interviews up to an amount of 300 €. Second, the commuting assistance financially supports the daily commuting to work with 20 Euro cent per kilometer for the first six months in the new job. Third, the separation assistance subsidizes temporary accommodation costs of up to 260 € per month for a period of maximal 6 months, e.g., for renting a second apartment at the new working location. And fourth, the relocation assistance provides full coverage of transportation costs (with a maximum of 4,500 €) associated with a permanent move to the new working location. In order to being eligible to both separation and relocation assistance, the daily commuting time to the new working location has to exceed 2.5 hours.

The application for all subsidy types has to be submitted to the LEA before the actual event that should be subsidized takes place. Moreover, job seekers are only eligible if the prospective employer does not cover the requested costs, and subsequent program participation is allowed. The final decision about subsidy receipt is at the caseworker's discretion (no legal claim). The caseworker decides based on the individual labor market situation of the applicant and the available budget of the local employment agency for mobility programs.

3.2.2 Theoretical Framework

To analyze theoretically the potential effects of the different subsidies on individuals' job search strategy and labor market outcomes, we consider a sequential job search model in a stationary environment (see e.g. Mortensen, 1986) where job seekers are allowed to search simultaneously in local and distant labor markets. Therefore, we define local as within commuting distance whereby distant jobs are those that would require a residential relocation. The model as outlined below is in line with earlier studies looking at simultaneous job search in distinct labor markets, such as Hosios (1990) and Acemoglu (2001) who consider job search in different sectors, or Damm and Rosholm (2003) and Arntz (2005) who allow for job search in different geographical labor markets, and Van den Berg and Gorter (1997) for differing commuting times.

Model Setup: Each job seeker decides on how much effort he/she wants to spend in local and distant job search activities, denoted by e_l and e_d respectively. This decision on search effort as well as regional differences in terms of labor market conditions imply different job offer arrival rates for local $\alpha_l(e_l)$ and distant jobs $\alpha_d(e_d)$. Both functions are increasing with respect to effort, i.e. $\frac{\partial \alpha_l}{\partial e_l} > 0$ and $\frac{\partial \alpha_d}{\partial e_d} > 0$. Furthermore, the two labor markets are characterized by different wage offer distributions $F_l(w_l)$ for local and $F_d(w_d)$ for distant jobs, which are assumed to be known by the job seeker. Accepting a distant job offer involves higher costs compared to a local offer. The additional costs, denoted by $\kappa > 0$, arise due to the relocation including, e.g., transportation costs, job change of partner, school change of children, social ties etc. Finally, when not accepting a job offer the search process continues, where searching for a job causes costs $c(e_l, \lambda e_d)$ depending on both types of search effort. Search costs are assumed to increase with respect to effort, i.e., $\frac{\partial c}{\partial e_l} > 0$, $\frac{\partial c}{\partial e_d} > 0$, and $\lambda > 1$ denotes the additional search costs for distant compared to local job search, e.g., due to higher travel costs for job interviews or higher effort is needed to receive information about vacancies.

Optimal Search Strategy: Within this framework, the optimal search strategy is to accept any offer with a wage that exceeds the individual reservation wage which is defined as the lowest net wage at which the job seeker is indifferent between accepting the offer and remaining unemployed. The job seeker rejects every job offer with a wage below the reservation wage. For a given discount rate r, the reservation wage ϕ can be derived by equalizing the inter-temporal utility of accepting a given local or distant job offer and the utility of staying unemployed (see for instance Rogerson et al., 2005, and Appendix 3.6.1 for details):

$$\phi = -c(e_l, \lambda e_d) + \alpha_l(e_l) E_{F_l} \max\left\{\frac{w_l - \phi}{r}, 0\right\} + \alpha_d(e_d) E_{F_d} \max\left\{\frac{w_d - (\kappa + \phi)}{r}, 0\right\} \tag{3.1}$$

where the first term on the right hand side depicts the search costs, and the second and third term the return to local and distant job search, respectively. Given the job offer arrival rates, the cost function and the wage distributions, the job seeker chooses the optimal level of effort on local and distant job search by maximizing the inter-temporal utility, i.e., $\frac{\partial \phi}{\partial e_l} = \frac{\partial \phi}{\partial e_d}$ which yields:

$$G(e_l, e_d) = \lambda \frac{\partial c}{\partial e_d} - \frac{\partial c}{\partial e_l} + \frac{\partial \alpha_l}{\partial e_l} E_{F_l} \max\left\{\frac{w_l - \phi}{r}, 0\right\} - \frac{\partial \alpha_d}{\partial e_d} E_{F_d} \max\left\{\frac{w_d - (\kappa + \phi)}{r}, 0\right\} = 0 \tag{3.2}$$

This first order condition implies that the availability of mobility programs affects the equilibrium levels of search effort for local and distant jobs by changing the costs of accepting a distant job offer κ as well as the additional search costs for distant jobs λ. In the following, we discuss conclusions for each program separately. Assuming that $\frac{\partial G}{\partial e_l} < 0$ and $\frac{\partial G}{\partial e_d} < 0$ ensures that Equation 3.2 characterizes a maximum. This implies that the marginal return with respect to both types of search effort, determined by the job offer arrival rates α_l and α_d, increase to a smaller degree than the marginal search costs imposed by the function c. Hence, the model predicts:

$$\frac{\partial e_d}{\partial \lambda} < 0, \quad \frac{\partial e_d}{\partial \kappa} < 0, \quad \frac{\partial e_l}{\partial \lambda} > 0 \quad \text{and} \quad \frac{\partial e_l}{\partial \kappa} > 0. \tag{3}$$

which allows conclusions with respect to the impact of the subsidies on the search effort. In the following, we discuss the expected effects for each program separately.

Equipment and transition assistance Both subsidies do not have a direct impact on λ or κ. Therefore, without further assumptions, we would not expect a change in the search strategy in terms of geographical mobility. With respect to the exit rate to employment, we would expect an increase given that both subsidies reduce the general costs of accepting a job offer reducing the reservation wage.

Travel cost assistance This subsidy reduces λ, the additional costs of distant relative to local job search which has two effects. On the one hand, the decreasing the costs of distant job search allows the job seeker to spend in total more effort in both job search activities (endowment effect). On the other hand, local job search becomes more expensive relative to distant job search which encourages the job seeker to shift effort from local to distant search activities (price effect). In total, this implies a higher level of distant search effort and a reduction of local search effort which increases (decreases) the exit rate to distant (local) jobs. However, it should be noted that, for local job search, the price and the endowment effect act into different directions and therefore the overall consequences for local job search crucially depend on the functional form of G.[28]

Separation and relocation assistance Both subsidies reduce κ. Everything else equal, this leads to a higher exit rate to employment due to a reduction in the reservation wage ϕ (see Equation 3.1). However, the reduction in κ also increases the net wage for distant job offers making distant job offers more attractive. This results in an increase in ϕ as well as increased effort in distant job search. Both have opposing effects with respect to the exit rate to employment. Concerning local job search, the model predicts a negative impact as the return to distant job search increases relative to local job search which encourages job seeker's to shift their effort to distant job search.

Commuting assistance The commuting assistance reduces the costs of accepting jobs that involve daily commuting. By definition, this does not involve a relocation and hence affect only local jobs. However, given that the absolute amount of the subsidy is higher the larger the commuting distance, it seems to be reasonable to assume that primarily jobs

[28]In general, the same is true for the overall effect on distant job search. However, since $\lambda > 1$, it is much more likely that $\frac{\partial G}{\partial e_d} < 0$ compared to $\frac{\partial G}{\partial e_l} < 0$ which can be seen from condition 3.19 and 3.20 in Appendix 3.6.1.

involving long-distant commuting are accepted due to the presence of the commuting assistance. For these jobs involving long-distance commuting, the perception whether such a vacancy would involve a relocation is very subjective and the job seekers assessment might changes over time. Given the fact that the commuting assistance expires after six months, it seems to be plausible that recipients might commute only during the first months and finally, move to the new working location. Therefore, the commuting assistance might also reduce the costs of accepting a distant job offer (κ) and generate similar effects as in the case of separation and relocation assistance.

To sum up, based on the job search model, paying job seekers a subsidy that supports geographical mobility is expected to increase job seekers' effort in distant job search activities. However, the consequences for local job search and the exit rate to employment remain theoretically ambiguous.

3.2.3 Data and Descriptive Statistics

For the empirical analysis, we use the *IZA Evaluation Dataset Survey* as provided by the International Data Service Center (IDSC) of the Institute for the Study of Labor (IZA). The data consists of survey information on 17,396 individuals who entered unemployment between June 2007 and May 2008 in Germany (see Arni et al., 2014, for details). The survey consists of three interview waves. This first interview took place shortly after entry into unemployment (on average 10 weeks). After 12 and 36 months, the participants received a second and third interview. Due to panel attrition, only 8,915 and 5,786 individuals are observed in the second and third wave, respectively. Besides an extensive set of socio-demographic characteristics and labor market outcomes, the survey contains a large variety of non-standard questions about job search behavior, social networks, psychological factors, cognitive and non-cognitive skills, subjective assessments on future outcomes, and preferences.

We implement the following restrictions to define our estimation sample: We use the first and second wave only.[29] Although the third wave would increase the observation window to 36 months after entry into unemployment, it induces a low number of observations (due to panel attrition) which would significantly reduce the statistical power of the empirical model. We further consider only individuals who report in the first interview that they actively search for employment (including self-employment), as only those received the questions on job search behavior which are crucial for our analysis. Active job search is defined as having sent out at least one application between entry into unemployment and

[29]Section 3.4.4 shows the robustness of our results with respect to panel attrition.

Table 3.1: Selected Observed Differences between Local and Distant Job Seekers

	Local job seekers	Distant job seekers	P-value
No. of observations	4,625	1,799	
1)Socio-demographic and household characteristics			
Age (in years)	37.17	31.65	0.00
Women	0.54	0.39	0.00
Upper secondary school	0.25	0.40	0.00
University degree	0.17	0.35	0.00
Married or cohabiting	0.46	0.22	0.00
Two or more children	0.17	0.08	0.00
2)Labor market history			
Unemployment benefit receipt	0.76	0.74	0.02
Level of unemployment benefit (missings=0)	489.36	494.91	0.67
Share of months spent in employment since age 18	0.66	0.55	0.00
3)Personality traits[(a)]			
Openness	4.97	5.23	0.00
Conscientiousness	6.22	6.21	0.44
Extraversion	5.15	5.21	0.04
Neuroticism	3.81	3.60	0.00
Locus of control	4.99	5.13	0.00
4)Expectations and socio-cultural characteristics			
Expected probability to participate in ALMP program[(b)]			
Low (0-3)	0.25	0.24	0.55
Medium (4-6)	0.16	0.14	0.01
High (7-10)	0.24	0.25	0.79
Expected monthly net income in €	1,275	1,526	0.00
Partner is full-time employed[(c)]	0.50	0.30	0.00
Home-ownership	0.42	0.30	0.00
Car-ownership	0.66	0.63	0.02
High language skills English	0.24	0.46	0.00
5)Regional characteristics			
Living in West Germany	0.71	0.65	0.00
Local unemployment rate in %	8.97	9.45	0.00
Local vacancy rate in %	11.38	11.08	0.11
Share of working population in industry sector	26.36	25.48	0.00

Note: All numbers are percentages unless otherwise indicated. P-values are based on two-tailed t-tests on equal means between local and distant job seekers.
[(a)] Personality traits are measured with different items on a 7-Point Likert-Scale.
[(b)] Expected ALMP probabilities are measured on a 0-10 scale increasing from low to high and categorized into three groups.
[(c)] Includes also partners not living in the same household.

the first interview. This restriction excludes individuals who either already found a job or are inactive. To estimate the effect of mobility programs on distant job search, we divide the sample into distant and local job seekers. The definition of *distant job seeker* is based on the survey question whether the job seeker also applied for vacancies which would require a relocation; respondents who negated this question are defined as *local job-seekers*. This classifying question is measured at the first interview for which the exact timing differs significantly across individuals (one to four months after entry into unemployment). Therefore, we do control for the timing of the first interview in the empirical model to take the correlation between search radius and unemployment duration into account.[30] The final estimation sample consists of 4,625 local and 1,799 distant job-seekers.

Table 3.2: Differences in Job Search Behavior and Labor Market Outcomes

	Local job seekers	Distant job seekers	P-value
No. of observations	4,625	1,799	
A. Job search behavior (measured in wave 1)			
Average weekly no. of job applications			
Total	1.36	2.21	0.00
Local jobs	1.36	1.29	0.33
Distant jobs	0.00	0.91	0.00
Hourly reservation wage in €$^{(a)}$	7.07	7.69	0.00
B. Labor market performance (measured in wave 2)			
Regular employed	0.50	0.54	0.00
Regular self-employed	0.03	0.05	0.00
Subsidized self-employed	0.03	0.04	0.07
Hourly earnings in €$^{(b)}$	8.18	9.27	0.00
Weekly working hours$^{(b)}$	35.78	41.73	0.00
C. Job related and household variables (measured in wave 2)			
Relocation between wave 1 and wave 2 (on county level)	0.03	0.12	0.00
Receipt of mobility assistance related to first transition	0.00	0.03	0.00
Equivalent household income in €/month$^{(c)}$	1,312	1,406	0.00
Partner is full-time employed	0.50	0.35	0.00
Life satisfaction (0=low, 10=high)	6.81	6.76	0.36

Note: All numbers are percentages unless otherwise indicated. P-values are based on two-tailed t-tests on equal means between local and distant job seekers.
$^{(a)}$Reservation wages are only observed for a smaller subsample of individuals, i.e., those who are still unemployed during the first interview (local job seekers: N=3,332; distant job seekers: N=1,191).
$^{(b)}$Hourly earnings in wave 2 are only observed for a smaller subsample of individuals, i.e., those who are already (self-)employed at the second interview (local job seekers: N=2,818; distant job seekers: N=1,173).
$^{(c)}$ The equivalent household income is the total household income weighted by the household members. According to the OECD-modified scale the first adult receives a weight of 1.0, each subsequent household member aged 14 and over is weighted with 0.5 and each child aged under 14 receives a weight of 0.3.

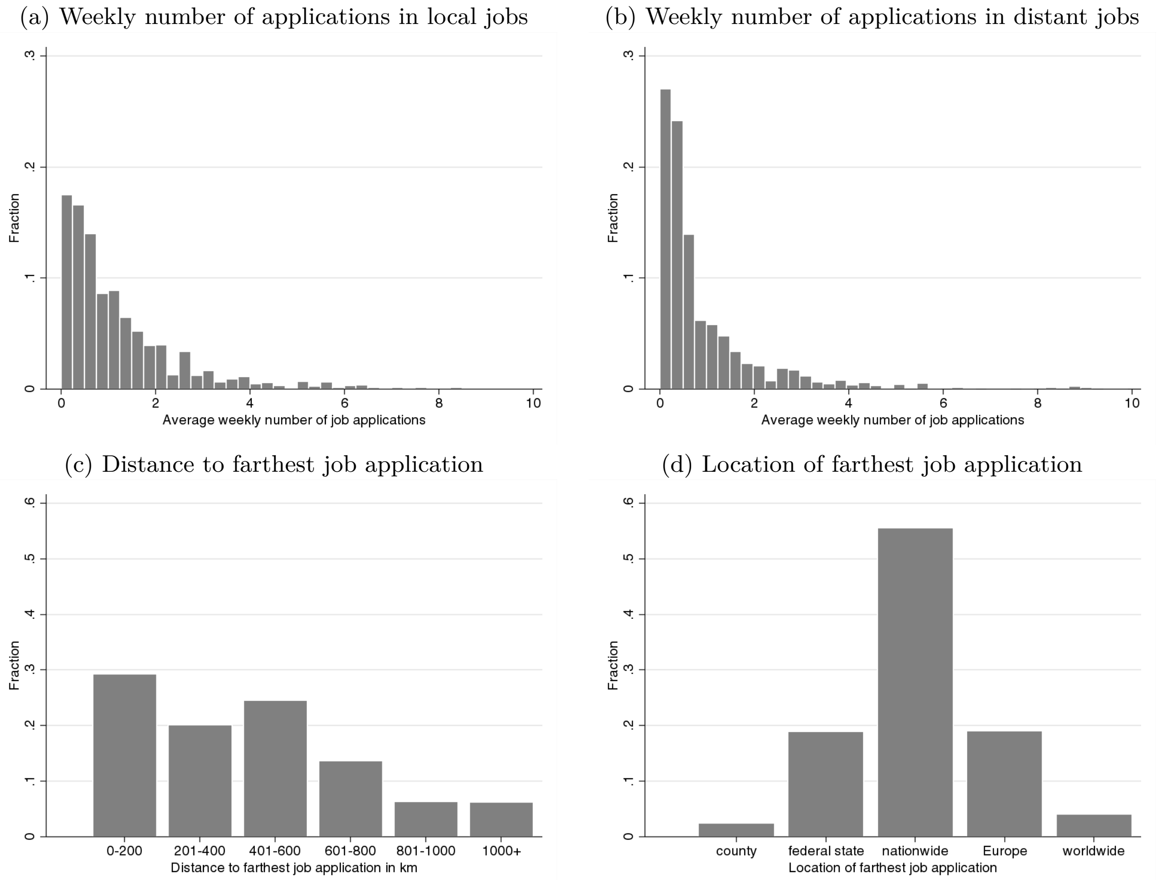
Table 3.1 shows selected descriptive statistics of observed individual, household and regional characteristics. First, it can be seen that distant job-seekers are very different compared to local job seekers in terms of observed characteristics. They tend to be younger, higher educated, have more employment experience and higher earnings in the past. Moreover, they are less likely to have family obligations and property. Second, with respect to personality, distant job seekers tend to be more open, less neurotic and to have a more internal locus of control. Finally, distant job seekers are living in rather disadvantaged regions in terms of labor market conditions.

As outcome variables, we consider different characteristics with respect to (i) individuals' job search behavior (measured in wave 1), (ii) labor market performance (measured in wave 2) and (iii) other job-related and household variables (also measured in wave 2). Table 3.2 shows unconditional differences between local and distant job seekers with respect to different outcome variables. In panel A and B, it can be seen that the selection

[30]For instance, individuals might extend their search radius with increasing unemployment duration if local job search fails. Without consideration, this might bias our results.

of distant job seekers is reflected by the job search behavior and more favorable labor market outcomes. With respect to job search, distant job seekers put more effort into job search activities and have a higher reservation wage. Search effort is measured by the average weekly number of applications. In addition to Table 3.2, Figure 3.1 shows the exact distribution of the number of job applications and search radius. With respect to labor market performance, distant job seekers are more likely to be employed, earn higher wages and work more hours than local job seekers.

Figure 3.1: Distribution of Job Search Effort



Note: (a) depicts the distribution of weekly applications in local jobs for all individuals (N=6,424), while (b), (c) and (d) depict three dimensions of distant search effort for distant job seekers only (N=1,799).

Panel C of Table 3.2 shows that distant job seekers are also more likely to move their place of residence between the two points of measurement and to receive a financial support for the relocation. Moreover, we consider the equivalent household income which is the total household income weighted by the household members.[31] It can be seen that distant job seekers face a higher equivalent household income than local job seekers. Finally, we do not find differences in terms of life satisfaction.

[31] According to the OECD-modified scale the first adult receives a weight of 1.0, each subsequent household member aged 14 and over is weighted with 0.5 and each child aged under 14 receives a weight of 0.3.

3.3 Estimation Strategy

As discussed in Section 3.2.2, it remains theoretically ambiguous how increasing the search radius by offering mobility programs affects the overall job search strategy and what consequences it would imply for subsequent labor market outcomes. The main problem when estimating the effect of distant job search using non-experimental data is the simultaneous correlation of unobserved characteristics, such as motivation of a job-seeker or social ties, with the job seeker's willingness to search for distant jobs and the outcome variables.

We address the endogeneity problem by using an instrumental variable approach. In fact, we exploit regional variation in terms of the local employment agencies' preferences for mobility programs (instrument) to identify the effect of the availability of mobility programs on the individual probability to search for distant jobs (1st stage), as well as the causal impact of searching for distant jobs on both the overall search strategy and labor market outcomes (2nd stage). In the following, we explain the instrument and the estimation strategy in more detail.

3.3.1 The Local Treatment Intensity as Instrumental Variable

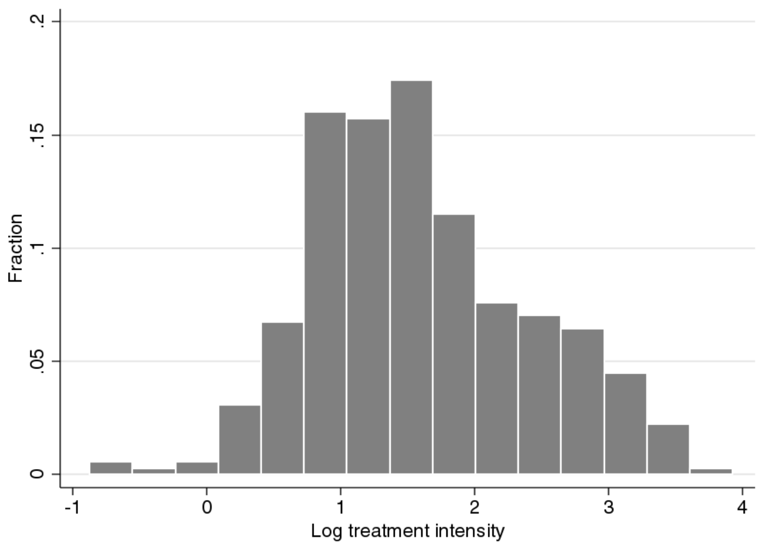
In an ideal setting, we would actually like to have access to experimental data on (at least) two regions with the only difference that one region offers mobility programs and the other does not. As long as job seekers are randomly assigned to either of the two regions, an unconditional comparison would yield a causal interpretation. Unfortunately, such an experiment does not exist in reality so that we have to find a similar "quasi" experimental situation to causally identify the above mentioned mechanism. In fact, we exploit a special feature of the administration of the German employment agency with respect to the allocation of ALMP programs. While the Federal Employment Agency determines the budget for each Local Employment Agency (LEA) and the set of ALMP programs, the single LEAs have autonomy in allocating the assigned budget to the predetermined ALMP programs (see Blien et al., 2009; Fertig et al., 2006). This autonomy generates regional variation in terms of the intensity at which job seekers are treated with mobility programs, i.e., certain regions assign higher budgets to mobility programs than others. The allocation decision by the LEA is based on two dimensions: (i) local labor market conditions and (ii) preferences of the administrative boards of the LEAs, capturing beliefs and experiences about the effectiveness of certain ALMP programs. The empirical challenge is to isolate that part of the preferences which is exogenous with respect to the job seekers search behavior and labor market outcomes. Therefore, we include several control variables for local labor market conditions, time characteristics and different types of regional fixed effects. We show that the remaining variation in the instrument can be considered to be exogenous. In addition, we run an extensive sensitivity analysis, including the implementation of an alternative instrument, to underline the validity of our approach (see Section 3.4.4). Finally, given that unemployed are assigned to local employment agencies based on their place of residence, we argue that the variation in terms of LEAs' preferences for mobility programs affects the individual decision to search for distant jobs and at the same time, are exogenous to the individual job seeker (see discussion on relevance and exogeneity below).

We define our instrument, the local treatment intensity of mobility programs, as follows:[32]

$$Z_{jt-1} = \log \left[\frac{N^{ma}_{jt-1}}{N^{ue}_{jt-1}} \times 100 \right], \tag{3.3}$$

where N^{ma}_{jt-1} denotes the number of recipients of mobility programs and N^{ue}_{j} the average stock of unemployed job seekers in each LEA district $j = 1, ..., 178$ both measured in $t-1$, i.e., the year before the job seekers entered unemployment to avoid that our estimation sample contributes to the construction of the instrument. The instrument reflects the intensity at which each LEA offers/uses mobility programs. The distribution of the instrument within our estimation sample can be seen in Figure 3.2.

Figure 3.2: Distribution of Log Treatment Intensity



Note: Depicted is the log treatment intensity (original instrument) among German LEA districts pooled for 2006 and 2007.

In order to identify causal local average treatment effects (LATE, see e.g., Imbens and Angrist, 1994), the instrument has to fulfill three main conditions. It has to be

[32] We use the treatment intensity as the LEAs do not provide information on the initially planned expenditures for mobility programs.

relevant, exogenous and affect the probability to search for distant jobs in a monotonic way.

Relevance The main idea behind the instrument is that the preferences of the LEA for mobility programs influences the probability that a job seeker receives knowledge about the availability of the subsidies and hence, its job search strategy. In Germany, every job seeker will be assigned to a caseworker when registering as unemployed. The caseworker and the job seeker meet regularly to discuss the job search strategy including possible participation in ALMP. During the meetings, caseworkers in regions with high treatment intensities, and therefore a strong preference for mobility programs, are more likely to inform job seekers about the availability of the programs, compared to low intensity regions. Moreover, there is no legal claim to mobility programs but the final decision on subsidy receipt is at the caseworker's discretion (see Section 3.2.1). Therefore, in addition to the information channel, caseworkers in high treatment intensity regions are also more likely to give a positive indication with respect to the final approval of the subsidy due to higher available budgets. This features ensures that our instrument remains relevant even if one assumes perfect information among job seekers, i.e., all job seekers know about the availability of mobility programs independent of the treatment intensity.

It can be assumed that the information as well as approval channel affects the job seekers' willingness to apply for distant vacancies. Column 1 of Table 3.3 shows the first stage estimates where we regress a binary indicator for distant job search on the instrument and control variables (see Equation 3.6 below). It can be seen that our instrument has a significant impact on the job seeker's willingness to apply for distant vacancies. Doubling the treatment intensity increases the probability of distant job search by about 4 percentage points. For the average region, doubling the treatment intensity would imply a rise from 7 to 14%. The resulting F-statistic of 13.9 is sufficiently large (> 10) to reject the hypothesis of a weak instrument (see Staiger and Stock, 1997).

Exogeneity The exogeneity condition requires the instrument to be randomly assigned (independence condition) and to have no influence on the outcome variables other than through its effect on the probability to search for distant jobs (exclusion restriction). This assumption only holds if we manage to isolate the variation in our instrument that arises due to different preferences among LEAs from the part of the variation that is due to regional differences that also affect individuals' labor market outcomes. Figure 3.3a shows the geographical distribution of our instrument Z among LEA districts. The endogenous assignment of Z can be clearly seen, i.e., in particular disadvantaged regions (predominately in the east and north of Germany) tend to use mobility programs at a higher intensity. Therefore, we control for a large set of local labor market conditions

Table 3.3: First Stage Estimation Results and Placebo Tests

	Baseline	Placebo tests I	II	III	IV	Check monotonicity
	(1)	(2)	(3)	(4)	(5)	(6)
Log local treatment intensity (Z_j)						
Mobility programs	0.040^{***} (0.011)					
Vocational training		0.014 (0.010)				
Job creation schemes			0.004 (0.014)			
Sanctions				-0.009 (0.013)		
Insolvencies					0.007 (0.009)	
Commuting assistance						0.024^{***} (0.007)
Control variables						
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Regional characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Time characteristics	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	6,424	6,424	6,424	6,424	6,424	6,424
F-statistic of weak identification	13.850	2.131	0.073	0.452	0.612	11.223

Note: Dependent variable: D_i - indicator for distant job search. OLS estimation. */**/*** indicates statistical significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA-level. Full estimation results for the baseline model can be found in Table 3.10 in the Appendix (Section 3.6).

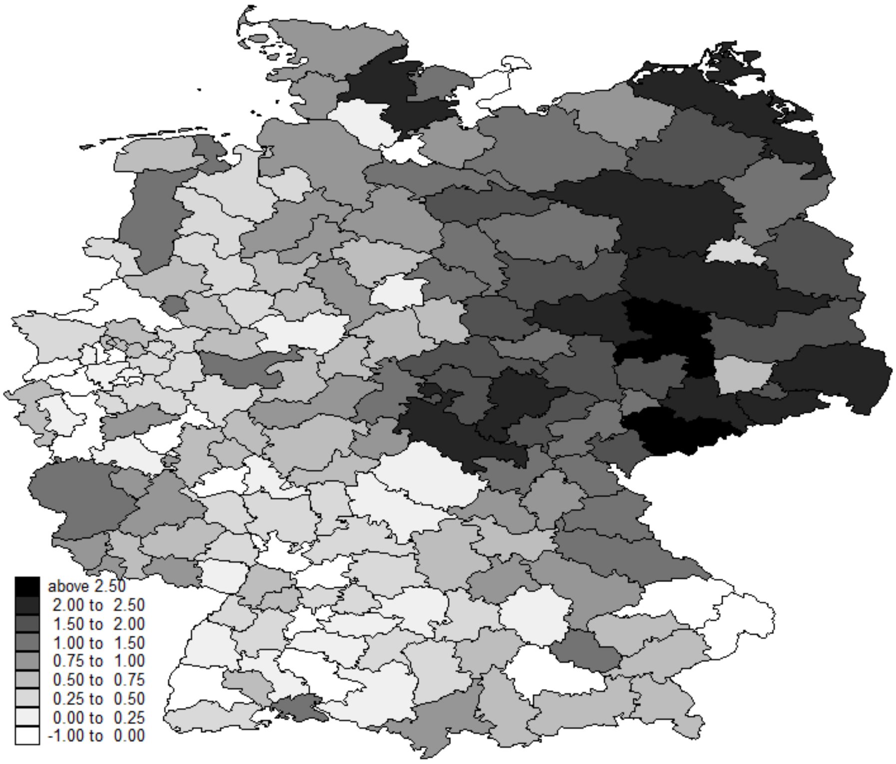
including local unemployment rate, vacancy rate, GDP per capita and industry structure and time characteristics including the month of entry into unemployment and the duration between the entry into unemployment and the first interview. It can be expected that the remaining variation in our instrument proxies the LEAs' preferences for mobility programs.

However, even after having controlled for labor market indicators, one might be concerned that there are further unobserved regional differences that influence the instrument and the individual outcome variables simultaneously. For instance, the workforce in region A might have higher preferences for geographical mobility than the workforce in region B. If the LEA in region A would adjust the supply of mobility programs due to these preferences, our instrument would be endogenous. Therefore, we additionally control for local emigration rates to capture time-constant regional specific preferences for geographical mobility. We measure the emigration rates before the introduction of the mobility programs in 1998 to have a proxy for regional preferences that is independent of the LEA's policy.[33] Again, year-specific preferences are ruled out by using the lagged treatment intensity as an instrument. The distribution of the instrument conditional on

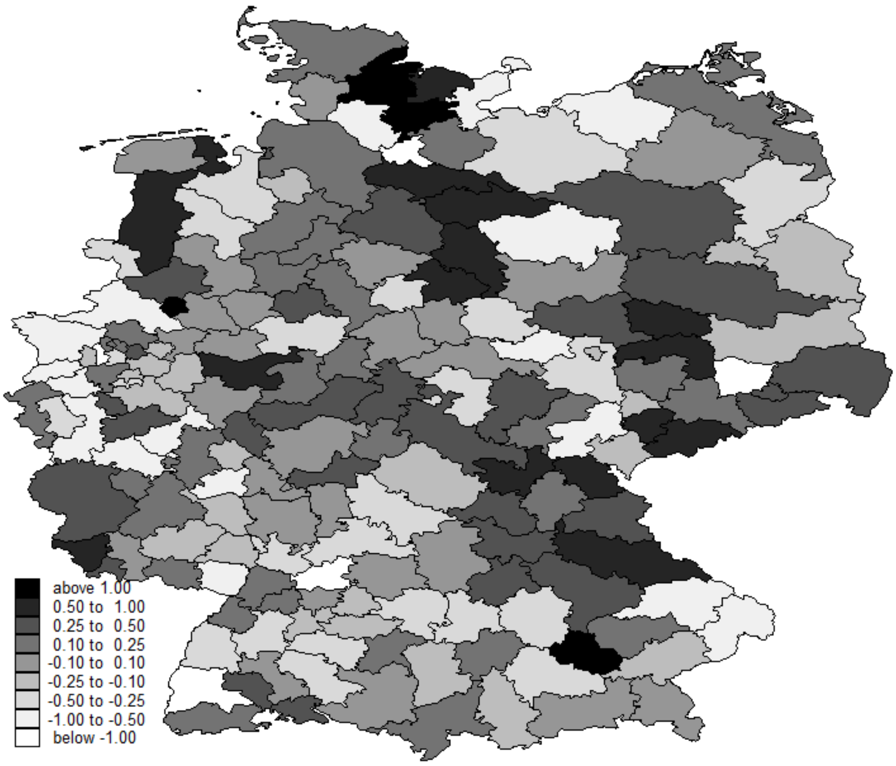
[33]We include dummy variables characterizing 5%-quantiles of the average yearly emigration rate on the county level in the period 1995 to 1997 (see Table 3.10 in Appendix 3.6 for full specification).

Figure 3.3: Geographical Distribution of Local Treatment Intensities in Germany

(a) Unconditional Log Treatment Intensity Z_j



(b) Conditional Log Treatment Intensity $(Z_j|R_j)$



Note: Depicted is the geographical distribution of the unconditional log treatment intensity in 2006 (Figure 3.3a) and the log treatment intensities in 2006 conditional on regional characteristics (Figure 3.3b) among Local Employment Agencies in Germany.
Source: Statistic of the German Federal Employment Agency.

the regional characteristics is shown in Figure 3.3b, which shows a much more random distribution than Figure 3.3a.

In order to provide further evidence for the plausibility of the exogeneity assumption, we analyze (i) the correlation between the observed individual characteristics and the instrument as an indicator for potential correlation between unobserved characteristics and the instrument (similar to the test by Altonji et al., 2005, who compare individual control variables based on different values of the instrument), and (ii) the existence of regional clusters, i.e., whether the LEA's preferences are affected by the preferences of neighboring districts. With respect to the first, we regress in a first step (equation 3.4) the instrument on regional characteristics R_{jt} and time characteristics T_t to eliminate the part of the instrumental variation that arises due to regional and seasonal differences. The resulting residuals $\hat{V}_{jt}$ are expected to reflect the LEAs' preferences for mobility programs. In a second step (equation 3.5), we regress the residuals $\hat{V}_{jt}$ on the large set of individual characteristics X_i.

$$Z_{jt-1} = \alpha_1 R_{jt} + \alpha_2 T_t + V_{jt} \tag{3.4}$$

$$\hat{V}_{jt} = \alpha_3 X_i + U_i \tag{3.5}$$

Table 3.4 summarizes the estimation results. It can be seen that the regional characteristics explain a large part (72%) of the instrumental variation (see upper panel of Table 3.4). Furthermore, once adjusted for the regional characteristics, only a few of the observed individual characteristics have a significant influence on the adjusted instrument (lower panel of Table 3.4). In total, we observe 55 individual characteristics, while only five coefficients are significant at the 10%-level, three at the 5%-level, and one at the 1%-level (see column two of Table 3.4) and the R^2 strongly decreases when conditioning on regional characteristics.[34]

Furthermore, we provide evidence for the absence of regional clusters with respect to the instrumental variable. The existence of regional clusters would question the exogeneity of LEA's preferences as it would suggest that the preferences of one LEA are influenced by those of neighboring states. Therefore, we regress the instrumental variable on the average value of the instrumental variable in the neighboring districts. The results are shown in Table 3.5. It can be seen that the significant correlation between the LEA's treatment intensity and those of the neighboring LEA districts disappears completely once we include the regional control variables. In summary, the presented evidence suggests that our instrument, conditional on regional characteristics, creates exogenous variation

[34]Full estimation results for Equation 3.4 and 3.5 can be found in Table 3.11 and 3.12 in the (Section 3.6).

Table 3.4: Sensitivity Analysis: The Impact of Individual Characteristics on the Adjusted Instrument

	Instrumental variable	
	Raw	Adjusted
	Z_j	$\hat{V}_j$
	(1)	(2)
Equation 3.4: Regional and time characteristics R_j, T_t		
R^2		0.722
Adjusted R^2		0.719
Equation 3.5: Individual characteristics X_i		
Number of statistically significant coefficients at		
10%-level (*)	18	5
5%-level (**)	15	3
1%-level (***)	13	1
R^2	0.171	0.016
Adjusted R^2	0.164	0.007

Note: Depicted are the number of statistically significant variables (in total 55) at the 10%/5%/1%-level, when estimating the effect of observed individual characteristics on predicted residuals after regressing the instrumental variable on regional characteristics. $P-$Values are shown in brackets.

with respect to the job seekers willingness to apply for distant jobs. In Section 3.4.4, we present further robustness checks with respect to unobserved regional heterogeneity.

Table 3.5: Test on the Existence of Regional Clusters

Log treatment intensity	OLS (1)	OLS (2)
Log avg. treatment intensity in neighboring districts	0.856*** (0.062)	-0.140 (0.231)
Regional characteristics R_j		✓
No. of observations	176	176
R^2	0.525	0.682
Adjusted R^2	0.522	0.551

Note: Depicted are OLS estimates regressing the LEA's log treatment intensity in 2006 on log average treatment intensity of all neighboring LEA districts. Standard errors in parenthesis. */**/*** indicate statistically significance at the 10%/5%/1%-level.

Finally, in order to test whether the exclusion restriction is fulfilled and we identify the correct channel, i.e., the LEA's preferences for mobility programs, we run different placebo tests within the first stage using the treatment intensity for other ALMP programs (job creation schemes, vocational training), the intensity of benefit sanctions and the likelihood of corporate insolvencies as alternative instruments. All three factors might be correlated with our instrument and influence the individual decision to search for distant jobs.[35] As shown in column 2 to 5 in Table 3.3, none of the alternative factors have a significant impact on the individuals' willingness to apply for distant jobs. This makes us confident that the LEAs preferences for mobility programs is the only factor affecting individuals' search radius, supporting the validity of the exclusion restriction.

Monotonicity Finally, the monotonicity condition requires the probability to search for distant jobs to be a (positive) monotonic function of the instrument. The assumption would be violated if individuals would reduce distant job search due to a higher treatment intensity (existence of defiers). In our case, one might be concerned about the commuting assistance as it might encourage job seekers to apply for jobs within commuting time and hence, reduce search activities which would involve a relocation. However, as already discussed in Section 3.2.2, the commuting assistance can be expected to encourage job seekers to mainly search for jobs involving long-distance commuting, which could also be classified as distant jobs. Moreover, in practice caseworkers inform job seekers about all types of mobility programs (not commuting assistance only) which makes it very unlikely that a job seeker decides to stop searching for distant jobs once he/she receives the information. In addition, we also re-estimate the first stage using the entries into commuting assistance only to construct the instrument. If the monotonicity assumption is violated (due to commuting assistance), we would expect to find a negative coefficient for the treatment intensity of commuting assistance. However, column 6 of Table 3.3 shows a clear positive and significant coefficient. This means that commuting assistance does not reduce the job seekers willingness to search for distant jobs. Therefore, based on our argumentation and the first stage evidence, we assume that the monotonicity assumption is fulfilled.

3.3.2 Estimation Strategy

Assuming the treatment intensity is a valid instrument, we then estimate the treatment effect δ using the two-stage least squares estimator (2-SLS, e.g. Angrist and Imbens, 1995):

$$D_i = \alpha_1 + \gamma Z_{jt-1} + \beta_1 X_i + \pi_1 R_{jt} + T_t + U_i \tag{3.6}$$

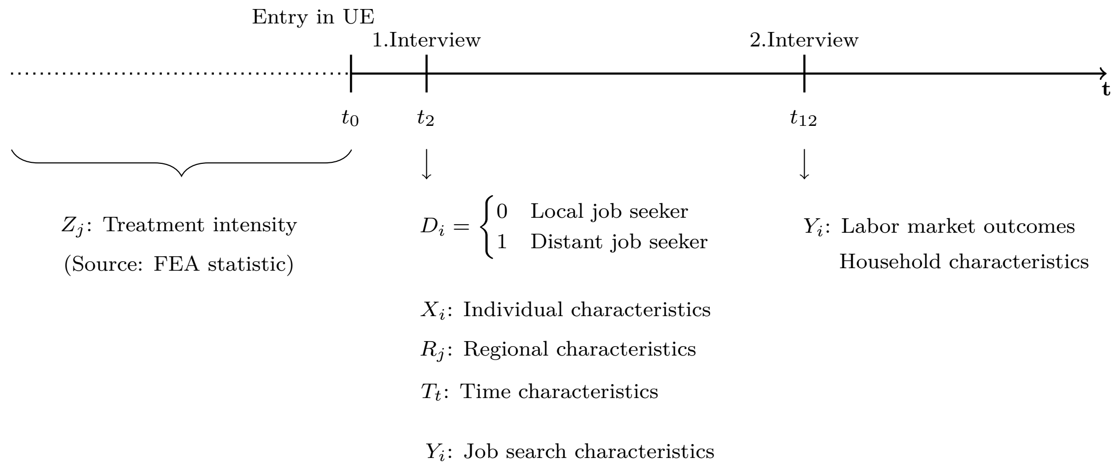
$$Y_i = \alpha_2 + \delta \hat{D}_i + \beta_2 X_i + \pi_2 R_{jt} + T_t + V_i, \tag{3.7}$$

where i denotes the individual, j the local employment agency district where the individual is located and t the year in which the individual entered unemployment. Y_i denotes the outcome variable of interest as defined in Section 3.2.3. D_i is a dummy variable indicating

[35] A higher intensity in mobility programs might automatically imply a lower likelihood to receive other ALMP programs, which might have an impact on individuals willingness to search for distant jobs. Similar arguments apply to benefit sanctions which might be used by employment agencies to influence the individuals search behavior. The local intensity of corporate insolvencies is used due to findings by Neffke et al. (2016) showing that firm closures affect the regional mobility of displaced workers.

distant job search and Z_{jt-1} is the instrumental variable as defined before. X_i contains control variables on the individual and R_{jt} on the regional level, while T_t contains time characteristics to capture common time trends affecting both the instrument and the outcome variables. It is important to note that the outcome variables Y, the observable characteristics X and the treatment indicator D vary only at the individual level, while the instrument Z and the regional characteristics R are region-/time-specific. This is because we observe each individual i only once, i.e., for each individual the timing t and the district j of entry into unemployment are fixed. See Figure 3.4 for the design of the empirical analysis and the full list of control variables.

Figure 3.4: Empirical Setting



Individual characteristics (X_i)
1)Scoio-demographic and household characteristics: Age, gender, marital status, school leaving degree, level of higher education, children in household.
2)Labor market history: Unemployment benefit receipt, level of unemployment benefits, share of months spent in employment/unemployment since age 18, employment status before unemployment.
3)Personality traits: Openness, conscientiousness, extraversion, neuroticism, locus of control.
4)Expectations and socio-cultural characteristics: Expected probability to participate in ALMP program, expected monthly net income, number of good friends outside the family, father has A-level qualification, life satisfaction, writing and language skills in German/English, employment status partner, home-/car-ownership.
Regional characteristics (R_j)
Place of residence (East- or West-Germany), local unemployment rate, local vacancy rate, local GDP per capita, local share of working population in different sectors (agriculture, industry and service), local average regional mobility rate 1995-1997.
Time characteristics (T_t)
Calender month of entry into unemployment, time between entry into unemployment and the first interview.

Note: This figure illustrates the empirical setting. All individuals enter unemployment at t_0 and received the first (second) interview after two (12) months. The distant job search indicator D as well as the control variables X_i and R_j are measured at the first interview (t_2). Concerning outcome variables, the job search characteristics are measured at t_2, while the labor market outcomes and household characteristics are measured at t_{12}. The instrument is constructed based on the last year before entry into unemployment ($t_{-12} - t_0$).

Given the empirical model, our focus is on estimating the coefficient on being a distant job seeker δ on the allocation of search effort and subsequent labor market outcomes. According to the seminal work of Imbens and Angrist (1994), using an instrumental variable approach allows us to identify the LATE which characterizes the treatment effect on the subgroup of compliers, those individuals who react to a change of the instrument (see also Heckman and Vytlacil, 1999; Imbens, 2001, who discuss the LATE including covariates). In our setting, when using the local treatment intensity as an instrument which is assumed to proxy the LEAs preferences for mobility programs, the LATE concept is highly useful as we identify the effect on those job seekers who actually change their search behavior due to differences with respect to the regional-specific policy style.

Using the 2SLS estimator, we implicitly assume that our instrument affects the decision to search for distant jobs first, and based on this decision, job seekers determine the remaining search strategy (as included in Y in the second stage). Alternatively, one could argue that job seekers simultaneously decide about all aspects of their job search strategy including the search radius as well as the level of search effort and the choice of different search methods which would imply a violation of the exclusion restriction. However, with respect to search effort, we argue that the decision to search for distant jobs is a necessary condition for investing effort in distant search at all. Therefore, the decision on search radius has to be taken first, before deciding on how to divide search effort. Moreover, Table 3.9 in Appendix 3.6 shows the results of the reduced-form estimation of the instrument on various job search characteristics. It can be seen that the local treatment intensity only affects the allocation of search effort through the decision to search for distant jobs (as explained before), while there is no impact on reservation wages or the search channels used. This suggest that our empirical strategy is appropriate and identifies the impact of distant job search on search characteristics (as included in Y) for those job seekers who start searching for distant jobs due to the availability of mobility programs.

3.4 Baseline Results

Table 3.6 presents our main results, column 1 contains the unconditional comparison between distant and local job seekers, column 2 the OLS results and column 3 the 2SLS results using the conditional treatment intensity as an instrument for distant job search. Substantial differences are partially visible between the OLS and the 2SLS results which can be explained by two reasons: (i) The OLS estimates simply compare average local and distant job seekers, while due to the LATE interpretation, 2SLS results are only informative for job seekers who actually change their search behavior due to the availability of the mobility programs. This group might differ substantially from the full population of all distant job seekers. (ii) A second explanation refers to the potential endogeneity of the search behavior. As local and distant job seekers might differ systematically with respect to unobserved characteristics, assuming that we use a valid instrument, 2SLS estimates are expected to allow for a causal interpretation of the result, OLS estimates might suffer from a selection bias.

3.4.1 Job Search Behavior

Panel A of Table 3.6 presents the results with respect to individuals' job search behavior as measured in the first wave of the survey. As discussed in Section 3.2.2, mobility programs reduce the costs of distant job search which is expected to encourage job seekers to spend more effort into distant job search activities. This is confirmed empirically by the 1st stage estimation (see Section 3.3.1 and Table 3.3). However, as the increased effort in distant job search might result in a reduction of local search effort, the net effect on total search effort is from a theoretical perspective ambiguous. Our estimation results now show a substitution effect in the sense that job seekers seem to shift their effort from local to distant job search in response to mobility programs. In fact, job seekers who apply for distant vacancies due to the availability of mobility programs send out about 1.3 less applications per week for local jobs (see 2SLS estimate in column 3 in Table 3.6). The increase in the number of applications for distant jobs corresponds to the reduction in local job search as indicated by the insignificant effect on the total number of applications. Furthermore, we do not find a significant difference in reservation wage. It seems that the positive effects (higher job offer arrival rate, lower search costs) outweigh the negative effects (cost reduction of accepting a job offer).

3.4.2 Employment Probabilities

Given the adjusted job search strategy of unemployed job seekers due to the availability of mobility programs, i.e., shift in search effort towards distant job search, the question remains how this affects individuals' labor market performance. The theoretical job search model does not predict a clear conclusion due to opposing effects on the reservation wage and job search effort. Panel B of Table 3.6 shows the empirical results for the labor market outcomes. With respect to regular employment, it can be seen that the increase in distant job search effort (due to mobility programs) leads to a 16 percentage point higher probability to be regular employed 12 months after entry into unemployment. In addition to the static estimation of the employment probability at t_{12}, we also estimate the monthly exit rate to employment within the 12 months period starting at entry into unemployment. Therefore, we apply a discrete time duration model where —in contrast to the standard literature that usually applies a logit or complementary log-log specification— we specify a linear probability model in order to adopt the 2SLS estimator. The duration model complements the static estimation as it allows conclusions with respect to the job

Table 3.6: Main Estimation Results

Outcome variable	OLS (1)	OLS (2)	2SLS (3)
A. Job search behavior (measured in wave 1)			
Average number of job applications per week			
Local jobs	-.064	-.031	-1.338***
	(0.069)	(0.078)	(0.343)
Total	0.849***	0.812***	0.273
	(0.078)	(0.087)	(0.352)
Log hourly reservation wage[(a)]	0.064***	-.018*	0.047
	(0.013)	(0.009)	(0.062)
B. Labor market outcomes (measured in wave 2)			
Regular employment	0.046***	0.001	0.157*
	(0.014)	(0.015)	(0.089)
Exit rate to regular employment[(b)]	0.011***	-.0001	0.063***
	(0.003)	(0.004)	(0.021)
Regular self-employment	0.016***	0.012**	-.008
	(0.006)	(0.006)	(0.043)
Subsidized self-employment	0.009*	0.008	-.092**
	(0.005)	(0.006)	(0.041)
Log hourly earnings in wave 2[(c)]	0.081***	0.033**	0.146**
	(0.019)	(0.016)	(0.073)
Weekly working hours[(c)]	5.950***	1.449***	-4.384*
	(0.493)	(0.469)	(2.408)
C. Job related and household variables (measured in wave 2)			
Relocation between wave 1 and wave 2 (on county level)	0.097***	0.075***	0.136***
	(0.009)	(0.009)	(0.04)
Receipt of mobility assistance related to first transition	0.030***	0.022***	0.108***
	(0.004)	(0.004)	(0.02)
Log equivalence income in wave 2	0.022	0.004	0.343***
	(0.017)	(0.016)	(0.103)
Life satisfaction (0=low, 10=high)	-.051	-.061	-.089
	(0.054)	(0.056)	(0.326)
Successful search channel			
Doing research on the Internet	0.067***	0.032***	0.206***
	(0.009)	(0.009)	(0.048)
Contacting friends, acquaintances, family etc.	-.007	-.007	0.068
	(0.009)	(0.011)	(0.06)
Control variables			
Individual characteristics	No	Yes	Yes
Regional characteristics	No	Yes	Yes
Time characteristics	No	Yes	Yes
No. of observations	6,424	6,424	6,424
F-statistic for weak identification			13.85

Note: Depicted are estimated differences between distant and local job seekers for different outcome variables using OLS and 2SLS estimation. Standard errors are shown in parenthesis and are clustered at the regional level (LEA district). ***/**/* indicate statistically significance at the 1%/5%/10%-level. Full estimation results for selected outcome variables can be found in Table 3.10 in the Appendix (Section 3.6).

[(a)] Reservation wages are only observed for individuals who are still unemployed during the first interview (N=4,523; F-statistic=7.86).

[(b)] Results are based on a discrete time duration model.

[(c)] Earning information and working hours are only observed for individuals in (self-)employment at the second interview (N=3,991; F-statistic=10.40).

finding prospects. The static model only considers existing employment spells at t_{12} while the duration model considers all transitions to employment, even those which might have ended already before t_{12}. Similar, to the static employment effect, we find a 6.3 percentage points higher exit rate into regular employment for distant job seekers.

Another interesting observation is that the increased search radius in response to the mobility programs does lead to a reduction in subsidized self-employment (while it has no significant effect on regular self-employment). In Germany, unemployed job seekers are eligible to generous start-up subsidies when starting their own business (see e.g. Caliendo and Künn, 2011). Unemployed individuals try to escape unemployment by starting their own business, in particular when regular jobs are very limited. Our finding now indicates that the availability of mobility programs seem to reduce the dependence on start-up subsidies, most likely as it increases job seekers' search radius and hence, job opportunities.

Finally, Panel B of Table 3.6 also shows information regarding hourly earnings and working hours (conditional on being employed in wave 2). It can be seen that distant job seekers (due to the availability of mobility programs) realize significantly higher earnings than local job seekers (+15%) and work less hours per week (-4.4 hours).

3.4.3 Job Related and Household Variables

Beside the classical labor market outcomes, the rich survey information allow us to consider also other outcome variables which are related to labor market performance and household characteristics (see Panel C of Table 3.6). Unsurprisingly, distant job seekers also face a significantly higher likelihood to move to a different county (about 14 percentage points) and to receive a subsidy out of the mobility programs (about 11 percentage points). In addition to the positive earnings effect, the effect on household income is also positive and statistically significant (about 28%). The effect on household income can be explained, on the one hand by the higher labor market attachment of the partner (partners of distant job seekers are about 14 percentage points more likely to be full-time employed), and on the other hand by the higher employment probability of distant job seekers. Furthermore, we find no significant effect on life satisfaction.

3.4.4 Sensitivity Analysis

Unobserved Regional Heterogeneity

In the main estimation, we use the lagged treatment intensity with respect to mobility programs as an instrument for the decision to search for distant jobs. Thereby, we assume that conditional on X_i, R_{jt} and T_t the instrument is independent of regional specific preferences for geographical mobility which might be correlated with the individual outcome variables. As this assumptions is crucial for our identification strategy to hold, we test its justification by (i) applying an alternative instrument which is less likely to be correlated with regional specific preferences for geographical mobility and (ii) including regional fixed effects.

As described in Section 3.2.1, two out of the six mobility programs are not directly related to geographical mobility but nevertheless categorized as mobility programs due to administrative reasons which implies that the LEA assigns a joint budget to all six mobility programs. We exploit this administrative feature to construct an alternative instrument that only takes entries into transition and equipment assistance into account. Using the alternative instrument will reduce the potential influence of unobserved regional heterogeneity as the two programs are not directly related to geographical mobility of the unemployed. Nevertheless, it can be expected to remain relevant for individuals' decision to search for distant jobs as entries into all six programs are positively correlated due to one joint budget. As a second robustness check, we include regional fixed effects to cover time-invariant unobserved regional heterogeneity. However, the low number of observation within the survey prevents regional fixed effects on the LEA level. Therefore, we include fixed effects on a higher regional level and divide Germany into six different geographical areas.[36] In combination with the dummy variables for past emigration rates this allows us to compare only LEA districts that are located within one of the six regions and the workforce had similar preferences for regional mobility in the past.

Table 3.7 shows the estimation results for selected outcome variables.[37] The first two columns contain the main estimation results using the original instrument excluding and including regional fixed effects, while column three and four show the results using the alternative instrument. First of all, it can be seen that the first stage estimates are (as expected) smaller for the alternative instrument, but still statistically significant. The resulting F-statistics decreases below the critical value of 10 suggesting that there might be a weak instrument problem. We have to keep this in mind when interpreting the results. The estimated coefficients for the different outcome variables are very similar across columns. And although minor differences in point estimates exist, all results would lead to exact the same conclusions than based on the main estimation results. In summary, the impact of remaining unobserved regional heterogeneity affecting the instrument and individuals' outcome variables also seems to be negligible.

[36] The classification is based on geographical position of the federal state and available number of observations in the survey: 1) North-West: Bremen, Hamburg, Lower Saxony, Schleswig-Holstein; 2) North-East: Berlin, Brandenburg, Mecklenburg-Western Pomerania; 3) West: North Rhine-Westphalia, 4) East: Saxony, Saxony-Anhalt, Thuringia; 5) South-West: Baden-Wuerttemberg, Hesse, Rhineland-Palatinate, Saarland; 6) South-East: Bavaria.

[37] Results for other outcome variables are similarly robust and are available upon request.

Table 3.7: Sensitivity Analysis: Unobserved Regional Heterogeneity

	Instrumental variable			
	Original		Alternative	
	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
First stage: Applied for distant vacancies				
Instrumental variable: Local treatment intensity				
Original	0.04***	0.037***		
	(0.011)	(0.012)		
Alternative			0.022***	0.018**
			(0.008)	(0.009)
A. Job search behavior (measured in wave 1)				
Average number of job applications per week				
Total	0.273	0.328	0.238	0.274
	(0.352)	(0.344)	(0.348)	(0.344)
Local jobs	-1.338***	-1.247***	-1.335***	-1.260***
	(0.343)	(0.334)	(0.339)	(0.334)
Log hourly reservation wage$^{(a)}$	0.047	0.044	0.038	0.038
	(0.062)	(0.064)	(0.062)	(0.064)
B. Labor market outcomes (measured in wave 2)				
Regular employment	0.157*	0.173*	0.218**	0.227**
	(0.089)	(0.089)	(0.091)	(0.089)
Subsidized self-employment	-.092**	-.094**	-.091**	-.094**
	(0.041)	(0.041)	(0.042)	(0.042)
Log hourly wage in wave 2$^{(b)}$	0.146**	0.158**	0.162**	0.173**
	(0.073)	(0.076)	(0.076)	(0.078)
C. Job related and household variables (measured in wave 2)				
Log equivalence income in wave 2	0.343***	0.379***	0.367***	0.402**
	(0.103)	(0.104)	(0.107)	(0.107)
Life satisfaction (0=low, 10=high)	-.089	-.056	0.006	0.023
	(0.326)	(0.331)	(0.342)	(0.348)
Control variables				
Individual characteristics	Yes	Yes	Yes	Yes
Regional characteristics	Yes	Yes	Yes	Yes
Time characteristics	Yes	Yes	Yes	Yes
Regional fixed effects$^{(c)}$	No	Yes	No	Yes
No. of observations	6,424	6,424	6,424	6,424
F-statistic for weak identification	13.850	9.609	7.208	4.334

Note: Depicted are estimated differences between distant and local job seekers for several outcome variables using 2SLS as well as the corresponding first stage estimation results. The alternative instrument does only include entries into transition and equipment assistance while the original instrument considers entries in all six mobility programs (see Equation 3.3). Standard errors are shown in parenthesis and are clustered at the regional level (LEA district). ***/**/* indicate statistically significance at the 1%/5%/10%-level.

$^{(a)}$Reservation wages are only observed for individuals who are still unemployed during the first interview (N=4,523).

$^{(b)}$Wages are only observed for individuals in employment at the second interview (N=3,991).

$^{(c)}$The classification is based on geographical position of the federal state and available number of observations in the survey. The following six fixed effects are included: 1) North-West: Bremen, Hamburg, Lower Saxony, Schleswig-Holstein; 2) North-East: Berlin, Brandenburg, Mecklenburg-Western Pomerania; 3) West: North Rhine-Westphalia, 4) East: Saxony, Saxony-Anhalt, Thuringia; 5) South-West: Baden-Wuerttemberg, Hesse, Rhineland-Palatinate, Saarland; 6) South-East: Bavaria.

Panel Attrition

Another major concern might be selective attrition between the first and second interview. This might be particularly relevant in our setting as individuals who change their place of residence usually face a lower probability of being contacted in the second wave. We test the sensitivity of our results with respect to selective panel attrition by focusing on the job search characteristics because these outcome variables are measured at the first interview and hence, are observable for all individuals in the survey. Table 3.8 shows the 2SLS estimation results using the main estimation sample (restricted to individuals participating in wave 2, column 1) as well as the full sample in wave 1 (column 2). It can be seen that the regression coefficients are almost identical between the two samples. This clearly indicates that selective panel attrition does not bias our results.

Table 3.8: Sensitivity Analysis: Panel Attrition - Job Search Behavior

	Estimation sample (1)	Full sample wave 1 (2)
First stage: Applied for distant vacancies		
Local treatment intensity	0.040***	0.043***
	(0.011)	(0.008)
A. Job search behavior (measured in wave 1)		
Average number of job applications per week		
Local jobs	-1.338***	-1.353***
	(0.343)	(0.313)
Total	0.273	0.219
	(0.352)	(0.32)
Log hourly reservation wage$^{(a)}$	0.047	-.003
	(0.062)	(0.075)
Control variables		
Individual characteristics	Yes	Yes
Regional characteristics	Yes	Yes
Time characteristics	Yes	Yes
No. of observations	6,424	12,326
F-statistic for weak identification	13.85	27.74

Note: Depicted are the 2SLS estimation results with respect to the job search behavior for the main estimation sample (compare Table 3.6) and a full sample including all individuals interviewed in wave 1. Standard errors are shown in parenthesis and are clustered at the regional level (LEA district). ***/**/* indicate statistically significance at the 1%/5%/10%-level.
$^{(a)}$Reservation wages are only observed for individuals who are still unemployed during the first interview. Main estimation sample: N=4,523, F-statistic=7.86. Full sample: N=8,872; F-statistic=19.08.

3.5 Conclusion

We use rich survey data on unemployed job seekers in Germany to analyze the impact of mobility programs on the individuals' job search behavior and labor market outcomes. Mobility programs aim to encourage the geographical mobility among job seekers. The German ALMP offers a wide range of financial support, e.g., subsidy for travel costs to distant job interviews or relocation costs. Job search theory predicts that the availability of mobility programs will lead to an increase in the search effort for distant job vacancies, as the subsidy reduces the relative costs for distant job search compared to local job search. However, theory remains ambiguous with respect to the effect on overall search effort, as well as resulting job finding probabilities. Therefore, this chapter provides first empirical evidence on the question whether the existence of mobility programs affects individuals' willingness to search for distant jobs and hance, their job search strategy and labor market outcomes.

Based on survey data on inflows into unemployment in Germany, we use regional differences in terms of the intensity at which mobility programs are offered to job seekers as an instrumental variable to generate exogenous variation on the individual probability to search for distant jobs. The idea is that job seekers living in a LEA district with a high intensity of mobility programs also face a higher probability to receive knowledge about the existence of the mobility programs (via the caseworker) which is expected to increase their willingness to search for distant jobs. This exogenous variation in the first stage allows us then to estimate the causal effect of searching for distant jobs on other search characteristics and in particular, on subsequent labor market outcomes.

Our estimation results confirm the theoretical prediction that job seekers intensify their search effort with respect to distant job vacancies if they have access to mobility programs. We further show that this increase in search effort for distant jobs results in an equal reduction in search effort for local jobs. This means that job seekers do not increase their overall search effort but rather shift resources from local to distant job search in response to the mobility programs. The increase in search radius results in higher employment rates, higher wages and a reduction of subsidized self-employment. The latter suggests that access to mobility programs apparently reduces the dependence on other forms of governmental support, in this case start-up subsidies. This is a promising finding especially in the light of the relative low costs per participant for mobility programs.

In addition, our findings have two other important implications. First, it is shown that job seekers respond to the availability of mobility programs by changing their actual behavior. The existence of the subsides leads to distant job search and results in a higher level labor market mobility. This suggests that deadweight effects which can expected to be relatively large with such programs, in the sense that those who move would also move without the subsidy, are not a major concern in the present case. Second, the instrumental variable approach gives our estimates the interpretation of local average treatment effects (LATE, see Imbens and Angrist, 1994), i.e., the estimates reflect the impact on those job-seekers who start searching for distant jobs due to the LEA's support of mobility programs. We argue that this parameter is of high interest for policy makers as they have full control over the (regional) intensity of mobility programs (instrument). Given the high regional disparities in terms of unemployment rates within European countries, this chapter provides clear evidence that the introduction or an increase in mobility programs might be an effective tool to increase the search radius of job seekers and hence, reduce unemployment.

3.6 Appendix

3.6.1 Technical Details on the Spatial Job Search Model

The following section discusses the implications of the spatial search model in more detail. Based on the model setup presented in Section 3.2.2, the optimal search strategy is to accept any wage offer with a net wage that exceeds the individual reservation wage ϕ and reject any offer with a net wage that is below ϕ. The reservation wage is defined as the lowest net wage at which the job seeker is indifferent between accepting the job offer and remaining unemployed. For a given discount factor r, the inter-temporal value of accepting a job is defined as the actual net wage:

$$rV_l = w_l \quad \text{for local jobs, respectively} \quad rV_d = w_d - \kappa \quad \text{for distant jobs.} \tag{3.8}$$

The net wage of a local job is simply given as w_l, while the wage of distant jobs is given as the net wage w reduced by the cost associated to the relocation. Given the search cost function $c(e_l, e_d)$, the inter-temporal values of remaining unemployed is given as:

$$rV_u = -c(e_l, \lambda e_d) + \alpha_l(e_l) \int_0^\infty \{V_l(w_l) - V_u\} dF_l(w_l) + \alpha_d(e_d) \int_0^\infty \{V_d(w_d) - V_u\} dF_d(w_d), \tag{3.9}$$

which is, by definition, equal to the reservation wage $\phi = rV_u$ and yields the reservation wage, as defined in equation 3.1 (e.g. Rogerson et al., 2005):

$$\phi = -c(e_l, \lambda e_d) + \alpha_l(e_l) E_{F_l} \max\left\{\frac{w_l - \phi}{r}, 0\right\} + \alpha_d(e_d) E_{F_d} \max\left\{\frac{w_d - (\kappa + \phi)}{r}, 0\right\} \tag{3.10}$$

First-order Condition: Given the job offer rates, the cost function and the wage distribution a job seeker chooses the optimal level of effort on local and distant job search in order to maximizes his inter-temporal utility: $\frac{\partial \phi}{\partial e_l} = \frac{\partial \phi}{\partial e_d} = 0$. Hence, the equilibrium condition can be characterized by equation 3.2:

$$\frac{\partial \alpha_l}{\partial e_l} E_{F_l} \max\left\{\frac{w_l - \phi}{r}, 0\right\} - \frac{\partial c}{\partial e_l} = \frac{\partial \alpha_d}{\partial e_d} E_{F_d} \max\left\{\frac{w_d - (\kappa + \phi)}{r}, 0\right\} - \lambda \frac{\partial c}{\partial e_d} \tag{3.11}$$

where the job seeker equalizes the marginal utility with respect to both types of job search.

Second-order Condition: For the ease of notation, let $R_l = \max\left\{\frac{w_l-\phi}{r}, 0\right\}$ and $R_d = \max\left\{\frac{w_d-(\kappa+\phi)}{r}, 0\right\}$ which can be interpreted as the expected discounted returns to local/distant job search. For condition 3.2, characterizing a maximum it must be true that:

$$\frac{\partial^2\phi}{\partial e_d^2}\frac{\partial^2\phi}{\partial e_l^2} - \frac{\partial^2\phi}{\partial e_d e_l} = \left(R_d\frac{\partial^2\alpha_d}{\partial e_d^2} - \lambda^2\frac{\partial^2 c}{\partial e_d^2}\right)\left(R_l\frac{\partial^2\alpha_l}{\partial e_l^2} - \frac{\partial^2 c}{\partial e_l^2}\right) - \lambda\frac{\partial^2 c}{\partial e_l \partial e_d} > 0 \tag{3.12}$$

$$\text{and} \quad \frac{\partial^2\phi}{\partial e_d^2} = R_d\frac{\partial^2\alpha_d}{\partial e_d^2} - \lambda^2\frac{\partial^2 c}{\partial e_d^2} < 0. \tag{3.13}$$

Search Effort: The effect of λ, respectively κ, on e_d and e_l can be derived by taking the total differential of equation 3.2, which is given as:

$$\left(R_d\frac{\partial^2\alpha_d}{\partial e_d^2} - \lambda^2\frac{\partial^2 c}{\partial e_d^2} + \lambda\frac{\partial^2 c}{\partial e_l\partial e_d}\right)de_d - \frac{\partial R_d}{\partial\kappa}\frac{\partial\alpha_d}{\partial e_d}d\kappa$$
$$= \left(R_l\frac{\partial^2\alpha_l}{\partial e_l^2} - \frac{\partial^2 c}{\partial e_l^2} + \lambda\frac{\partial^2 c}{\partial e_l\partial e_d}\right)de_l - \left(e_d\lambda\frac{\partial^2 c}{\partial e_d^2} + \frac{\partial c}{\partial e_d}\right)d\lambda. \tag{3.14}$$

By assuming that $d\kappa = 0$ and $de_l = 0$, respectively $de_d = 0$, we can derive the derivative of e_d, respectively e_l, with respect to λ:

$$\frac{\partial e_d}{\partial\lambda} = e_d\frac{\lambda\frac{\partial^2 c}{\partial e_d^2} + \frac{\partial c}{\partial e_d}\frac{1}{e_d}}{R_d\frac{\partial^2\alpha_d}{\partial e_d^2} - \lambda^2\frac{\partial^2 c}{\partial e_d^2} + \lambda\frac{\partial^2 c}{\partial e_l\partial e_d}} \tag{3.15}$$

$$\frac{\partial e_l}{\partial\lambda} = -e_d\frac{\lambda\frac{\partial^2 c}{\partial e_d^2} + \frac{\partial c}{\partial e_d}\frac{1}{e_d}}{R_l\frac{\partial^2\alpha_l}{\partial e_l^2} - \frac{\partial^2 c}{\partial e_l^2} + \lambda\frac{\partial^2 c}{\partial e_l\partial e_d}} \tag{3.16}$$

Moreover, we can derive the effect of κ on e_d, respectively e_l, in a similar way:

$$\frac{\partial e_d}{\partial\kappa} = -\frac{\frac{\partial R_d}{\partial\kappa}\frac{\partial\alpha_d}{\partial e_d}}{R_d\frac{\partial^2\alpha_d}{\partial e_d^2} - \lambda^2\frac{\partial^2 c}{\partial e_d^2} + \lambda\frac{\partial^2 c}{\partial e_l\partial e_d}} \tag{3.17}$$

$$\frac{\partial e_l}{\partial\kappa} = \frac{\frac{\partial R_d}{\partial\kappa}\frac{\partial\alpha_d}{\partial e_d}}{R_l\frac{\partial^2\alpha_l}{\partial e_l^2} - \frac{\partial^2 c}{\partial e_l^2} + \lambda\frac{\partial^2 c}{\partial e_l\partial e_d}} \tag{3.18}$$

Assuming that the marginal costs of job search increases with respect to the level of effort: $\frac{\partial^2 c}{\partial e_d^2} > 0$ and $\frac{\partial^2 c}{\partial e_l^2} > 0$, the numerator of equation 3.15 and 3.16 becomes positive. Moreover, the numerator of 3.17 and 3.18 is negative without any further assumptions. Therefore, assuming

$$\lambda^2\frac{\partial^2 c}{\partial e_d^2} - R_d\frac{\partial^2\alpha_d}{\partial e_d^2} > \lambda\frac{\partial^2 c}{\partial e_l\partial e_d} \tag{3.19}$$

ensures that

$$\frac{\partial e_d}{\partial \lambda} < 0 \quad \text{and} \quad \frac{\partial e_d}{\partial \kappa} < 0,$$

and

$$\frac{\partial^2 c}{\partial e_l^2} - R_l \frac{\partial^2 \alpha_l}{\partial e_l^2} > \lambda \frac{\partial^2 c}{\partial e_l \partial e_d} \tag{3.20}$$

leads to

$$\frac{\partial e_l}{\partial \lambda} > 0 \quad \text{and} \quad \frac{\partial e_l}{\partial \kappa} > 0.$$

For instance, assuming that for given levels of search effort e_l and e_d the change of the marginal search costs is the same for an additional unit of e_l, respectively e_d, i.e.: $\frac{\partial^2 c(e_l,e_d)}{\partial e_l^2} = \frac{\partial^2 c(e_l,e_d)}{\partial e_d^2} = \frac{\partial^2 c(e_l,e_d)}{\partial e_l \partial e_d}$, condition 3.19 will hold without any further assumptions, while it will depend on $\lambda, F_l(w_l)$ and $\alpha_l(e_l)$ whether condition 3.20 is fulfilled.

Reservation Wages: The effect of λ on the reservation wage can be directly derived from equation 3.1 is given as:

$$\frac{\partial \phi}{\partial \lambda} = R_l \frac{\partial \alpha_l}{\partial e_l}\frac{\partial e_l}{\partial \lambda} + R_d \frac{\partial \alpha_d}{\partial e_d}\frac{\partial e_d}{\partial \lambda} - \frac{\partial c}{\partial e_l}\frac{\partial e_l}{\partial \lambda} - \frac{\partial c}{\partial e_d}\frac{\partial e_d}{\partial \lambda} - e_d,$$

which becomes negative if the increase of e_l with respect to λ is sufficiently small:

$$\frac{\partial e_l}{\partial \lambda} < \frac{e_d + \frac{\partial c}{\partial e_d}\frac{\partial e_d}{\partial \lambda} - R_d \frac{\partial \alpha_d}{\partial e_d}\frac{\partial e_d}{\partial \lambda}}{\frac{\partial c}{\partial e_l} - R_l \frac{\partial \alpha_l}{\partial e_l}}. \tag{3.21}$$

Similarly, the effect of κ on the reservation wages is given as:

$$\frac{\partial \phi}{\partial \kappa} = \alpha_d \frac{\partial R_d}{\partial \kappa} + R_d \frac{\partial \alpha_d}{\partial e_d}\frac{\partial e_d}{\partial \kappa} + R_l \frac{\partial \alpha_l}{\partial e_l}\frac{\partial e_l}{\partial \kappa} - \frac{\partial c}{\partial e_d}\frac{\partial e_d}{\partial \kappa} - \frac{\partial c}{\partial e_l}\frac{\partial e_l}{\partial \kappa},$$

and becomes positive if the increase of e_l with respect to κ is sufficiently small:

$$\frac{\partial e_l}{\partial \kappa} < \frac{\frac{\partial c}{\partial e_d}\frac{\partial e_d}{\partial \kappa} - \alpha_d \frac{\partial R_d}{\partial \kappa} - R_d \frac{\partial \alpha_d}{\partial e_d}\frac{\partial e_d}{\partial \kappa}}{\frac{\partial c}{\partial e_l} - R_l \frac{\partial \alpha_l}{\partial e_l}}. \tag{3.22}$$

3.6.2 Supplementary Tables

Table 3.9 shows the reduced-form estimation with respect to job search characteristics.

Table 3.10 shows the full estimation results for the first stage estimates, as well as the second stage estimates for three selected outcome variables: (i) the total number of average job applications per week measured in wave 1, (ii) a dummy variable indicating regular employment in wave 2 and (iii) the realized log hourly wage in wave 2. All estimates refer to the baseline specification using the original instrument.

Table 3.11 shows the full estimation results of all regional and seasonal characteristics on the log local treatment intensity using the original instrument referring to equation 3.4.

Table 3.12 shows the full estimation results of all individual characteristics on the log local treatment intensity (column 1), respectively the residual variation after conditioning of regional and seasonal characteristics, using the original instrument. The results refer to equation 3.5.

Table 3.9: Reduced-form Estimation: The Effect of Local Treatment Intensities on Job Search Behavior

	OLS (1)
A. Job search behavior (measured in wave 1)	
Average number of job applications per week	
Distant jobs	0.090***
	(0.023)
Local jobs	-0.134**
	(0.062)
Total	-0.045
	(0.067)
Log hourly reservation wage[(a)]	-0.011
	(0.008)
Preparation of business start-up	0.008
	(0.007)
No. of search channels (0=low, 10=high)	-0.068
	(0.052)
No. of active search channels[(b)] (0=low, 5=high)	-0.033
	(0.026)
Control variables	
Individual characteristics	Yes
Regional characteristics	Yes
Time characteristics	Yes
No. of observations	6,424

Note: Depicted are reduced-form effects of the log treatment intensity on job search outcomes measured in wave 1. Standard errors are shown in parenthesis and are clustered at the regional level (LEA district). ***/**/* indicate statistically significance at the 1%/5%/10%-level.
[(a)] Reservation wages are only observed for individuals who are still unemployed during the first interview (N=4,523).
[(b)] Active search channels include: posting an advertisement myself, contacting social networks, contacting a private agent (with/without) agency voucher and direct applications at companies.

Table 3.10: Baseline Estimation Results: Full Specification

	1st stage		2nd stage					
	Distant job search in wave 1		Avg. weekly number of total applications in wave 1		Regular employed in wave 2		Log hourly wage in wave 2	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Log treatment intensity (original)	0.040***	(0.011)						
Distant job search (1=yes)			0.273	(0.352)	0.157*	(0.089)	0.146**	(0.073)
Socio-demographic characteristics								
School leaving degree (Ref.: None)								
Lower sec. degree	-0.040	(0.039)	-0.191	(0.233)	0.081**	(0.041)	0.060	(0.048)
Middle sec. degree	-0.039	(0.039)	-0.143	(0.231)	0.067*	(0.039)	0.104**	(0.045)
(Spec.) Upper sec. degree	-0.026	(0.041)	-0.319	(0.226)	0.052	(0.043)	0.175***	(0.047)
Higher education (Ref.: None)								
Internal/external prof. training	0.089***	(0.019)	0.017	(0.188)	0.104***	(0.023)	0.055*	(0.030)
University degree	0.231***	(0.024)	-0.070	(0.197)	0.127***	(0.031)	0.141***	(0.040)
Female	-0.004	(0.014)	0.122*	(0.073)	0.031**	(0.015)	-0.052***	(0.017)
Living in West-Germany	0.045*	(0.027)	0.153	(0.125)	-0.009	(0.029)	0.128***	(0.030)
German citizenship	0.027	(0.029)	-0.251	(0.335)	0.095***	(0.034)	0.013	(0.042)
Migration background	-0.006	(0.019)	0.123	(0.093)	0.018	(0.022)	0.008	(0.026)
Age (Ref.: 16-24 years)								
25-34 years	-0.069***	(0.022)	-0.391***	(0.119)	0.089***	(0.019)	0.042**	(0.018)
35-44 years	-0.151***	(0.023)	-0.386**	(0.158)	0.078***	(0.026)	0.036*	(0.021)
45-55 years	-0.165***	(0.026)	-0.512***	(0.149)	-0.026	(0.025)	0.020	(0.023)
Married or cohabiting	-0.024*	(0.014)	-0.037	(0.098)	-0.019	(0.018)	0.017	(0.015)
Children (Ref.: None)								
One child	-0.050***	(0.014)	0.039	(0.099)	0.021	(0.019)	0.037**	(0.018)
Two children or more	-0.054***	(0.019)	-0.173*	(0.099)	-0.001	(0.023)	0.094***	(0.021)
Type of job looking for (Ref.: None)								
Full- or part-time employment	0.077*	(0.042)	-0.189	(0.225)	-0.413***	(0.041)	-0.258***	(0.083)
Full-time employment only	0.060***	(0.014)	0.058	(0.081)	0.024	(0.018)	0.015	(0.019)
Part-time employment only	-0.040***	(0.015)	-0.363***	(0.099)	-0.086***	(0.027)	0.052**	(0.025)
Labor market history								
Unemployment benefit recipient	0.032	(0.024)	0.293	(0.281)	0.006	(0.028)	0.010	(0.037)
Level of UI benefits (100 €/month)	0.006***	(0.002)	0.026**	(0.011)	-0.004*	(0.002)	0.026***	(0.003)
Share lifetime months unemployed	0.009**	(0.004)	-0.007	(0.023)	-0.019***	(0.005)	-0.021***	(0.005)
Share lifetime months employed	-0.000	(0.001)	-0.002	(0.002)	0.001*	(0.001)	0.001	(0.001)
Employment status before UE (Ref.: Other)								
Regular employed	-0.021	(0.019)	-0.068	(0.094)	0.100***	(0.020)	0.028	(0.026)
Subsidized employed	-0.009	(0.027)	0.209	(0.241)	0.046	(0.031)	-0.041	(0.032)
School, apprentice, military, etc.	0.094***	(0.024)	-0.150	(0.143)	0.048*	(0.026)	-0.031	(0.032)
Maternity leave	-0.051**	(0.021)	-0.019	(0.126)	-0.057*	(0.034)	0.043	(0.042)
Personality traits								
Openness (standardized)	0.029***	(0.005)	0.064*	(0.036)	-0.015*	(0.008)	-0.004	(0.008)

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Continued Table 3.10.

Conscientiousness (standardized)	0.000	(0.006)	0.092***	(0.029)	0.009	(0.006)	0.001	(0.006)
Extraversion (standardized)	-0.007	(0.006)	0.068**	(0.031)	-0.009	(0.007)	0.007	(0.007)
Neuroticism (standardized)	-0.012**	(0.006)	-0.067*	(0.035)	0.008	(0.007)	-0.018**	(0.008)
Locus of control (standardized)	0.005	(0.006)	0.015	(0.029)	0.013**	(0.006)	0.008	(0.007)
Expectations and socio-cultural characteristics								
No. of good friends outside family	0.001	(0.001)	0.006	(0.005)	-0.000	(0.001)	-0.000	(0.001)
Father has A-level qualification	0.018	(0.015)	-0.114	(0.081)	-0.015	(0.016)	0.010	(0.016)
Employment status partner (Ref.: No partner)								
Full-time employed	-0.080***	(0.016)	0.119	(0.091)	0.021	(0.017)	0.055***	(0.020)
Part-time employed	-0.043*	(0.026)	0.394***	(0.137)	0.050**	(0.025)	0.045*	(0.027)
Education	-0.027	(0.028)	-0.074	(0.115)	-0.006	(0.025)	-0.016	(0.025)
Unemployed	0.003	(0.031)	0.217	(0.224)	-0.028	(0.032)	0.000	(0.042)
Other	-0.020	(0.026)	0.532**	(0.217)	-0.014	(0.031)	0.090***	(0.032)
Problems with childcare (Ref.: None)								
Some problems	-0.031	(0.022)	-0.137	(0.113)	-0.038	(0.029)	-0.014	(0.027)
Big problems	0.011	(0.022)	0.020	(0.158)	-0.077**	(0.033)	-0.025	(0.041)
Life satisfaction (Ref.: Medium (4-6))								
Low (0-3)	0.043**	(0.019)	0.170	(0.124)	-0.064***	(0.021)	-0.049*	(0.026)
High (7-10)	-0.022*	(0.012)	-0.189***	(0.060)	0.011	(0.013)	0.020	(0.013)
Expected ALMP participation probability (Ref.: Missing)								
Low (0-3)	0.008	(0.017)	-0.076	(0.171)	0.039*	(0.023)	0.026	(0.025)
Medium (4-6)	0.007	(0.019)	-0.208	(0.186)	0.018	(0.021)	0.007	(0.026)
High (7-10)	0.027	(0.020)	-0.022	(0.169)	0.019	(0.024)	0.004	(0.026)
Expected monthly net income (Ref.: Missing)								
≤ 25%-quantile	-0.044**	(0.019)	0.148	(0.168)	-0.253***	(0.027)	-0.140***	(0.025)
25-50%-quantile	-0.051**	(0.025)	0.152	(0.184)	-0.212***	(0.028)	-0.084***	(0.030)
50-75%-quantile	-0.018	(0.022)	0.176	(0.186)	-0.235***	(0.029)	-0.059**	(0.026)
>75%-quantile	0.073***	(0.027)	0.298	(0.252)	-0.263***	(0.030)	0.065**	(0.030)
High writing ability German language	0.077**	(0.032)	0.183	(0.158)	0.009	(0.045)	-0.004	(0.046)
High speaking ability German language	-0.013	(0.022)	0.030	(0.113)	0.031	(0.026)	0.015	(0.026)
High writing ability English language	0.053***	(0.018)	0.069	(0.075)	0.038*	(0.020)	0.017	(0.020)
High speaking ability English language	0.067***	(0.018)	0.139*	(0.084)	-0.036*	(0.022)	-0.001	(0.020)
Home-ownership	-0.047***	(0.012)	0.102	(0.078)	0.009	(0.014)	0.040***	(0.014)
Car-ownership	-0.004	(0.011)	0.075	(0.059)	0.070***	(0.014)	0.050***	(0.016)
Regional characteristics								
Local unemployment rate in %	0.008**	(0.003)	0.026	(0.021)	-0.009**	(0.004)	0.003	(0.004)
GDP per capita in €	-0.001	(0.001)	-0.002	(0.004)	0.000	(0.001)	0.000	(0.001)
Local vacancy rate in %	0.003	(0.002)	0.002	(0.008)	-0.001	(0.002)	0.005***	(0.002)
Share of working population in sector (Ref.: Agriculture)								
in Manufacturing sector	-0.004	(0.004)	0.066***	(0.016)	0.004	(0.004)	0.004	(0.004)
in Service sector	-0.004	(0.004)	0.067***	(0.016)	0.004	(0.004)	0.003	(0.004)
Average emigration rate 1995-1997 (quantile) (Ref.: 0-5%)								
5-10%	-0.024	(0.029)	0.046	(0.117)	0.041	(0.041)	0.027	(0.049)
10-15%	-0.041	(0.027)	0.345**	(0.164)	0.042	(0.048)	0.079*	(0.040)
15-20%	-0.110***	(0.034)	-0.004	(0.159)	0.061	(0.051)	0.112***	(0.041)

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Continued Table 3.10.

20-25%	-0.014	(0.027)	0.129	(0.133)	0.033	(0.046)	0.024	(0.041)
25-30%	-0.049	(0.031)	0.220	(0.169)	0.062	(0.048)	0.064	(0.039)
30-35%	-0.053	(0.034)	0.137	(0.146)	0.010	(0.047)	0.074*	(0.045)
35-40%	-0.038	(0.028)	0.195	(0.252)	0.040	(0.055)	0.064	(0.042)
40-45%	-0.015	(0.033)	0.118	(0.151)	-0.014	(0.050)	0.041	(0.043)
45-50%	-0.056	(0.036)	0.207	(0.186)	0.036	(0.048)	0.126***	(0.040)
50-55%	-0.052*	(0.027)	0.123	(0.159)	0.031	(0.053)	0.053	(0.040)
55-60%	-0.055*	(0.030)	0.148	(0.150)	0.005	(0.047)	0.045	(0.042)
60-65%	-0.093**	(0.036)	0.078	(0.173)	0.057	(0.050)	0.064	(0.042)
65-70%	-0.079**	(0.038)	0.070	(0.185)	0.076	(0.048)	0.033	(0.045)
70-75%	-0.098***	(0.029)	0.073	(0.234)	0.079	(0.051)	0.074	(0.046)
75-80%	-0.077***	(0.029)	0.719***	(0.245)	0.061	(0.049)	0.101**	(0.051)
80-85%	-0.057*	(0.032)	0.229	(0.196)	0.072	(0.045)	0.088*	(0.047)
85-90%	-0.032	(0.034)	0.151	(0.199)	0.028	(0.046)	0.070*	(0.042)
90-95%	-0.038	(0.037)	0.023	(0.230)	0.060	(0.056)	0.039	(0.042)
95-100%	-0.049	(0.032)	0.259	(0.200)	0.048	(0.050)	0.058	(0.045)
Seasonal characteristics								
Month of entry into unemployment								
July 2007	-0.012	(0.032)	-0.263*	(0.156)	-0.053	(0.039)	-0.002	(0.041)
August 2007	0.047	(0.031)	-0.033	(0.134)	-0.050	(0.037)	0.002	(0.030)
September 2007	0.009	(0.029)	0.135	(0.145)	-0.023	(0.033)	0.028	(0.034)
October 2007	-0.016	(0.031)	0.018	(0.137)	0.008	(0.034)	0.015	(0.034)
November 2007	-0.014	(0.028)	-0.089	(0.140)	0.005	(0.038)	0.016	(0.032)
December 2007	0.005	(0.035)	-0.251	(0.170)	-0.063	(0.038)	0.044	(0.036)
January 2008	-0.008	(0.033)	-0.211	(0.170)	-0.074*	(0.038)	0.048	(0.041)
February 2008	-0.064*	(0.034)	-0.094	(0.167)	-0.069*	(0.040)	0.033	(0.043)
March 2008	-0.005	(0.032)	-0.041	(0.152)	-0.061	(0.038)	0.058	(0.038)
April 2008	0.006	(0.030)	0.443*	(0.263)	-0.050	(0.036)	0.030	(0.034)
May 2008	-0.040	(0.030)	-0.109	(0.132)	-0.085**	(0.034)	-0.001	(0.036)
Time between entry into unemployment and first interview								
8 weeks	0.028	(0.034)	-0.244	(0.188)	-0.042	(0.047)	0.056	(0.060)
9 weeks	0.064*	(0.035)	-0.197	(0.206)	-0.045	(0.051)	0.066	(0.061)
10 weeks	0.038	(0.035)	-0.355	(0.218)	-0.103**	(0.050)	0.069	(0.060)
11 weeks	0.063*	(0.038)	-0.467**	(0.237)	-0.100*	(0.055)	0.052	(0.062)
12 weeks	0.097**	(0.039)	-0.640***	(0.215)	-0.122**	(0.055)	0.008	(0.066)
13 weeks	0.064	(0.045)	-0.727***	(0.237)	-0.058	(0.059)	0.004	(0.071)
14 weeks or more	0.073*	(0.044)	-0.904***	(0.236)	-0.114**	(0.058)	0.052	(0.067)
Constant	0.641*	(0.381)	-4.765***	(1.562)	-0.089	(0.383)	0.962**	(0.428)
Observations	6,424		6,424		6,424		3991	
R^2	0.216		0.058		0.110		0.286	
Adjusted R^2	0.203		0.043		0.096		0.268	
F-statistic for weak identification	13.850							

Note: Depicted are full 2SLS estimation result for the 1st stage (column 1) and the 2nd for three selected outcome variables (column 2-4). ***/**/* indicate statistically significance at the 1%/5%/10%-level. Standard errors in parenthesis.

Table 3.11: OLS Estimation of Regional/Seasonal Characteristics on Instrument

	Log local treatment intensity Z_j			
	Coef.	SE	Coeff.	SE
Regional characteristics				
Local unemployment rate in %	-0.007	(0.005)	-0.003	(0.004)
GDP per capita in 1,000 €	-0.008***	(0.001)	-0.007***	(0.001)
Local vacancy rate in %	-0.017***	(0.002)	-0.013***	(0.002)
Share of working population in (Ref.: Agriculture sector)	ref.		ref.	
in Manufacturing sector	-0.089***	(0.005)	-0.041***	(0.005)
in Service sector	-0.091***	(0.005)	-0.044***	(0.005)
Average regional mobility rate 1995-97 (quantile) (Ref.: 0-5%)	ref.		ref.	
5-10%	-0.345***	(0.047)	-0.208***	(0.043)
10-15%	-0.342***	(0.046)	-0.187***	(0.042)
15-20%	-0.267***	(0.050)	-0.062	(0.047)
20-25%	-0.434***	(0.050)	-0.152***	(0.047)
25-30%	-0.444***	(0.049)	-0.171***	(0.046)
30-35%	-0.378***	(0.048)	-0.187***	(0.046)
35-40%	-0.277***	(0.050)	-0.081*	(0.046)
40-45%	-0.236***	(0.049)	-0.118**	(0.046)
45-50%	-0.970***	(0.049)	-0.787***	(0.050)
50-55%	-0.508***	(0.052)	-0.339***	(0.048)
55-60%	-0.370***	(0.050)	-0.219***	(0.047)
60-65%	-0.140***	(0.051)	-0.056	(0.047)
65-70%	-0.170***	(0.054)	-0.267***	(0.051)
70-75%	-0.397***	(0.051)	-0.225***	(0.048)
75-80%	-0.189***	(0.051)	-0.173***	(0.047)
80-85%	-0.064	(0.053)	-0.111**	(0.049)
85-90%	0.103*	(0.054)	0.141***	(0.052)
90-95%	-0.001	(0.059)	0.006	(0.055)
95-100%	-0.148***	(0.055)	-0.163***	(0.052)
Living in West-Germany	-1.245***	(0.033)		
Place of residence (Ref.: North)				
West			-0.721***	(0.025)
South-West			-0.411***	(0.026)
South-East			-0.287***	(0.029)
North-East			0.703***	(0.037)
Mid-East			1.017***	(0.036)
Seasonal characteristics				
Month of entry into unemployment (Ref.: June 2007)	ref.		ref.	
July 2007	0.043	(0.041)	0.037	(0.037)
August 2007	0.032	(0.038)	0.032	(0.034)
September 2007	0.054	(0.039)	0.045	(0.035)
October 2007	0.052	(0.039)	0.057	(0.035)
November 2007	0.023	(0.039)	0.018	(0.035)
December 2007	0.034	(0.044)	0.009	(0.039)
January 2008	0.300***	(0.046)	0.298***	(0.041)
February 2008	0.504***	(0.045)	0.499***	(0.040)
March 2008	0.521***	(0.042)	0.516***	(0.038)
April 2008	0.526***	(0.040)	0.524***	(0.035)
May 2008	0.523***	(0.039)	0.519***	(0.035)
Time between entry into UE and interview (Ref.: 7 weeks)	ref.		ref.	
8 weeks	0.037	(0.051)	0.024	(0.046)
9 weeks	0.024	(0.052)	0.016	(0.047)
10 weeks	0.040	(0.053)	0.039	(0.048)
11 weeks	0.010	(0.056)	0.005	(0.050)
12 weeks	0.065	(0.059)	0.056	(0.053)
13 weeks	-0.057	(0.065)	-0.028	(0.058)
14 weeks or more	0.012	(0.061)	0.005	(0.054)
Constant	7.237***	(0.459)	1.521***	(0.463)
No. of observations	3,889		3,889	
R^2	0.722		0.779	
Adjusted R^2	0.719		0.776	

Notes: OLS estimates of regional/seasonal characteristics on log treatment intensity. ***/**/* indicate statistically significance at the 1%/5%/10%-level. Standard errors in parenthesis. The number of observations refer to the realized combinations of LEA districts, months of entry into UE and weeks between the entry and the interview. [(a)] The following regional fixed effects are constructed based on federal states: 1) North (Bremen, Hamburg, Lower Saxony, Schleswig-Holstein), 2) West (North Rhine-Westphalia), 3) South-West (Baden-Wuerttemberg, Hesse, Rhineland-Palatinate, Saarland), 4) South-East (Bavaria), 5) Mid-East (Saxony, Saxony-Anhalt, Thuringia), 6) North-East (Berlin, Brandenburg, Mecklenburg-Western Pomerania).

Table 3.12: OLS Estimation of Individual Characteristics on IV Residuals

	Uncond. instrument Z_j		Adjusted instrument $\hat{V}_j$			
	Coef.	SE	Coef.	SE	Coef.	SE
School leaving degree (Ref.: None)	ref.		ref.		ref.	
Lower sec. degree	-0.123*	(0.069)	-0.042	(0.038)	-0.046	(0.034)
Middle sec. degree	0.218***	(0.069)	-0.025	(0.038)	-0.027	(0.034)
(Spec.) Upper sec. degree	0.073	(0.072)	-0.061	(0.039)	-0.043	(0.036)
Higher education (Ref.: None)	ref.		ref.		ref.	
Internal/external prof. training	0.152***	(0.038)	0.027	(0.021)	0.019	(0.019)
University degree	0.180***	(0.045)	0.044*	(0.025)	0.026	(0.022)
Female	-0.107***	(0.025)	0.016	(0.013)	0.006	(0.012)
German citizenship	0.054	(0.060)	0.011	(0.033)	0.016	(0.030)
Migration background	-0.388***	(0.037)	-0.038*	(0.020)	-0.014	(0.018)
Age (Ref.: 16-24 years)	ref.		ref.		ref.	
25-34 years	-0.011	(0.033)	0.028	(0.018)	0.020	(0.016)
35-44 years	-0.063*	(0.037)	-0.008	(0.020)	-0.006	(0.018)
45-55 years	-0.018	(0.039)	-0.003	(0.021)	0.008	(0.019)
Married or cohabiting	0.076***	(0.029)	0.012	(0.016)	0.011	(0.014)
Children (Ref.: None)	ref.		ref.		ref.	
One child	-0.042	(0.029)	-0.024	(0.016)	-0.013	(0.014)
Two children or more	-0.089**	(0.036)	0.004	(0.020)	-0.009	(0.018)
Unemployment benefit recipient	0.144***	(0.030)	0.032*	(0.016)	0.033**	(0.015)
Level of UI benefits (100 €/month)	-0.019***	(0.003)	-0.003*	(0.002)	-0.003**	(0.002)
Lifetime months in unemployment	0.072***	(0.008)	0.006	(0.004)	0.006	(0.004)
Lifetime months in employment	-0.002*	(0.001)	-0.001	(0.001)	-0.001	(0.001)
Employment status before UE (Ref.: Other)						
Regular employed	0.066*	(0.038)	-0.008	(0.021)	-0.012	(0.019)
Subsidized employed	0.042	(0.053)	-0.009	(0.029)	-0.024	(0.026)
School, apprentice, military, etc.	0.092**	(0.046)	0.020	(0.025)	0.016	(0.023)
Maternity leave	0.089	(0.066)	0.008	(0.036)	0.013	(0.032)
Openness (standardized)	-0.022**	(0.011)	0.008	(0.006)	0.011*	(0.005)
Conscientiousness (standardized)	0.007	(0.011)	-0.001	(0.006)	0.001	(0.006)
Extraversion (standardized)	-0.006	(0.011)	-0.014**	(0.006)	-0.008	(0.006)
Neuroticism (standardized)	-0.019*	(0.011)	-0.010	(0.006)	-0.009*	(0.006)
Locus of control (standardized)	-0.005	(0.011)	-0.002	(0.006)	-0.009	(0.006)
No. of good friends outside family	0.006***	(0.002)	-0.000	(0.001)	-0.001	(0.001)
Father has A-level qualification	0.015	(0.028)	-0.027*	(0.015)	-0.021	(0.014)
Employment status partner (Ref.: No partner)						
Full-time employed	0.042	(0.029)	-0.009	(0.016)	-0.007	(0.014)

Continued on next page.

Continued Table 3.12.

Part-time employed	-0.086*	(0.045)	-0.021	(0.025)	-0.031	(0.022)
Education	-0.022	(0.044)	0.014	(0.024)	0.023	(0.022)
Unemployed	0.207***	(0.057)	0.005	(0.031)	0.002	(0.028)
Other	0.002	(0.048)	0.023	(0.026)	0.016	(0.024)
Problems with childcare (Ref.: None)						
Some problems	0.093*	(0.048)	0.016	(0.026)	0.025	(0.024)
Big problems	0.111*	(0.059)	0.068**	(0.032)	0.050*	(0.029)
Life satisfaction (Ref.: Medium (4-6))						
Low (0-3)	-0.038	(0.038)	-0.013	(0.021)	-0.008	(0.019)
High (7-10)	-0.045**	(0.023)	0.014	(0.012)	0.004	(0.011)
Expected ALMP participation probability (Ref.: Missing)						
Low (0-3)	0.120***	(0.038)	0.005	(0.021)	0.014	(0.019)
Medium (4-6)	0.034	(0.041)	-0.010	(0.022)	0.003	(0.020)
High (7-10)	0.031	(0.038)	0.011	(0.021)	0.016	(0.019)
High writing ability German language	0.103	(0.070)	0.035	(0.038)	0.041	(0.035)
High speaking ability German language	-0.049	(0.043)	-0.002	(0.023)	0.016	(0.021)
High writing ability English language	-0.122***	(0.033)	-0.001	(0.018)	-0.010	(0.016)
High speaking ability English language	-0.140***	(0.033)	-0.007	(0.018)	-0.008	(0.017)
Job search (Ref.: None)	ref.		ref.		ref.	
Full- or part-time employment	0.186**	(0.072)	0.012	(0.039)	0.028	(0.036)
Full-time employment only	0.066**	(0.029)	-0.010	(0.016)	0.003	(0.014)
Part-time employment only	-0.376***	(0.041)	-0.034	(0.022)	-0.020	(0.020)
Homeowner	0.146***	(0.022)	0.037***	(0.012)	0.020*	(0.011)
Expected monthly net income (Ref.: Missing)						
$\leq$ 25%-quantil	0.220***	(0.044)	0.019	(0.024)	0.016	(0.022)
25-50%-quantil	0.052	(0.047)	0.002	(0.026)	-0.007	(0.023)
50-75%-quantil	-0.043	(0.044)	0.004	(0.024)	0.004	(0.022)
>75%-quantil	-0.178***	(0.047)	-0.021	(0.025)	-0.015	(0.023)
Carowner	0.062***	(0.022)	0.030**	(0.012)	0.040***	(0.011)
Constant	-3.419***	(0.123)	-0.091	(0.067)	-0.114*	(0.061)
Including regional fixed effects[a]	No		No		Yes	
Observations	6424		6424		6424	
R^2	0.171		0.018		0.018	
Adjusted R^2	0.164		0.010		0.009	

Note: OLS estimates of individual characteristics on IV residuals of the type I-instrument. ***/**/* indicate statistically significance at the 1%/5%/10%-level. Standard errors in parenthesis. [a]The following regional fixed effects are constructed based on federal states: 1) North (Bremen, Hamburg, Lower Saxony, Schleswig-Holstein), 2) West (North Rhine-Westphalia), 3) South-West (Baden-Wuerttemberg, Hesse, Rhineland-Palatinate, Saarland), 4) South-East (Bavaria), 5) Mid-East (Saxony, Saxony-Anhalt, Thuringia), 6) North-East (Berlin, Brandenburg, Mecklenburg-Western Pomerania).

4 The Return to Labor Market Mobility

In many European countries, labor markets are characterized by high regional disparities in terms of unemployment rates on the one hand and low geographical mobility among the unemployed on the other hand. In order to counteract the geographical mismatch of workers, the German active labor market policy offers a subsidy covering moving costs to incentivize unemployed job seekers to search/accept jobs in distant regions. Based on administrative data, this chapter provides the first empirical evidence on the impact of this subsidy on participants' prospective labor market outcomes. We use an instrumental variable approach to take endogenous selection based on observed and unobserved characteristics into account when estimating causal treatment effects. It is shown that unemployed job seekers who participate in the subsidy program and move to a distant region receive higher wages and find more stable jobs compared to non-participants. Moreover, the positive effects are (to a large extent) the consequence of a better job match due to the increased search radius of participants.[38]

[38]This chapter is based on joint work with Marco Caliendo and Steffen Künn (see Caliendo et al., 2017b).

4.1 Introduction

Many European countries are characterized by high regional disparities in terms of unemployment rates. For instance, the European Commission (Eurostat) reports regional unemployment rates for Germany (France) ranging from 2.7% to 10.8% (7.1% to 15.6%) in 2012, while the Southern European countries face even higher disparities ranging between 4.1% to 19.3% in Italy and 15.6% to 34.4% in Spain. Besides differences in real wages and labor productivity across regions, regional disparities in unemployment rates can be explained, in particular, by regional labor market tightness and a mismatch of vacancies and skills on a regional level (Taylor and Bradley, 1997; Giannetti, 2002). Although these regional disparities exist, geographical labor mobility in European countries is relatively low compared to the US, Canada or Australia (e.g. Puhani, 2001; Decressin and Fatás, 1995; Bentivogli and Pagano, 1999).[39] This is somewhat surprising given that geographical labor mobility is considered to be an efficient adjustment mechanism to macroeconomic shocks (see Blanchard et al., 1992; Borjas, 2006, for evidence on the U.S.). Therefore, the question arises why European unemployed job seekers living in areas characterized by high unemployment rates do not move to more prosperous areas in order to find employment. Besides cultural reasons (preferences for certain regions, social environment, etc.), it is very likely that financial constraints prevent unemployed job seekers to search/accept distant jobs (see e.g. Ardington et al., 2009). In this context, a subsidy covering the moving costs might be a sensible strategy to incentivize unemployed job seekers to relocate to distant regions in order to find employment. In addition to individual labor market gains due to the relocation, such a program might also lead to an overall reduction in unemployment rates as increased geographical labor mobility might shift excess labor supply from depressed to prosperous regions.[40]

In Germany, one such active labor market policy (ALMP) provides unemployed job seekers a monetary subsidy covering the moving costs when starting a job in a distant region.[41] However, it has not been examined as of yet whether the participation in the subsidy program and hence starting a new job in a distant region is a successful strategy for the unemployed. In this chapter, we contribute the first empirical evidence on the

[39] Yearly mobility rates in the US (~3%) are approximately three times larger than the European average (~1%), while the southern European countries that were heavily affected by the recent economic crisis, e.g., Spain, Italy or Portugal, exhibit especially low mobility rates within the EU (e.g. Nickell, 1997; Bonin et al., 2008).

[40] For example, Marinescu and Rathelot (2016) estimate that the US unemployment rate could be reduced by up to 3% when reallocating job-seekers among regions, while Razin and Yuen (1997) show that labor mobility is an income-equalizing force, but policies to facilitate the movement of labor across different regions are needed to exploit these adjustment potentials.

[41] A distant region is defined as a location outside the daily commuting radius. With respect to the program under scrutiny, the daily commuting time between the current and the new location must exceed 2.5 hours (for both ways) in order to be eligible for the subsidy.

labor market return for participants in the program, i.e., we investigate the impact of participation in the subsidy program on prospective labor market outcomes such as wages, job stability and long-term employment probability. Germany is a good example to study such a policy as its labor market is characterized by high regional disparities in terms of unemployment rates and wage levels (e.g. Lehmer and Ludsteck, 2011) while the geographical mobility within the population is rather low as in many other European countries.[42] Given that the program has been successful, the policy might also be interesting for other countries characterized by similar labor markets suffering from low geographical mobility among the unemployed.

Following the model by Rogers (1997), which extends the classical job search model by Mortensen (1986) with respect to the search radius of job seekers, such a subsidy is expected to impact both the job search behavior and subsequent job characteristics of unemployed individuals. On the one hand, the subsidy would directly reduce moving costs, decreasing job seeker's reservation wage for distant jobs and hence increasing the search radius. On the other hand, the increase in the search radius will raise the job offer arrival rate, which is expected to raise reservation wages. It remains ambiguous which effect on the reservation wage dominates. Moreover, job seekers are expected to move – if at all – to regions which show the highest returns to their skills in terms of wages (Borjas et al., 1992) and employment probabilities (Arntz et al., 2011).[43] This will most likely positively affect participants' labor market outcomes.

Based on a random sample of male entries into unemployment from 2005 and 2006 drawn from administrative data, we find that participants in the subsidy program predominately move to regions characterized by better economic conditions compared to their initial place of residence. Furthermore, descriptive statistics indicate improved labor market outcomes among participants, i.e., we find higher prospective employment probabilities, more stable jobs and higher wages than for non-participants. However, these gaps might be explained by structural differences between participants and non-participants and hence exist even in the absence of the treatment. Therefore, we exploit regional variation with respect to the intensity at which the local employment agencies (LEA) assign unemployed

[42]For instance, 68.5% of the prime-age population in Germany still lived in the same federal state in 2008 as where they grew up, which is comparable to the UK (68%), while the numbers are even higher in other European countries such as the Netherlands (73%), France (75%) and Spain (77%), Source: European Value Survey, own calculations. Moreover, Bonin et al. (2008) report that the share of the population that has moved their place of residence within Germany (compared to the year before) is relatively low and constant at about 1.3% within the period 1995-2006. Moreover, see, among others, Arntz (2005); Peukert and Smolny (2011); Lehmer and Ludsteck (2011) and Arntz et al. (2011) for the effects of geographical mobility on the German labor market in general, and Burda (1993); von Hagen (2000); Hunt (2006); Brücker and Trübswetter (2007) and Fuchs-Schündeln and Schündeln (2009) for the determinants and consequences of the East-West transition after the German reunification in 1990.

[43]In practice, those are expected to be areas characterized by better overall economic conditions compared to their current place of residence.

job seekers to mobility programs as an instrumental variable in order to identify causal treatment effects. At the beginning of a calendar year, each LEA receives a fixed budget for ALMP programs from the Federal Employment Agency. While the set of programs is predetermined by the Federal Employment Agency, each LEA decides independently which share of the received budget to spend on which programs, i.e., the intensity at which it uses certain programs. The decision on the allocation of the budget, respectively the policy mix, is taken by the administrative board of a LEA and depends on (i) the local labor market conditions and (ii) the preferences of the LEA.[44] In this context, we use "preferences" as a generic term for the beliefs and experiences of the administrative board with respect to the effectiveness of certain ALMP programs. We eliminate the endogenous variation in the instrumental variable arising from local labor market conditions or the job seekers' influence on LEAs' preferences by controlling for detailed local labor market conditions, including LEA fixed effects and using a lagged instrument in our empirical model. Thereby, we capture time-invariant and time-variant unobserved regional differences. As a result, the remaining variation in the instrument exogenously affects the individual decision to participate in the treatment due to two channels: (i) job seekers living in a district with a relatively high treatment intensity are – compared to job seekers in a district with a low treatment intensity – more likely to receive knowledge about the existence of the program (as mentioned by the caseworker during regular talks); (ii) higher available budgets for mobility programs in high intensity districts increase generosity in terms of subsidy approval. Both channels make job seekers in high treatment intensity districts more likely to search for distant jobs, and finally participate in the subsidy program.

The IV estimation results show that participants earn about 25% higher wages in the new job (which corresponds to 330 Euro/month) and find more stable jobs compared to non-participants. We further show that the positive wage effect is at least partly explained by upward job mobility. The analysis of the effect heterogeneity reveals that the treatment seems to be also beneficial for individuals who are generally less likely to move (e.g., older or married individuals) but are apparently stimulated by the treatment. Several robustness checks confirm the validity of our identification strategy. In particular, we utilize an alternative instrument in order to proxy the LEAs' preferences for mobility programs and find very similar effects.

The chapter is structured as follows. Section 4.2 provides institutional details on the subsidy program and summarizes results of related studies. Section 4.3 describes the data, the definition of the estimation sample, the setting of the empirical analysis

[44] Among others, Fertig et al. (2006) and Blien et al. (2009) illustrate that, since a labor market reform in 1998, LEAs in Germany have a high degree of autonomy when allocating labor market policies, which results in substantial differences with respect to policy styles among regions.

and presents descriptive statistics. Section 4.4 discusses the identification and estimation strategy, Section 4.5 presents the results, and Section 4.6 concludes.

4.2 Institutional Settings and Related Literature

4.2.1 Mobility Assistance in Germany and the Program Under Scrutiny

Programs designed to encourage the inter-regional labor mobility among unemployed job seekers were first introduced in Germany in 1998, whereby the use of these programs increased with the implementation of the "Hartz-Reform", a major labor market reform which was introduced between 2003 and 2005 (see, e.g., Caliendo and Hogenacker, 2012, for details). In their current version, the mobility programs offer unemployed job seekers who are willing to move locally in order to find employment a wide range of support, starting from simple reimbursement of travel expenses for distant job interviews up to financial support for commuting costs or full coverage of transportation costs.[45]

In this chapter, we focus on one particular mobility program – the relocation assistance – as we are interested in the effect of taking up employment in a distant labor market (which requires relocation) on the labor market performance of job seekers. The relocation assistance program provides financial support for the costs associated with a permanent or temporary move to a distant region in order to find employment. In general, all unemployed job seekers who are not able to find a job locally but in a distant region are eligible to the program. Thereby, it is required that the daily commuting time from the current place of residence to the location of the new job would exceed 2.5 hours.[46]

If this pre-condition is fulfilled, the unemployed job seeker faces two options: (i) he/she can move permanently to the new location or (ii) leaves his/her current place of residence unchanged and just lives during the working week at the new location. The second option is called double housekeeping because it requires the job seeker to rent a second accommodation at the new location. The relocation assistance program provides a financial subsidy for both options. A temporary relocation is supported by a monthly payment for renting a secondary flat of up to 260 € for a maximum of six months after the new job has been started. In case that the job seeker decides to move permanently to the new location, the program provides full coverage of the moving costs (with a maximum

[45] In addition, the mobility programs also contain measures which are not directly related to regional mobility, e.g., equipment assistance which supports the acquisition of work clothes and tools and transition assistance that provides an interest-free loan to job seekers in order to cover the costs of subsistence until the first wage payment arrives.

[46] In case that the daily commuting time is less than 2.5 hours, the individuals might be eligible for another mobility assistance program. For instance, the commuting assistance pays a subsidy of 0.20 € per kilometer for the first six months in the new job.

of 4,500 €).[47] The permanent relocation has to occur within a time window of two years after the new distant job has been started. The average costs for both types of relocation assistance in 2006 were about 1,177 € per participant, which is relatively cheap compared to other ALMP programs (e.g., 6,420 € for vocational training programs). Job seekers are not eligible to the subsidy when the employer provides accommodation. Consecutive participation in both program types is possible. The application for the subsidy has to be submitted, together with the employment contract, to the LEA before the move takes place. The final decision about granting the relocation assistance is taken by the caseworker based on the individual labor market situation of the applicant and the available budget of the local employment agency for mobility assistance programs. In practice, the caseworker indicates to the job seeker whether the subsidy would be approved or not before the job seeker accepts a certain job offer.

Table 4.1: Entries in ALMP Programs (in 1,000)

	2005	2006	2007	2008
Entries into unemployment	8,427	8,129	8,155	8,302
Entries into ALMP programs				
Mobility assistance (total)	221	281	352	375
Relocation assistance	46	55	68	68
Vocational training	152	265	360	447
Job creation schemes	78	79	66	67
Wage subsidies	144	226	262	264
Start-up subsidies	91	76	126	119

Source: Statistic of the German Federal Employment Agency.

Table 4.1 shows the number of entries into unemployment and different ALMP programs in Germany within the considered observation window in the empirical analysis. Besides the number of all entries into mobility assistance programs, we separately show the number of recipients for the program under scrutiny. It can be seen that the relocation assistance is a relatively small program compared to other ALMP programs like vocational training or wage subsidies. Less than 1% of the total entries into unemployment receive relocation assistance.

[47] The applicant has to provide three cost estimates from a professional moving company to the LEA. The most cost-efficient offer will be chosen. The subsidy is paid directly to the moving company. Alternatively, the agency can also reimburse the costs for a rental car.

4.2.2 Related Literature

The empirical evidence on similar mobility programs is very scarce internationally and non-existent for Germany. The international evaluation studies indicate positive returns to mobility assistance on labor market outcomes. For instance, Briggs and Kuhn (2008) analyze the Relocation Assistance Program in Kentucky (U.S.) as introduced in May 1998. The program pays a lump sum subsidy of up to $900 to households of welfare recipients given that they accept a full-time job offer that is at least 10 miles away from their current place of residence. Using IV estimation, the authors find a positive and significant effect on both employment and unconditional earnings. However, the results are mixed with respect to the earnings conditional on being employed. A second example for the U.S. is the Moving to Opportunity (MTO) program introduced in 1994 by the U.S. Department of Housing and Urban Development in five metropolitan areas, i.e., Baltimore, Boston, Chicago, Los Angeles and New York City. The program was implemented as a randomized experiment where housing vouchers were offered to low-income families in order to move to better neighborhoods. The aim was to improve their health status, educational opportunities and labor market outcomes. Several studies (e.g. Katz et al., 2001; Ludwig et al., 2005; Kling et al., 2007; Ludwig and Kling, 2007) investigate the effectiveness of this program and find that the MTO program successfully relocated these families to better neighborhoods and partly improved their health status, while there is no significant effect with respect to educational or labor market outcomes. An earlier study by Mueller (1981) finds the U.S. Job Search and Relocation Assistance from 1976 to have a positive effect on the labor market performance of participants. With this program, unemployed individuals who showed a high willingness to relocate were offered different types of job search assistance and financial support for the relocation. Descriptive evidence shows that participants end up with better employment prospects and higher wages. With respect to Europe, Rodríguez-Planas and Benus (2010) investigate the effectiveness of employment and relocation services for unemployed individuals in Romania, which reimburses expenses associated with moving to another community. Using propensity score matching, they find that the program has a positive and significant impact on the employment probability and earnings level of participants. Westerlund (1998) analyzes the effect of mobility grants on internal migration in Sweden. Using a regional fixed effects model, he finds no significant effects of varying grants on labor market mobility. However, the migratory behavior of the unemployed responds to changes in the regional labor market conditions.

4.3 Data, Settings and Descriptive Statistics

4.3.1 Data

This chapter uses the IZA/IAB Administrative Evaluation Dataset, which is based on the *Integrated Employment Biographies* (IEB) as provided by the Institute for Employment Research (IAB) and consists of a 5% random sample of entries into unemployment between 2001 and 2008 in Germany.[48] The IEB are administrative data based on different sources, e.g., employment history, benefit recipient history, training participant history and job search history. They therefore contain 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 characteristics including education, family status and health restrictions. The data do not contain information about working hours and periods in self-employment, time spent in inactivity or when individuals work as civil servants.

4.3.2 Sample Construction, Settings and Definition of Outcome Variables

Table 4.2 shows that the full dataset contains 918,906 individuals. For our analysis, we only consider entries into unemployment[49] (with a minimum duration of two weeks) in 2005 or 2006 whereby the selected individuals must have been employed for at least three months before entering unemployment with a monthly gross income of 600 € or more. The previous employment and earnings condition guarantees a “fresh” sample of entries into unemployment (no returnees from ALMP or periods of sickness, etc.) making the assumption that the selected individuals indeed search for employment plausible. We do not consider cohorts before 2005 to avoid any structural breaks within our observation window due to a major labor market reform in Germany (“Hartz-Reform”). We also exclude cohorts after 2006 in order to have a sufficiently large observation window of up to 48 months after entry into unemployment available (given that the data in its current version end in December 2010). Table 4.2 shows that 127,091 individuals are selected based on these criteria.

We further focus on prime-age (25-55 years) male individuals only because female moving behavior is less elastic with respect to factors such as education (e.g. Compton and Pollak, 2007; Brandén, 2013) or occupational choices (e.g. Halfacree, 1995; McKinnish,

[48] This chapter is based on a weakly anonymized sample of the IEB by the IAB (V9.01). The data can be accessed at the Research Data Center of the Federal Employment Agency at the IAB. For a detailed description of this dataset, see Caliendo et al. (2011); Eberle and Schmucker (2015).

[49] We define unemployment as being registered as unemployed at the Federal Employment Agency with or without benefit receipt including participation in ALMP.

2008), and women are sometimes constrained in the relocation decisions of families ("tied movers", e.g. Bielby and Bielby, 1992; Jürges, 2006; Clark and Huang, 2006). In line with this, we find that our instrument, the LEA's provision of mobility programs, does not determine the moving decision of women. Therefore, we decided to drop women from the analysis in order to have a clear identification of the treatment effect. Furthermore, we exclude individuals who do not find non-subsidized employment within 24 months after entry into unemployment. This condition is required given that the treatment is perfectly correlated with a transition to employment, i.e., every treated individual finds employment. Therefore, excluding control individuals who do not find employment within this time window (N=17,395) reduces the impact of potential unobserved differences between treated and control individuals.[50] Among participants, we exclude 28 observations. In total, 42,775 individuals remain who fulfill these restrictions (see Table 4.2).

Table 4.2: Definition of the Estimation Sample

	Individuals
Full sample (entering unemployment 2001 - 2008)	918,906
Entering unemployment in 2005/2006[a)]	127,091
Age restriction (25-55 years)	95,587
Men only	60,198
Transition to employment within 24 months	42,775
Definition of treatment status[b)]	30,397
Estimation sample	
Participants	538
Non-Participants	29,859

[a)] Entries into unemployment are restricted to individuals who were regular employed at least for the last three months before entry into unemployment with a gross income of at least 600 € per month.
[b)] See Supplementary Appendix A for details.

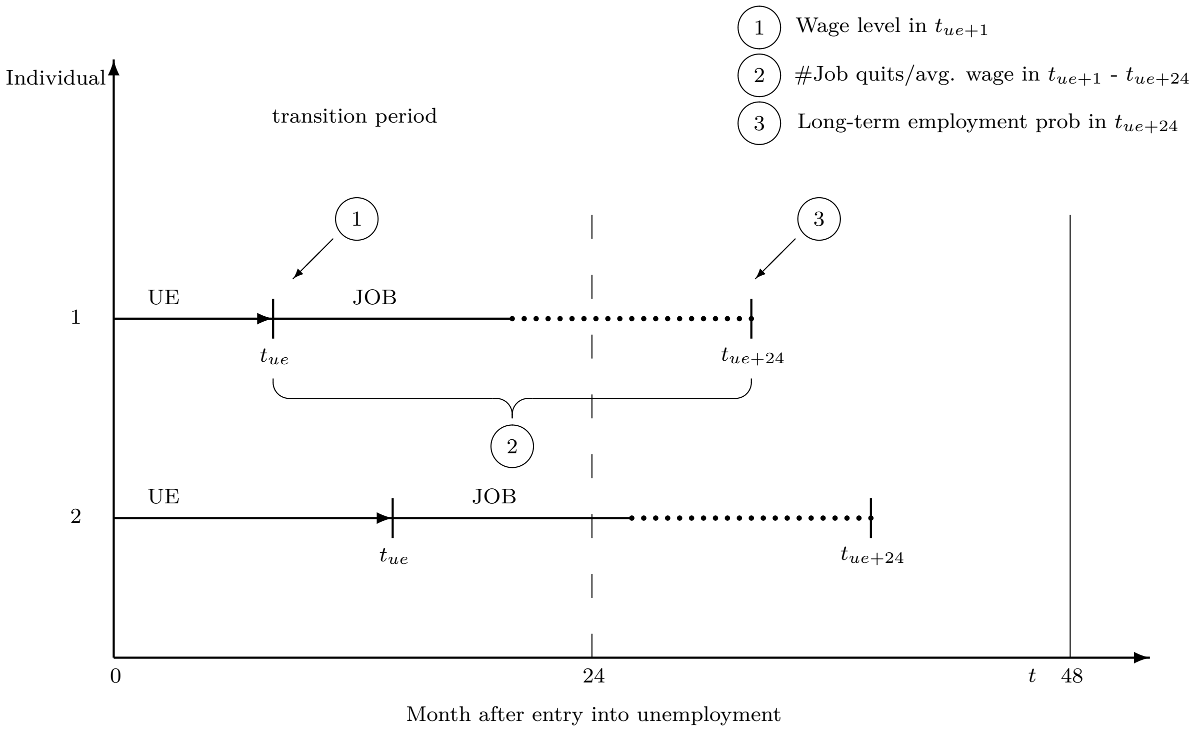
We further have to imply some technical restrictions. From the control group we exclude participants in other mobility programs and individuals who are located in districts where we observe no treated observations. Both restrictions are necessary in order to ensure the validity of our instrumental variable strategy discussed in the following section.[51] Among participants, we exclude 54 observations for which we can not unambiguously relate the subsidy receipt and the transition to employment. The final estimation sample

[50] Non-subsidized employment is defined as employment subject to social security contributions (excluding ALMP) with a monthly income of at least 600 €. The income condition is introduced in order to ensure a certain quality of employment as we do not observe the exact working time in the data. The transition period of 24 months is chosen based on the observation that 95% of all transitions take place within this time window. We discuss the sensitivity of our estimation with respect to both conditions in Section 4.5.4 and present further robustness checks.

[51] While the first restriction is required as the inclusion of those individuals in the control group might violate the monotonicity assumption, the second restrictions allows us to include regional fixed effects.

consists of 30,397 individuals in total, where 538 are participants in relocation assistance and 29,859 are non-participants (see Table 4.2) living in 147 out of 178 LEA districts. The low participation rate in our estimation sample corresponds to the overall low participation rate in relocation assistance as shown in Table 4.1, representing the low geographical mobility among the German population. Details with respect to the sample restrictions, as well as extensive robustness checks analyzing the sensitivity of our empirical analysis with respect to (i) the exclusion of individuals who do not find employment within 24 months, (ii) the exclusion of participants in other mobility programs, (iii) the definition of the treatment group and (iv) an extension of the entry window by two additional years are presented in Appendix 4.7.1.

Figure 4.1: The Transition Process and Labor Market Outcomes



Note: The figure illustrates the setting of the empirical analysis based on two exemplary observations. Every individual starts as being unemployed in t_0 and finds full-time non-subsidized employment within a transition period of 24 months. The exact month of transition is indicated by t_{ue}. For example, individual 1 starts a new job after six months in unemployment (i.e., $t_{ue} = 6$). Furthermore, we measure four baseline outcome variables: The initial daily wage in the first month in the new job (measured at t_{ue+1}), the average daily wage and the number of job quits measured within a period of 24 months after the transition to employment (t_{ue+1} until t_{ue+24}), and the long-term employment probability measured at the end of the observation window (t_{ue+24}).

Figure 4.1 illustrates the empirical setting based on two examples. Every individual starts as being unemployed in t_0 and finds full-time non-subsidized employment within a transition period of 24 months. The exact month of transition is indicated by t_{ue}. For example, individual 1 starts a new job after six months in unemployment (i.e., $t_{ue} = 6$). Given that our observation window is restricted to 48 months after entry into unemployment and the transition period consists of 24 months, we can follow each individual after transition to employment at least for 24 months. To answer the research question of whether participation in relocation assistance has an impact on the labor market performance of participants, we consider four baseline outcome variables (see Figure 4.1): (i) The initial daily wage in the first month in the new job (measured at t_{ue+1}). (ii) To measure wage growth and job stability, we consider the maximum observation period after the transition to employment (t_{ue+1} until t_{ue+24}) and measure the average daily wage and the number of job quits. (iii) We measure the long-term employment probability at the end of our observation window (t_{ue+24}). Note, we cannot estimate causal treatment effects with respect to unemployment duration given that we only observe the date of the subsidy payment, which determines the end of unemployment. To assess the impact on unemployment duration, one would need to observe the date of the program announcement to the job seeker, which is unobservable with the data at hand. Therefore, we focus on post-transition outcomes as described before.

4.3.3 Descriptive Statistics

With respect to the individual characteristics, Table 4.3 shows that participants are positively selected in terms of socio-demographic characteristics and previous labor market performance. Participants are on average younger (37.71 years vs. 38.54 years), better educated (e.g., 39% vs. 14% with upper secondary education, 28% vs. 7% with an university degree), and less likely to be married and have children (reducing the costs of taking up a distant job). With respect to labor market history, higher shares of participants worked in service occupations before entering unemployment, received a substantially higher wage (74.29 € vs. 67.50 €) and spent less time in unemployment (360 vs. 480 days) in the past. Moreover, participants also exhibited a higher willingness to commute in the past. On average, 45% of their jobs during the last five years involved daily commuting to a different local employment agency district, while this only applies to 29% of the non-participants. Furthermore, higher shares of participants had previously participated in any type of mobility assistance (26% vs. 6%), i.e., since its introduction in 1998.[52]

Table 4.3 further shows descriptive statistics with respect to outcome variables. It can be seen that participants remain unemployed longer than non-participants (6.36 vs. 5.39 months). In addition, Figure 4.2 shows the survival and hazard functions for the transition from unemployment to employment. Non-participants face a higher probability of leaving unemployment, in particular in the beginning of the unemployment spell, which explains the shorter average unemployment duration of non-participants. On the one

[52]The knowledge about the treatment is the main channel to identify the treatment effect (as discussed later on). Controlling for previous program participation rules out (at least to some extent) that endogenous selection into the treatment in the past determines the actual participation and may bias our estimation results.

Table 4.3: Selected Descriptive Statistics of Observed Characteristics

	Participants	Non-participants	$P-$value
No. of observations	538	29,859	
Individual characteristics			
Socio-demographic characteristics			
Age (in years)	37.71	38.54	0.02
(Spec.) Upper sec. degree	0.39	0.14	0.00
University degree	0.28	0.07	0.00
Children	0.33	0.40	0.00
Married	0.49	0.58	0.00
Labor market history			
Last daily income (in €)	74.29	67.50	0.00
Time spent in unemployment in last 10 years (in days)	360	480	0.00
Occupation of previous job			
Manufacturing	0.36	0.57	0.00
Technical occupation	0.09	0.04	0.00
Services	0.52	0.33	0.00
Share of jobs which involve commuting in last 5 years[a)]	0.45	0.29	0.00
Previous participation in mobility programs	0.26	0.06	0.00
Outcome variables			
Unemployment duration (in months)	6.36	5.39	0.00
First daily wage in t_{ue+1} (in €)	81.8	65.5	0.00
Average daily wage from t_{ue+1} to t_{ue+24} (in €)	86.3	67.9	0.00
Number of job quits from t_{ue+1} to t_{ue+24}	0.67	0.93	0.00
Employed in t_{ue+24}	0.78	0.73	0.02
Regional characteristics			
Local macroeconomic conditions[b)]			
Local unemployment rate			
at entry in t_0	0.14	0.13	0.00
after transition in t_{ue+1}	0.11	0.13	0.00
Local vacancy rate			
at entry in t_0	0.05	0.06	0.00
after transition in t_{ue+1}	0.08	0.06	0.00
Living in East-Germany			
at entry in t_0	0.49	0.31	0.00
after the transition in t_{ue+1}	0.22	0.29	0.00
Working location in t_{ue+1} relative to t_0			
in the same federal state	0.26	0.88	0.00
in a bordering federal state	0.27	0.08	0.00
in a non-bordering federal state	0.46	0.04	0.00
Distance to new working location in km[c)]	187.7	33.2	0.00
Move from non-urban to urban area[d)]	0.27	0.07	0.00
Move from urban to non-urban area[d)]	0.14	0.06	0.00

Note: All numbers are shares unless otherwise indicated. Individual characteristics are measured at entry into unemployment (t_0). $P-$values are based on a t-test on equal means.

a) Jobs outside the own local employment agency district (place of residence) are defined as jobs including daily commuting.

b) Measured at the employment agency district level.

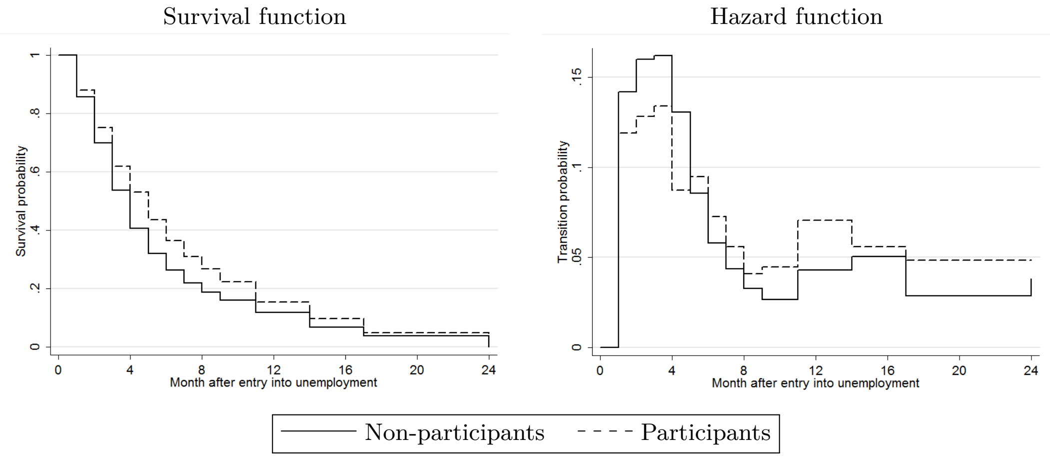
c) Distances between two regions are measured as the linear distance between the corresponding county seats.

d) Cities with more than 100,000 inhabitants are defined as urban areas, all other regions are classified as non-urban.

hand, the longer unemployment duration for participants is somehow surprising given that participants are positively selected in terms of observable labor market characteristics. On the other hand, the delay might be explained by higher reservation wages due to an increased job offer arrival rate due to a time-intensive preparation period of the relocation (searching for distant jobs, finding a new apartment, etc.) or because of a selection effect, i.e., those who to move a distant region might be those who are more selective with respect to jobs or those who fail to find a job locally.

With respect to post-transition labor market outcomes, participants receive higher daily wages at the beginning of the new job (81.8 € vs. 65.5 €) and also later on (86.3 € vs. 67.9 € within t_{ue+1} until t_{ue+24}), have more stable jobs (0.67 vs. 0.93 job quits within 24 months) and have a higher long-term employment probability in t_{ue+24} (78% vs. 73%).

Figure 4.2: Transition from Unemployment to Employment



Note: Depicted are unconditional survival and transition probabilities separated for participants and non-participants for the first 24 months after the entry into unemployment. Due to data anonymization reasons, survival and hazard rates are cumulated for months 10-11, 12-14, 15-17 and 18-24.

In addition to the outcome variables, Table 4.3 shows regional characteristics. Here it becomes clear that, as expected, individuals move predominately to areas characterized by better economic conditions, i.e., lower unemployment rates and higher vacancy rates, or from East to West Germany. Moreover, only a minority of non-participants takes up a distant job at all. While 46% of participants start a new job in a non-bordering federal state, only 4% of non-participants do so. On average, the new working location is 188 (33) kilometers away from the place where participants (non-participants) registered as unemployed.[53] Furthermore, participants predominately move to urban areas.

[53]Distance is measured as the linear distance between the corresponding county seats.

4.4 Empirical Analysis

The descriptive statistics presented in the previous section suggest that, in particular, individuals with rather positive labor market characteristics and performance in the past select into the program. Therefore, it is important to control for a large set of individual characteristics when investigating the causal impact of relocation assistance on the labor market performance of participants. Although we have very informative data available that allow us to control for individual (socio-demographics, labor market history etc.) and regional characteristics, it is very likely that the selection into treatment and thus the decision to move to a distant region also depends on unobserved factors, such as individuals' personality or decisions by the caseworker, which are simultaneously correlated with labor market outcomes.

4.4.1 The Local Treatment Intensity as Instrumental Variable

To overcome this endogeneity issue and get unbiased estimates of the treatment effect, we use an instrumental variable approach. The idea is to find an instrument Z that affects individuals' decisions to participate in the program but not the outcome of interest Y (or only through D) (see for instance, Heckman and Robb, 1985; Imbens and Angrist, 1994; Heckman and Vytlacil, 1999). In this chapter, we exploit the fact that local employment agencies (LEA) in Germany have a high degree of autonomy when allocating the different types of ALMP programs (including mobility programs) among unemployed job seekers.[54] In order to understand the idea behind the instrument, we briefly outline the allocation process of ALMP programs. At the beginning of a calendar year, each LEA receives a fixed budget for ALMP programs from the Federal Employment Agency (FEA). While the set of programs is predetermined by the FEA, each LEA decides independently what share of the received budget to spend on which programs, i.e., each LEA determines their own policy mix (see Blien et al., 2009; Fertig et al., 2006).

Ideally, we would like to measure the LEAs preferences by using the planned budget for mobility programs. However, as this information is not publicly available, we construct the *lagged* local treatment intensity (Z_{jt-1}) as the ratio of entries into mobility programs (N^{ma}_{jt-1}) and the average stock of unemployed job seekers in each LEA district j (N^{ue}_{jt-1}), in order to proxy for the available budget in the current year:[55]

[54] In Germany, 178 LEAs exist in total within our observation window. Similar regional variations are used as instrumental variables for instance by Briggs and Kuhn (2008), Frölich and Lechner (2010) and Card and Krueger (2000).

[55] All numbers are taken from the Statistics of the German Federal Employment Agency. Entries into mobility programs include all mobility programs, i.e., relocation assistance, commuting assistance, travel cost assistance for distant job interviews, as well as equipment and transition assistance (e.g., for work clothes, and financial aid to bridge the time until receipt of the first salary payment).

$$Z_{jt-1} = \frac{N^{ma}_{jt-1}}{N^{ue}_{jt-1}} \times 100. \tag{4.1}$$

The numbers are measured in the year before the considered entry window into unemployment (t-1). This ensures that our estimation sample will not contribute to the construction of the instrument. The instrument ranges between 0.14% and 44.0% (with a mean/median at 4.28/2.08%) within our estimation sample.[56]

We know from the previous literature that the decision about the allocation of the budget within a LEA district is taken by its administrative board and is based on two dimensions: the local labor market conditions and the preferences of the administrative board, capturing beliefs and experiences about the effectiveness of certain ALMP programs (see Blien et al., 2009; Fertig et al., 2006). The empirical challenge is to eliminate endogenous variation in Z arising from local labor market conditions or the job seekers' influence on LEAs' preferences. Therefore, we control for detailed local labor market conditions such as the local unemployment rate, availability of vacancies, GDP per capita, industry structure, and most importantly, include LEA fixed effects and use a lagged instrumental variable (so that our estimation sample does not contribute to the construction of Z). We argue that the remaining variation in Z proxies temporal changes with respect to the LEA's preferences for mobility programs which are exogenous to job seekers' labor market outcomes. Although we cannot completely rule out the existence of other time-varying unobserved factors (others than the LEAs' preferences) it should be noted that this would only invalidate the instrument if these other unobserved factors would be systematically correlated with the individual outcome variables (see discussion of exogeneity in Section 4.4.3).

Given that the job seekers are assigned to local employment agencies based on their place of residence and have no influence on the remaining variation in Z (which most likely reflects the LEAs' preferences), the instrument creates exogenous variation with respect to the individual participation decision stemming from two channels: (i) Through the *information channel* job seekers living in a district with a relatively high treatment intensity are – compared to job seekers in districts with a low treatment intensity – more likely to receive knowledge about the existence of the program and hence to participate in relocation assistance. In Germany, every unemployed job seeker will be assigned to a caseworker. The caseworker and the job seeker meet regularly to discuss the job search strategy, including possible ALMP participation. During these meetings, caseworkers in regions with high treatment intensities, and therefore a strong preference for the program,

[56] We test the robustness of our results with respect to outliers in the distribution of the instrument in Section 4.5.4.

are more likely to inform job seekers about the availability of the program compared to low treatment intensity districts. (ii) The second channel, the *approval channel*, is based on the institutional feature that there is no legal claim to mobility programs but the final decision on subsidy receipt is always at the caseworker's discretion (see Section 4.2.1). Therefore, caseworkers in high treatment intensity regions are more likely to give a positive indication with respect to the final approval of the subsidy due to higher available budgets for these programs. This feature ensures that our instrument remains relevant even if one assumes perfect information among job seekers, i.e., all job seekers know about the availability of mobility programs independent of the treatment intensity.

4.4.2 Estimation Strategy

Using this instrument, we then estimate the treatment effect δ using the two-stage least squares estimator (2-SLS, e.g. Angrist and Imbens, 1995):

$$D_i = \alpha_1 + \gamma Z_{jt-1} + \beta_1 X_i + \pi_1 R_{jt} + \eta_j + \lambda_t + U_i \tag{4.2}$$

$$Y_i = \alpha_2 + \delta \hat{D}_i + \beta_2 X_i + \pi_2 R_{jt} + \eta_j + \lambda_t + V_i, \tag{4.3}$$

where i denotes the individual, j the local employment agency district where the individual is located and t the year in which the individual entered unemployment. It is important to note that the outcome variables Y (as defined in Figure 4.1), the observable characteristics X (socio-demographic characteristics, short- and long-term labor market history, characteristics of the current unemployment spell) and the treatment indicator D vary only at the individual level, while the instrument Z and the regional characteristics R (East Germany, unemployment rate, vacancy rate, GDP per capita, industry structure) are region-/time-specific. This is because we observe each individual i only once, i.e., for each individual the timing t and the district j of entry into unemployment are fixed.

Furthermore, we include time fixed effects (λ) in both equations to capture common time trends affecting both the instrument and the outcome variables.[57] Most importantly for our identification strategy, we also include regional fixed effects at the LEA level (η) in both equations to take unobserved regional heterogeneity into account. This is important as, in addition to regional characteristics R, the treatment intensity might be determined by the job seekers' demand for relocation assistance. This would be problematic if there exist unobserved regional differences that influence the local demand for mobility assistance and labor market outcomes simultaneously. For instance, assuming that unemployed individuals in region A are generally higher motivated than in other regions, one could expect that they are also more willing to move in order to find employment. This would

[57] We include dummies for the calender year and the quarter in which the individual entered unemployment.

increase the demand for relocation assistance in region A and also lead to better labor market outcomes of individuals living originally in region A. Given that the LEA in region A would adjust their policy mix in subsequent years with respect to the increased demand for relocation assistance, the instrument would no longer be independent of the individual labor market outcomes, and estimation results would be biased. Therefore, we include regional fixed effects to capture time-invariant unobserved regional differences, while the time-varying part of these unobserved differences is assumed not to effect our estimates since we exploit the treatment intensity in the year before a job seeker enters unemployment, i.e., Z is measured in $t-1$.

4.4.3 Instrumental Variable Conditions and Discussion of Potential Violations

Assuming that there are heterogeneous effects of the treatment among participants, the instrument has to fulfill three main conditions in order to identify causal local average treatment effects (LATE, see e.g., Imbens and Angrist, 1994). The LATE can be interpreted as the average effect of the treatment on the subgroup of compliers, i.e., the individuals whose participation decision is actually influenced by the instrumental variable.

Table 4.4: First Stage Estimation: Participation in Relocation Assistance

	Baseline Model		Placebo I	Placebo II
	(1)	(2)	(3)	(4)
Local treatment intensity (Z_{jt-1})				
Mobility programs	0.104^{***}	0.128^{***}		
	(0.019)	(0.031)		
Vocational training			-0.007	
			(0.007)	
Job creation schemes				0.022
				(0.015)
Control variables				
Socio-demographic characteristics	✓	✓	✓	✓
Labor market history	✓	✓	✓	✓
Regional information	✓	✓	✓	✓
Information on current unemployment spell	✓	✓	✓	✓
Time fixed effects	✓	✓	✓	✓
LEA fixed effects		✓		
Number of observations	30,397	30,397	30,397	30,397
F-statistic for weak identification	30.89	16.89	0.84	1.99

Note: Dependent variable: D_i (treatment indicator). OLS estimation. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicates significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA-level.

Relevance The relevance condition requires that the instrument significantly determines the individual participation decision. Table 4.4 contains the first stage estimation results of equation (4.2). The first two columns show that the local treatment intensity has a significant impact on the probability of receiving relocation assistance with (column 1) and without (column 2) regional fixed effects. The resulting F-test confirms the relevance of the instrument with F-statistics larger than the critical value of 10, which is usually considered to suggest sufficiently strong instruments (Staiger and Stock, 1997).

Exogeneity The second assumption states that the instrument has to be randomly assigned (independence condition) and has to have no influence on the outcome variables other than through its effect on the program participation probability (exclusion restriction). It requires the instrument to be jointly independent of individual labor market outcomes and treatment assignment, i.e., the outcome Y with or without treatment is independent of Z. As outlined above, the instrument conditional on regional characteristics and fixed effects $(Z_{jt-1}|R_{jt}, \eta_j, \lambda_t)$ proxies the part of the LEA's preferences towards mobility programs, which is expected to be randomly assigned. In addition to the verbal explanation of the institutional setting, we also apply the following regression analysis to convince the reader that $Z_{jt-1}|R_{jt}, \eta_j, \lambda_t$ is indeed exogenous (similar to Altonji et al., 2005, who compare individual control variables based on different values of the instrument):

$$Z_{jt-1} = \alpha_1 R_{jt} + \eta_j + \lambda_t + V_{jt} \tag{4.4}$$

$$\hat{V}_{jt} = \alpha_2 X_i + U_i \tag{4.5}$$

Thereby, we regress in a first step the lagged instrument Z on regional characteristics R in the LEA district of origin j at time t and regional η as well as time fixed effects λ (Equation 4.4). The idea is to adjust the instrument Z for regional and seasonal economic conditions so that the resulting residuals $\hat{V}$ proxy the preferences of the local employment agency for mobility programs. Note that $\hat{V}$ might capture also other time-varying unobserved factors. However, as we consider the lagged instrument Z_{jt-1} and account for current regional characteristics R_{jt} and fixed effects (η_j,λ_t), it can be expected that these factors are not systematically correlated with the individual outcome variables. The estimation results in the upper part of Table 4.5 show that the regional characteristics and in particular, the regional fixed effects explain a large and significant part of the variation in Z (R^2=0.624 without η and R^2=0.885 including η). In a second step, we regress the remaining variation in the instrument $\hat{V}$ on the observed individual characteristics X, which are not included in Equation (4.4). The lower part of Table 4.5 shows the number of statistically significant coefficients when estimating Equation (4.5) by OLS. The first column uses the unconditional instrument Z as a dependent variable, while columns (2) and (3) use the instrument conditional on regional characteristics and fixed effects $\hat{V}$ which has been estimated by Equation 4.4 excluding (column 2) and including (column 3) regional fixed effects. It can be seen that the number of statistically significant coefficients decreases dramatically from columns (1) to (3), indicating very small correlations between the observed individual characteristics and the instrument conditioning on regional characteristics. In addition, the explanatory power of the estimation decreases dramatically as indicated by the declining R^2 from columns (1) to (3). In summary, this suggests that our identification strategy indeed exploits exogenous variation with respect to the participation decision which cannot be explained by systematic differences between participants and non-participants. Given that we control for a large set of observed individual characteristics (in addition to regional characteristics including regional fixed effects) in our main 2-SLS regression analysis, the remaining unexplained variation of the instrument is even more likely to be exogenous.

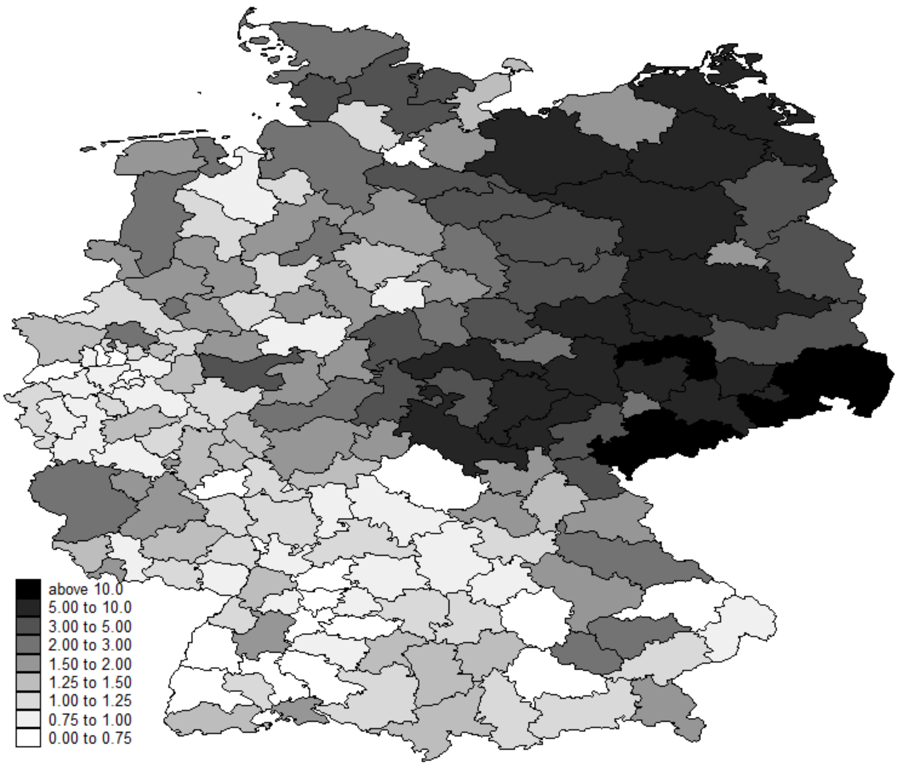
In addition, we provide graphical evidence for the justification of the independence assumption in Figure 4.3. Figure 4.3a on the left shows the distribution of the unconditional treatment intensity (Z) across Germany. Unsurprisingly, the instrument is correlated with local labor market conditions, i.e., the highest treatment intensities can be found in Eastern Germany, while the lowest treatment intensities exist in prosperous areas in the south of Germany. However, as our estimation strategy requires the treatment intensity to be exogenous conditional on observable regional characteristics (including regional fixed effects). Figure 4.3b shows the distribution of the difference with respect to local treatment intensities between 2004 and 2005 conditional on regional characteristics (local unemployment rate, availability of vacancies, GDP per capita and industry structure). This refers to the adjusted instrument ($\hat{V}$ in Equation 4.5) and characterizes the source of identification within the fixed effect estimation. Figure 4.3b visually supports our claim that the adjusted instrument is randomly distributed across Germany.

This evidence makes us very confident that the instrument conditional on regional characteristics and fixed effects $(Z_{jt-1}|R_{jt}, \eta_j, \lambda_t)$ is as good as randomly assigned among regions.[58] However, we might still be concerned that the exclusion restriction is violated, i.e., that the instrument might have an influence on the outcome variables other than through its effect on the program participation probability. Here, we could think about two possible violations: (i) if the instrument would be significantly correlated with the individual participation probability in other ALMP programs, i.e., a higher treatment probability in mobility programs might result in a lower participation probability in another program. If this is true, then our instrument might affect Y not through participation in

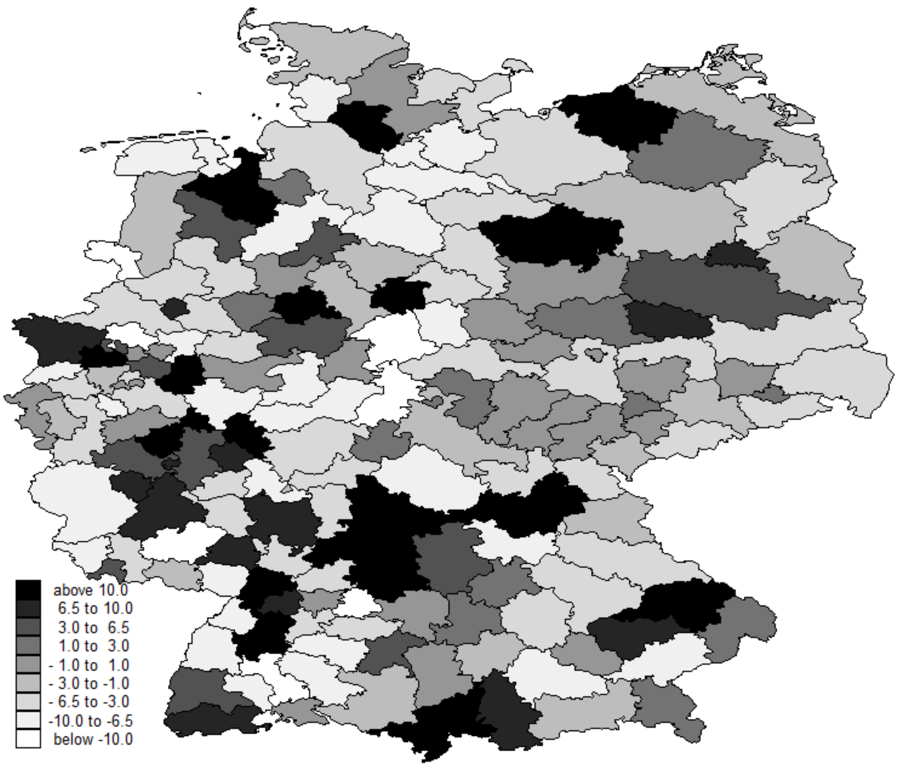
[58]This is supported by the strong robustness of our results with respect to time-varying unobserved heterogeneity, as shown in Section 4.5.4.

Figure 4.3: Geographical Distribution of Local Treatment Intensities in Germany

(a) Unconditional Treatment Intensity (Z^{2005})



(b) ($Z^{2005} - Z^{2004}$|Regional characteristics)



Note: Figure (a) shows the geographical distribution of the unconditional treatment intensity in 2005 (Z^{2005}) and Figure (b) the differences between treatment intensities in 2004 and 2005 conditional on regional characteristics ($Z^{2005} - Z^{2004}$|Regional characteristics) among Local Employment Agencies in Germany.
Source: Statistic of the German Federal Employment Agency.

relocation assistance but through not-participating in the other program. To investigate whether this is a relevant channel, we implement a placebo test in the first stage estimation to check whether the participation in relocation assistance is uniquely determined by the treatment intensity of mobility programs or other ALMP programs (see again Table 4.4). Therefore, we calculate the treatment intensity for other ALMP programs (job creation schemes, vocational training) and re-estimate the first stage using the alternative instruments. As indicated by the small and insignificant coefficients in columns (3) and (4), as well as the resulting F-statistics that are clearly below the critical value of 10, there is no evidence that the receipt of relocation assistance is correlated with the (non-)participation in other ALMP programs. This makes us confident that the explained channel does not violate the exclusion restriction. (ii) A second potential violation of the exclusion restriction would occur if there is an endogenous regional shock which affects the instrument and the labor market outcomes of participants simultaneously, e.g., firm closure on the local level. To avoid such a violation, we use the local treatment intensity with a lag of one year before the entry into unemployment and control for a large set of regional characteristics (including regional fixed effects). Furthermore, it should be noted that in Germany additional programs (based on additional funding) are usually offered to employees in cases where a large firm which has a significant impact on the local labor market closes down. As a consequence, it can be expected that the assignment of ALMP programs are not affected substantially. Finally, we can conclude that the exclusion restriction seems to be a plausible assumption here.

Monotonicity The monotonicity condition requires that the treatment probability is a (positive) monotonic function of the instrument excluding the presence of defiers. In other words, this excludes individuals who do not participate due to the higher treatment intensity. As the treatment intensity proxies the caseworkers willingness to inform job seekers about the availability of the subsidy and to accept applications, it is very unlikely that there are individuals who face a lower participation probability because of a higher treatment intensity. This is in particular true as we exclude participants in other mobility programs from the control group (see discussion in Section 4.3.2). Hence, we assume that the monotonicity assumption is fulfilled.

4.5 Estimation Results

4.5.1 Baseline Results

Panel A in Table 4.6 presents our baseline results and shows the treatment effects (δ) for the four different labor market outcomes as defined in Section 4.3.3. Besides the IV

Table 4.5: The Impact of Observed Characteristics on the IV Variation

	Unconditional Instrument	Adjusted Instrument	
	Z_j	$\hat{V}_j$	$\hat{V}_j$
	(1)	(2)	(3)
Equation 4.4			
Regional characteristics			
Local unemployment rate		0.189*	0.370***
		(0.100)	(0.116)
Local vacancy rate		0.034	0.164***
		(0.049)	(0.052)
GDP per capita in 10,000 €		-0.000	0.003
		(0.000)	(0.002)
Share of working population (ref.: agricultural sector)			
in industry sector		-0.004***	-0.010***
		(0.001)	(0.002)
in service sector		-0.004***	-0.004
		(0.001)	(0.003)
Living in East-Germany		0.078***	—
		(0.010)	
Time fixed effects		✓	✓
LEA fixed effects			✓
R^2		0.624	0.885
No. of observations		1,191	1,191
Equation 4.5			
Number of statistically significant coefficients of X_i at the 5%-level			
Socio-demographic characteristics	12	4	1
Labor market history	24	4	3
Information on current unemployment spell	7	6	1
Total	43	14	5
R^2	0.260	0.006	0.003
Number of observations	30,397	30,397	30,397

Note: OLS estimation. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. Therefore, the unit of observation in the estimation of Equation 4.4 is each LEA district where we observe treated individuals in each quarter in 2005 and 2006. This results in 1,191 LEA-quarter observations. With respect to Equation 4.5, we note that 73 variables are included in the specification in total. Full estimation results of Equation 4.5 can be found in Table 4.19 in the Appendix.

results (with and without regional fixed effects), we also present the OLS estimates. All specifications include several control variables for socio-demographic characteristics, short- and long-term labor market history and characteristics of the current unemployment spell (e.g., benefit entitlement, duration, other ALMP participation).[59] Moreover, we include regional and time-fixed effects, as well as time-varying regional-specific control variables. In addition to Table 4.6, Figure 4.4 shows the monthly employment effect within our observation window.

The OLS results suggest higher wages and more stable jobs for program participants compared to non-participants, but no significant effect on the long-term employment probability. Using the instrument to deal with unobserved terms affecting the selection into

[59]We test the robustness of our results with respect to the inclusion of potentially endogenous control variables in Section 4.5.4.

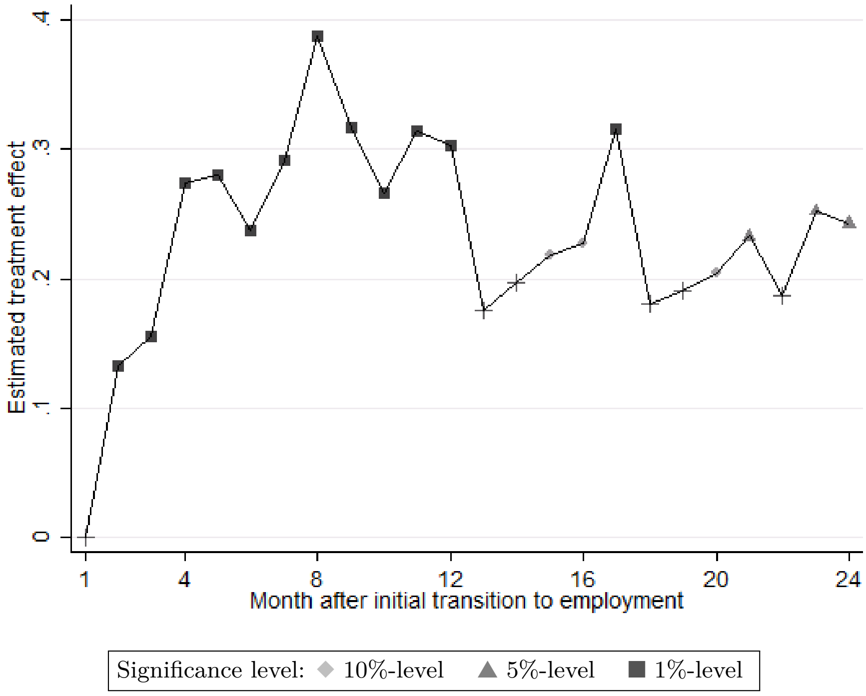
Table 4.6: Main Estimation Results

	Mean non-participants	OLS (1)	IV (2)	IV (3)
A) Baseline Results				
Log first daily wage in t_{ue+1}	65.54	0.138***	0.470***	0.245***
		(0.015)	(0.117)	(0.088)
Log average daily wage from t_{ue+1} to t_{ue+24}	67.93	0.145***	0.355***	0.163**
		(0.014)	(0.109)	(0.079)
No. of job quits from t_{ue+1} to t_{ue+24}	0.928	-0.126***	-0.875***	-1.027***
		(0.041)	(0.301)	(0.309)
Employed in t_{ue+24}	0.733	0.018	0.264**	0.243**
		(0.019)	(0.116)	(0.100)
B) Better Economic Conditions or Upward Job Mobility?				
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution				
within LEA district	0.501	0.067***	0.384***	0.224***
		(0.010)	(0.081)	(0.059)
within LEA district and sector	0.512	0.066***	0.294***	0.139**
		(0.010)	(0.078)	(0.059)
Control variables				
Socio-demographic characteristics		✓	✓	✓
Labor market history		✓	✓	✓
Regional information		✓	✓	✓
Information on current unemployment spell		✓	✓	✓
Time fixed effects		✓	✓	✓
LEA fixed effects				✓
No. of observations		30,397	30,397	30,397
F-statistic for weak identification			30.89	16.89

Note: Depicted are estimated treatment effects using OLS and 2-SLS estimation. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA level.

relocation assistance leads to even more promising effects, where the positive effect on the long-term probability also becomes statistically significant. Although the inclusion of regional fixed effects (to control for time-invariant regional unobserved heterogeneity that might affect the exogeneity of the instrument) leads to a reduction in point estimates, the treatment effects remain more positive than the OLS results. The results in column (3), which is our preferred specification, suggest that participants earn in the first month of the new job, on average, 25% more than non-participants. The wage difference declines over time (16%) but remains positive and statistically significant. Furthermore, participants experience significantly less job quits within our observation window and have a 24%-points higher employment probability 24 months after the initial transition from unemployment to the new job. In addition, Figure 4.4 shows that the positive employment effect is also quite stable over time after an initial adjusting period. In summary, the results suggest that program participation and hence the decision to move to a distant labor market improves the employment prospects of participants substantially.

Figure 4.4: Treatment Effect on Monthly Employment Probabilities



Note: Depicted are estimated treatment effects of relocation assistance on monthly employment rates for the first 24 months after the transition to regular employment and the corresponding significance levels using 2SLS estimation including LEA fixed effects. Standard errors are clustered at the LEA district level.

The differences between the OLS and IV results may be caused by two factors. First, conditional on observed characteristics it seems that participants have worse unobserved characteristics, which makes them fail to find employment locally and leave for distant labor markets. This might be explained by the selection based on the caseworker level which is unobserved in our data: As the final decision about the approval of the subsidy is up to the caseworker, who is legally constrained to check whether the job seeker could find employment locally or without the subsidy, it seems to be natural that predominately applications of low ability individuals will be approved, and thus OLS estimates are downward biased.

The second explanation is based on the fact that the 2-SLS estimator identifies the local average treatment effect (LATE) on those individuals induced to participate in the program due to a change in the instrumental variable (e.g. Angrist et al., 1996; Heckman, 1997; Heckman and Vytlacil, 2005). In our case, this is the effect on those job-seekers who chose the treatment due to the higher treatment intensity in their LEA district. Therefore, our IV results are only informative for a specific subgroup of participants and might differ from the average treatment effect on all treated individuals (ATT). However, we argue that the LATE is the policy relevant parameter here given that policy makers can directly influence the treatment intensity (instrument) and hence the number of job seekers that move due to the existence of the subsidy. We expect that the LATE identifies the treatment effect on those (low-ability) individuals who would not move in absence of the program but who are, due to the high treatment intensity, induced to change their behavior and collect the potentially large returns of the move.

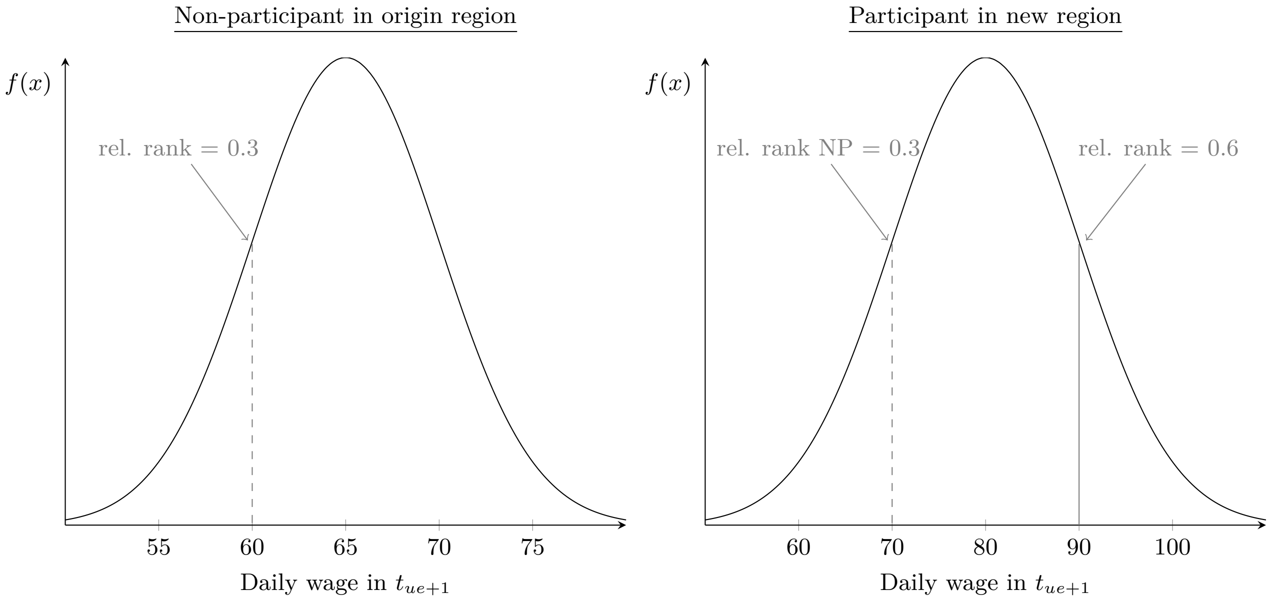
4.5.2 Economic Conditions and Job Match Quality as Underlying Mechanisms

The positive effect of the moving subsidy on the labor market performance might be explained by three different channels: (i) participants move to regions characterized by better economic conditions compared to their region of origin, (ii) the existence of the subsidy increases the search radius of job seekers, which is likely to also increase the quality of the job match, and (iii) the relocation might have a positive effect on participants' unobserved characteristics, e.g., individuals' motivation or the new social environment, etc.

While we have no information on the importance of the third aspect (as unobserved with the data at hand), we do provide evidence on the relevance of the first two channels. To do so, we take a closer look at the first wage in the new job and consider an additional outcome variable that measures the relative rank of the realized wage in t_{ue+1} of a participant (non-participant) in the new job within the overall wage distribution in the new (origin) region. The relative rank ranges between 0 and 1 and is independent of the absolute value of the wage. Figure 4.5 illustrates the underlying idea. Let us assume that an average non-participant in the origin region realizes a nominal daily wage in t_{ue+1} of 60 € in the new job, which corresponds to a relative rank of 0.3 within the regional wage distribution (left side of Figure 4.5). Now, consider the case of participants. Given that the new region is characterized by better economic conditions, we would expect a wage distribution that is shifted towards the right so that the same relative rank of 0.3 in the new region corresponds to a higher nominal wage (as illustrated by 70 € on the right side of Figure 4.5). However, if upward job mobility additionally drives the wage effect, we would expect to find a higher relative rank for participants compared to non-participants (who represent the counterfactual situation). In our example, the difference in the nominal wage between 60 € and 70 € arises due to better economic conditions in the new region (shift in wage distribution), while the increase from 70 € to 90 € is due to upward job mobility (increase in job match quality).

Based on this concept, a zero effect in terms of the relative rank would indicate that the positive wage effect in t_{ue+1} of 25% (as shown in Panel A in Table 4.6) is just a

Figure 4.5: Relative Rank in Wage Distribution



Note: The figure illustrates the construction and the underlying idea of an additional outcome variable that measures the relative rank of the realized wage in t_{ue+1} of a participant (non-participant) in the new job within the overall wage distribution in the new (origin) region. Assume that an average non-participant in the origin region realizes a nominal daily wage in t_{ue+1} of 60 € in the new job, which corresponds to a relative rank of 0.3 within the regional wage distribution (left side of Figure 4.5). The numbers are artificially chosen and are not based on actual observations. The right panel shows the case of participants. Given that the new region is characterized by better economic conditions, we would expect a wage distribution that is shifted towards the right so that the same relative rank of 0.3 in the new region corresponds to a higher nominal wage (as illustrated by 70 €). However, if upward job mobility additionally drives the wage effect, we would expect to find a higher relative rank for participants compared to non-participants (who represent the counterfactual situation). In our example, the difference in the nominal wage between 60 € and 70 € arises due to better economic conditions in the new region (shift in wage distribution), while the increase from 70 € to 90 € is due to upward job mobility (increase in job match quality). Therefore, a comparison of participants and non-participants in terms of their relative rank provides evidence on the mechanisms behind the wage effects.

consequence of the better economic conditions in the new region. In contrast, a positive effect with respect to the relative rank would suggest that, beside the better economic conditions, upward job mobility is a driving factor of the positive wage effect in t_{ue+1}. Moreover, as the industrial composition might differ across regions, we also consider the relative rank within the wage distribution separated by region and sector as an additional outcome variable.

Panel B in Table 4.6 shows positive and statistically significant effects with respect the relative rank within the regional wage distribution. Based on the IV estimation including regional fixed effects (column 3), participants' wages in t_{ue+1} are located about 22 percentage points closer towards the right of the regional wage distribution compared to non-participants' wages in t_{ue+1}. This clearly supports the hypothesis that the positive wage effect in t_{ue+1} of 25% (as shown in Panel A in Table 4.6) is driven by both the better economic conditions in the new region and upward job mobility (better job matches) of participants. Although additionally conditioning on the industry sector slightly reduces the effect, it is still positive and statistically significant.

This evidence might also contribute to the question of whether people who move (and make use of the subsidy) are really better off in terms of real wages than those who stay in the region of origin. Although higher regional wage levels are associated with higher price levels (see Roos, 2006), the finding that participants move upwards in the wage distribution might indicate that the higher nominal wage is not only a manifestation of differences in regional price levels between the region of origin and the new working location.[60]

4.5.3 Heterogeneous Treatment Effects

In order to improve the efficiency of the subsidy allocation, an interesting question relates to the consideration of heterogeneous treatment effects among different subgroups. This might be relevant for two reasons. First of all, it can be assumed that the relocation generates additional monetary and non-monetary costs which are not covered by the subsidy, e.g., school change for children, selling personal property, job change of partner, finding a new apartment/house, leaving social networks behind.[61] Therefore, given that the additional costs are likely to vary with respect to household size, we might expect to find larger effects for households where children or a partner are present. Secondly, given that the subsidy is the same for all recipients, the incentives to move are larger for those who expect higher returns, while the subsidy is more likely to be necessary to make the relocation cost-effective for those job seekers who expect low returns of the relocation. Therefore, we consider the local unemployment rate and the previous wage level as proxies for a job seeker's potential returns to the relocation. The idea is that people living in regions with relatively poor economic conditions, as well as those with high abilities (which is indicated by a high wage in the previous job), have the largest potential for wage increases when they move to a different local labor market.

In order to test for the presence of heterogeneous effects, we re-estimate our baseline model for different subgroups based on certain characteristics indicating individuals' family obligations as well as the local unemployment rate and previous wages. Moreover, we also consider a measure which directly relates to the individual preferences for geographical mobility. In order to create this measure we exploit a different data source, the *German*

[60]However, without further assumptions concerning regional price levels or individual consumption data (to calculate real wages), we cannot unambiguously conclude that participants are also better off in terms of real wages than those who stay.

[61]In line with this, Brauninger and Tolciu (2011) argue that individual's mobility depend on their social environment, and it is likely that economic incentives (disparities in unemployment rates or wages) and policies (subsidy) might be insufficiently strong enough to affect an individual's decision to move.

Table 4.7: Instrumental Variable Estimation: Heterogenous Treatment Effects

	Married or cohabiting		One child or more		Local UE rate		Last wage ≥ median		Moving prob ≥ median	
	No	Yes	No	Yes	Low	High	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A) Baseline Results										
Log first daily wage in t_{ue+1}	0.249***	0.181*	0.215**	0.266***	0.262***	0.223**	0.502***	0.174*	0.172**	0.285***
	(0.095)	(0.094)	(0.097)	(0.075)	(0.083)	(0.096)	(0.197)	(0.089)	(0.089)	(0.076)
Log average daily wage from t_{ue+1} to t_{ue+24}	0.186**	0.168*	0.150	0.268***	0.262***	0.161**	0.473***	0.091	0.208***	0.164**
	(0.088)	(0.089)	(0.098)	(0.079)	(0.077)	(0.080)	(0.161)	(0.075)	(0.070)	(0.073)
No. of job quits from t_{ue+1} to t_{ue+24}	-0.852***	-0.553	-0.656**	-0.508	-0.820***	-0.767*	-0.522	-0.434	-1.126***	-0.107
	(0.293)	(0.394)	(0.324)	(0.330)	(0.292)	(0.429)	(0.452)	(0.307)	(0.358)	(0.306)
Employed in t_{ue+24}	-0.011	0.199*	0.047	0.167	0.149	0.406***	0.091	0.118	0.142	0.153
	(0.141)	(0.117)	(0.140)	(0.114)	(0.117)	(0.139)	(0.193)	(0.105)	(0.119)	(0.122)
B) Better Economic Conditions or Upward Job Mobility?										
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution										
within LEA district	0.229***	0.165**	0.214***	0.201***	0.187***	0.167**	0.476***	0.164**	0.095*	0.218***
	(0.072)	(0.078)	(0.069)	(0.068)	(0.054)	(0.071)	(0.146)	(0.068)	(0.056)	(0.059)
within LEA district and sector	0.143*	0.100	0.122*	0.220***	0.133**	0.097	0.368***	0.058	0.049	0.115*
	(0.080)	(0.076)	(0.068)	(0.068)	(0.060)	(0.082)	(0.141)	(0.061)	(0.074)	(0.066)
Control variables										
Socio-demographic characteristics	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Labor market history	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Regional information	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Information on current unemployment spell	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LEA fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
No. of observations	10,838	13,768	16,279	8,457	14,712	12,887	11,530	13,426	12,829	13,857
No. of participants	276	262	357	181	283	255	244	294	268	270
F-statistics for weak identification	10.10	6.83	10.95	4.95	9.01	3.99	6.01	11.35	3.75	9.98

Note: Depicted are 2-SLS estimation results for different subgroups with and without LEA fixed effects. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. Low (High) UE rate characterizes LEA districts with an unemployment rate below (above) the median in the corresponding federal state. Local UE rate: Low (High) UE rate characterizes LEA districts with an unemployment rate below (above) the median in the corresponding federal state. Last wage: Wage earned from last employment spell before entering unemployment. Moving prob: Predicted moving probability based on observable characteristics. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA level.

Socio-Economic Panel (SOEP), and run a parametric estimation on individuals general willingness to move due to family or occupation related reasons using the 1999 and 2009 wave. The model specification includes main demographic and regional indicators such as age, household context, education, local unemployment rate etc. which are observed in both the SOEP and our estimation sample.[62] Using the SOEP, instead of our estimation sample, has basically two major advantages: (i) in the SOEP data we can observe the willingness to move, which is expected to be a better proxy for the individual preferences compared to the actual moving decision and (ii) as we use a sample of the full population, instead of only considering unemployed workers, the prediction of our model is less likely affected by the presence of the treatment which would be a potential source of endogeneity bias.

The estimation results for the different subgroups are presented in Table 4.7. As indicated by the low F-statistics, for some subgroups, e.g., job seekers with strong family obligations, those living in regions with high unemployment rates or facing a low moving probability, the instrument has only a limited influence on the individual participation decision. Therefore, the results have to be interpreted with some caution (see, e.g., Bound et al., 1995, who show that 2-SLS estimates are biased towards OLS when the relationship between the instrument and the endogenous variable is weak).

Nevertheless, we find a positive effect on first wages and on the relative rank of the first wage within the LEA district, similar to the baseline estimates for all subgroups.[63] We interpret the absence of a clear pattern with respect to the non-monetary moving cost and the expected returns of the relocation, as more evidence that the moving subsidy is a suitable instrument in order to improve the labor market performance of unemployed job seekers. It should be noted that the results in column 7 and 8 indicate explicitly stronger relative wage effects for those who have been in the lower part of the wage distribution before entering unemployment. Moreover, column 9 confirms the effectiveness of the relocation assistance also for individuals who are, based on their observable characteristics, initially less likely to relocate.

4.5.4 Robustness Analysis

Although we provide supportive evidence that the instrument conditional on regional characteristics and fixed effects can be considered as good as randomly distributed across

[62] The SOEP is a population representative longitudinal study of about 11,000 households and 30,000 individuals in Germany (see Wagner et al., 2007). The full specification and estimation results are presented in Table 4.18 in Appendix 4.7.2.

[63] Since the first wage directly after the transition to employment is already known by the job seeker when making the relocation decision, we expect to see a particularly heterogeneous effect here. For the sake of completeness, we present heterogeneous treatment effects for the full set of outcome variables.

regions in Section 4.4.3, one might still be concerned that there exist time-varying unobserved regional differences that affect the allocation of relocation assistance among LEA districts. As this would be a threat to the validity of our estimation strategy, we additionally run extensive robustness checks that address this issue. In addition, we also test the sensitivity of our findings with respect to several other potential sources of bias. In the following, we briefly discuss our findings, while the corresponding estimation results can be found in Appendix 4.7.2.

Time-varying unobserved heterogeneity: In order to test the sensitivity with respect to unobserved time-varying regional heterogeneity, we adopt three different approaches (see Table 4.12). First, in addition to the district's own labor market conditions, we include the local labor market conditions of all neighboring districts in the regressions. The idea is that neighboring districts can be reached mostly within a commuting distance and hence, the prevailing conditions might influence the LEAs beliefs about the local capacity (compared to distant labor markets) to absorb unemployed job seekers. This might affect the LEAs preferences for mobility programs. Comparing column 1 (main results) and column 2 (including controls for neighboring districts) indicates the robustness of our results. Second, we apply an alternative instrument by using the lagged absolute number of entries in mobility programs, i.e., the numerator of Equation 4.1, in order to test whether results are robust with respect to time-varying unobserved factors which potentially affect the number of unemployed but not immediately the number of treated individuals. The results based on this alternative IV (column 3) do hardly differ to the main results (column 1).

Finally, we apply another alternative instrument by exploiting a specific institutional setting of the German UI system. As explained in Section 4.2.1, in total six different types of mobility programs exist (relocation assistance is one of them) and the LEA assigns a joint budget to all six programs. Although all six programs are categorized as mobility programs due to administrative reasons, two of them are basically unrelated to geographical mobility. This concerns the equipment and transition assistance. The first provides financial support for working clothes and other working equipment, while the second provides an interest-free loan to bridge the time until the first wage payment of a new job. While we use entries into all six programs to construct our IV in the main estimation (see Equation 4.1), we now only use entries into those two programs to construct the alternative instrument. Given that the two programs are not directly related to the geographical mobility of the unemployed workforce, using this alternative instrument reduces the potential influence of time-varying unobserved regional heterogeneity. However, the alternative IV can be expected to remain relevant for the individuals' participation in relocation assistance as entries into all six programs are positively correlated due to one joint budget. Again, results hardly change (compare column 1 and 4 in Table 4.12 in Appendix 4.7.2). To sum up, it can be concluded that once we control for the detailed local market conditions and include regional fixed effects the instrument seem to create exogenous variation with respect to the participation decision.[64]

Other potential sources of bias: We test the sensitivity of our results with respect to several different other potential sources of bias. First, we adopt an alternative definition of employment, as there might be concerns that our estimates are rather a consequence of participants' higher working hours than increasing hourly wages. Therefore, we re-estimate our baseline results using a more restrictive employment definition. However, excluding those job seekers who find only part-time employment has nearly no impact on our estimation results (see Table 4.13 in Appendix 4.7.2).[65] Second, we estimate a specification using a different set of control variables. While socio-demographic characteristics and information on labor market history are measured at entry into unemployment and hence, can be reliably assumed to being unrelated to the treatment, information on the current unemployment spell might be endogenous. For instance, the unemployment duration could be affected by the availability of the subsidy. Therefore, we run a specification test and find that the exclusion of information on the current unemployment spell does not significantly change the results (see Table 4.14 in Appendix 4.7.2). Third, given the relatively skewed distribution of the instrumental variable, we also exclude LEA districts with the 5% highest/lowest treatment intensities. As shown in Table 4.15 in Appendix 4.7.2, this has nearly no impact on the estimation results. Fourth, we use a randomly reduced sample of non-participants (see Table 4.16), as 2-SLS estimates might be affected by a low ratio of treated and non-treated observations (see Chiburis et al., 2012). Again, the results are highly robust when restricting the control group to 20,000 (column 2), 10,000 (column 3), 5,000 (column 4), respectively 2,000 observations (column 5). Although the point estimates vary a bit across the different samples, the differences are not significant, i.e., the conclusions would remain unchanged. Finally, we extended the sample by additionally including entries into unemployment in 2007 and 2008 (see Table 4.17 in the Supplementary appendix). Please note that this extension restricts the analysis to the short-run outcomes only. The effects on the first wage as well as the relative

[64]It should be noted that the high robustness of our results might be also explained by using a lagged instrument, i.e., any time-varying unobserved factor would only be a threat to our identification strategy if it would affect the individual labor market outcomes with a lag of one year.

[65]For the main analysis, we refrain from using a more restrictive definition where we condition on full-time employment only, as it would reduce the external validity of the results because it is not required by the institutional settings of the program. Moreover, it should be noted that 95% of the male individuals work full-time and daily wages are calculated based on actual workdays and not based on monthly income. Hence, we can also rule out that systematic differences between participants and non-participants with respect to the number of workdays influences our results.

rank within the regional wage distribution are slightly reduced but remain statistically significant leaving the main conclusions unchanged.

4.5.5 Discussion of Economic Implications

As implied by our baseline results, the moving subsidy has a positive effect of about 25% on the first daily wage after the transition to regular employment. This seems to be a strong impact in relative terms; however, the absolute wage level of the target population is fairly low. For example, considering the average non-participant in our estimation sample, he could earn about 330 € more per month if he would make use of the subsidy and move to a distant region. Moreover, in the long run, this treatment effect will be reduced to an average of 220 € per month over a period of 2 years, which suggests that about one third of the moving bonus paid by the employer directly after the transition is just an early realization of the overall raise in nominal wages over time, and the wage difference between treated and non-treated decreases in the long-run. Furthermore, the analysis of the effect heterogeneity reveals that the relative wage effects are the largest for job seekers who are initially at the lower end of the wage distribution.

The analysis on the relative rank within a region, and respectively, within a sector, indicates that geographically mobile job seekers also experienced an upward job mobility in the new job. This is even more remarkable in the light of the relatively low program costs for the employment agency compared to other ALMP measures. For example, a participant in vocational training creates costs that are about six times larger than a subsidized mover, while vocational training is related to a strong locking-in effect during program participation (e.g. Lechner and Wunsch, 2008). Moreover, positive effects on participants' labor market outcomes are modest and can be found only in the very long run (e.g. Fitzenberger et al., 2008; Lechner et al., 2011). In contrast to this, the relocation assistance implies only small, if any, locking-in effect in unemployment, and strong positive effects on labor market outcomes are realized in the short-run already.

4.6 Conclusion

We use German administrative data on entries into unemployment in 2005 and 2006 to evaluate the effectiveness of relocation assistance on labor market outcomes. The relocation assistance is part of the German ALMP system and provides unemployed job seekers a subsidy to move to distant labor markets in order to find employment. The main aim of this program is to encourage geographical mobility among the unemployed, expecting an overall reduction in unemployment rates by shifting excess labor supply from depressed to prosperous areas.

The decision to participate in relocation assistance and hence to move to a distant region is likely to be correlated with usually unobserved factors, such as personality and motivation by the unemployed or considerations by the caseworker. Therefore, we use an IV strategy to identify causal treatment effects. Using the lagged local treatment intensity of mobility assistance programs as a proxy for the local employment agencies' preferences for these programs, the IV estimation results show that receiving the relocation assistance and hence moving to a distant labor market leads to significantly higher wages and more stable jobs in the future compared to non-participants. Several robustness checks with respect to the identification strategy, in particular with respect to impact of time-varying unobserved heterogeneity, confirm the high validity of the results.

Descriptive evidence shows that participants move predominately to regions with better economic conditions. In the causal analysis, we find positive wage and employment effects. In fact, participants have 25% higher wages in the new job compared to non-participants, a higher job stability over time and also higher employment probabilities in the long-run. While the wage effect appears fairly large at the first glance, one has to take into account that the underlying nominal wage level is rather low, corresponding to an absolute income effect of 330 Euro/month (which is decreasing to 220 Euro/month over time). Moreover, we provide evidence that the positive wage effect is not only a manifestation of the change in the economic conditions. Considering the relative rank of the realized wage of a participant (non-participant) in the new job within the overall wage distribution in the new (origin) region, we find that participants move up the wage distribution, within their new economic environment. Therefore, we conclude that the availability of the subsidy encourages job seekers to search for new jobs nationwide, which raises the number of obtainable vacancies and increases the quality of the job match. In summary, our results imply strong positive consequences on participants labor market performance in terms of nominal wages, upward job mobility and employment prospects. These results are even more remarkable in the light of the relatively low program costs compared to other ALMP measures, like vocational training, with less positive effects.

The analysis of the effect heterogeneity shows that the treatment seems to be also beneficial for individuals who are generally less likely to move but are apparently stimulated by the treatment. It further reveals that our main findings also hold among subgroups with different levels of non-monetary moving costs (which are not captured by the subsidy) and different levels of expected returns. However, even if household and regional characteristics only play a minor role in the effectiveness of the relocation in terms of subsequent income, these characteristics are important for the selection into the program.

Additional costs which are not covered by the subsidy, e.g., school change for children, selling personal property, job change of partner, finding a new apartment/house, and leaving social networks behind could prevent job seekers from utilizing the relocation more effectively and lower the influence of the LEA's policy mix on the search behavior of job seekers with children or a partner. Therefore, if policy makers want to increase the program's currently low take-up and hence further improve geographical mobility among the unemployed, especially among those with strong family obligations, one possible channel – in addition to simply raising the local treatment intensity – would be to increase the subsidy payment beyond the pure transportation costs (particularly given that the current program costs are relatively low compared to other ALMP programs). To increase the efficiency of such a policy, one might vary the amount of the extra payment in addition to the pure transportation costs based on individual/household characteristics, such as marital status or the presence of children. This extra payment would further reduce the job seeker's reservation wage for distant jobs and therefore increase their willingness to move to distant regions even for lower wages. This would help to fill available vacancies in prosperous areas by unemployed job seekers from deprived areas to a larger extent. Such an improvement in the aggregate matching function will reduce unemployment on a national level.

Finally, in light of our findings, this chapter brings about several important further questions. Unfortunately, because we neither observe the timing of the program announcement (i.e., when the job seeker receives knowledge about the program) nor the actual job search behavior during the unemployment spell, we are prevented from analyzing potential deadweight effects stemming from the fact that some participants might have moved even in the absence of the program. Moreover, with the data at hand, we can only focus on the impact of relocation assistance on post-transition labor market outcomes. Therefore, future research should analyze the impact of the existence of relocation assistance on individuals' job search behavior (reservation wages, intensity etc.) and unemployment duration, as well as general equilibrium effects with respect to the program's possible shifting of excess labor supply from depressed to more prosperous areas. These effects will become even more important when policy makers aim to increase the take-up rates of mobility programs.

4.7 Appendix

4.7.1 Construction of the Estimation Sample

This section provides details and robustness checks with respect to the construction of the estimation sample.

Sample Restrictions: Based on the sample of entries into unemployment between 2005 and 2006 who start new employment within 24 months (N=42,775, see Table 4.2), we need to identify treated individuals, i.e., those who received relocation assistance related to the selected transition to employment. Due to data restrictions, we only observe the exact date at which an individual received the subsidy, but we do not have an identifier available that would allow us to unambiguously merge the subsidy payment to a transition to employment. Therefore, we have to make the following assumptions in order to define the treatment group: (i) The subsidy payment has to take place within a time window of six months before and after the transition to employment, and (ii) as the subsidy payment requires the take up of a distant job, we only keep treated individuals with a change in their residential location, i.e., the place where they initially registered as unemployed must differ from the working location of the new job. Concerning condition (i), we tested different time windows, and six months appeared to be the most appropriate in terms of a trade-off between bias reduction and size of the treatment group. Panel A of Table 4.8 shows the results for all individuals receiving the subsidy payment within a time window of +/- 3 months around the transition to employment, while Panel B of Table 4.8 shows results for a subsample including only individuals who receive the payment within the first six months after the transition. It can be seen that the estimation results for alternative time windows are very robust compared to our main results reported in Table 4.6; hence, they would lead to the same conclusions.

Due to the two assumptions, in total, 54 individuals were excluded from the group of participants. The control group contains all individuals with a transition to employment but without a receipt of relocation assistance, excluding individuals who participate in other mobility programs (N=7,250 corresponding to 20% of non-participants). The latter is due to content and methodological reasons. From a content-related view, we exclude participants in other mobility programs in order to estimate a clear effect of participating in relocation assistance, thus avoiding distorting effects due to participants in similar programs in the control group. From a methodological view, the exclusion of participants in other mobility programs from the control group is required to avoid any influence arising from the similarity of the program under scrutiny and to increase the validity of our instrument to identify causal treatment effects. As described in the Section 4.4.1, our

Table 4.8: Sensitivity Analysis: Alternative Treatment Windows

	Mean non-participants	OLS (1)	IV (2)	IV (3)
A. Treatment window +/- 3 Months				
Log first daily wage in t_{ue+1}	65.33	0.150***	0.466***	0.286***
		(0.017)	(0.116)	(0.091)
Log average daily wage from t_{ue+1} to t_{ue+24}	67.71	0.152***	0.352***	0.199***
		(0.015)	(0.108)	(0.080)
No. of job quits from t_{ue+1} to t_{ue+24}	0.933	-0.117***	-0.788***	-1.043***
		(0.043)	(0.298)	(0.312)
Employed in t_{ue+24}	0.733	0.004	0.255**	0.202**
		(0.021)	(0.107)	(0.096)
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution				
within LEA district	0.501	0.071***	0.387***	0.261***
		(0.011)	(0.079)	(0.059)
within LEA district and sector	0.512	0.070***	0.304***	0.190***
		(0.011)	(0.078)	(0.059)
No. of observations		28,510	28,510	28,510
F-statistic for weak identification			26.87	14.87
LEA fixed effects				✓
B. Treatment window +6 Months				
Log first daily wage in t_{ue+1}	65.37	0.137***	0.633***	0.329***
		(0.016)	(0.154)	(0.105)
Log average daily wage from t_{ue+1} to t_{ue+24}	67.76	0.147***	0.544***	0.276***
		(0.015)	(0.136)	(0.087)
No. of job quits from t_{ue+1} to t_{ue+24}	0.933	-0.134***	-1.361***	-1.327***
		(0.042)	(0.404)	(0.367)
Employed in t_{ue+24}	0.733	0.019	0.380***	0.328***
		(0.018)	(0.151)	(0.114)
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution				
within LEA district	0.501	0.065***	0.519***	0.290***
		(0.011)	(0.111)	(0.071)
within LEA district and sector	0.51	0.067***	0.434***	0.215***
		(0.010)	(0.112)	(0.070)
No. of observations		28,993	28,993	28,993
F-statistic for weak identification			19.86	13.05
LEA fixed effects				✓

Note: Depicted are causal treatment effects using OLS and 2-SLS estimation. All estimations include several control variables for socio-demographic characteristics, short- and long-term labor market history, benefit entitlement, local macroeconomic conditions at entry into unemployment and the initial unemployment duration, and time-fixed effects. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA level.

Table 4.9: Sensitivity Analysis: Correlation Between the Share Excluded from the Control Group and the Instrumental Variable

	Share of excluded control participants in other mobility programs (1)	individuals due to exclusion of individuals who do not find employment within 24 months (2)
Difference in local treatment intensity (2005-2004)	-0.0021	0.0004
	(0.0015)	(0.0015)
Regional information R	✓	✓
No. of observations	147	147
R^2	0.4464	0.6246

Note: OLS estimation. Dependent variable: Share of individuals in each LEA district that is excluded from the estimation sample (control group) due to sample restriction. */**/*** indicates significance at the 10%/5%/1%-level.

instrument drives the participation in relocation assistance and the other mobility programs simultaneously. Therefore, including recipients of other types of mobility programs in the control group would reduce the power of the instrument to disentangle treated and non-treated individuals and hence violate the monotonicity assumption. Moreover, it would harm the exogeneity assumption if the instrument affects the labor market outcome of non-participants who participated in other mobility programs. Finally, we exclude all individuals with missing information in one of the relevant variables (N=198) as well as all non-participants living in LEA districts without any participants in our estimation sample (N=4,876), which corresponds to 31 out of the 178 LEA districts. The latter is necessary because we include LEA fixed effects in our estimation procedure.

Restricting the estimation sample is likely to induce a bias in the IV estimation if the number of excluded individuals in each LEA district (due to a particular restriction) is systematically correlated with the instrumental variable. In our case, one might particularly suspect (i) the exclusion of participants in other mobility programs from the control group (N=7,250) or (ii) the exclusion of individuals who find employment later than 24 months after entry into unemployment (N=17,395) to induce such a bias. Therefore, in order test whether these two restrictions are systematically correlated with the instrumental variable, we regress the share of excluded individuals due to one of the two restrictions in each LEA district on the instrumental variation that we use in our preferred specification, i.e., controlling for regional characteristics and including LEA fixed effects. Table 4.9 shows the results. For both restrictions, we do not find a significant correlation with the instrument.

Extended Estimation Sample: Table 4.10 shows descriptive statistics comparing the baseline estimation sample and an extended sample additionally including individuals who do not find employment within 24 months. We find no systematic differences. In addition, Table 4.11 shows the corresponding estimation results for both samples for a set of outcome variables which is available for all individuals. The results indicate that including a large number of non-participants with weak labor market outcomes (longer unemployment duration) increases the treatment effects but lowers the precision of the estimates.

Table 4.10: Selected Descriptive Statistics: Baseline vs. Extended Sample

	Baseline estimation sample			Extended estimation sample		
	Participants	Non-participants	p-value	Participants	Non-participants	p-value
No. of observations	538	29,859		562	43,242	
Outcome variables						
Employed						
in t_{24}	0.81	0.81	0.99	0.78	0.61	0.00
in t_{48}	0.75	0.73	0.23	0.75	0.59	0.00
Months in employment						
from t_0 to t_{24}	15.26	15.89	0.02	14.64	11.57	0.00
from t_0 to t_{48}	33.68	33.90	0.67	32.90	25.75	0.00
Total income in 1,000 €						
from t_0 to t_{24}	37.93	31.15	0.00	36.36	22.53	0.00
from t_0 to t_{48}	87.19	68.47	0.00	84.92	51.52	0.00
Regional information						
Local unemployment rate	0.14	0.13	0.00	0.14	0.13	0.00
Local vacancy rate	0.05	0.06	0.00	0.05	0.06	0.00
Living in East-Germany	0.49	0.31	0.00	0.48	0.29	0.00
Working population in industry sector	0.20	0.22	0.00	0.20	0.22	0.00
GDP per capita in 1,000 €	24.77	25.95	0.01	24.96	26.79	0.00
Socio-demographic characteristics						
Age in years	37.71	38.54	0.02	37.78	38.73	0.01
(Spec.) Upper sec. degree	0.39	0.14	0.00	0.39	0.17	0.00
University degree	0.28	0.07	0.00	0.28	0.09	0.00
Children	0.33	0.40	0.00	0.33	0.38	0.02
Married	0.49	0.58	0.00	0.49	0.56	0.00
Migration background	0.25	0.22	0.10	0.25	0.23	0.12
Labor market history						
Last daily income in €	74.33	67.20	0.00	74.31	68.06	0.00
Occupational group of previous job						
Manufacturing	0.36	0.57	0.00	0.36	0.53	0.00
Technical occupation	0.09	0.04	0.00	0.10	0.04	0.00
Services	0.52	0.33	0.00	0.52	0.38	0.00
Days unemployed in last 10 years	360	480	0.00	362	488	0.00
Total income in last 10 years in 1,000 €	167	187	0.02	167	192	0.01
Daily amount of UI benefits in €	22.54	19.09	0.00	22.35	18.71	0.00
Jobs involve commuting in last 5 years	0.45	0.29	0.00	0.45	0.30	0.00
Previous participation in mobility program	0.25	0.06	0.00	0.25	0.05	0.00

Note: All numbers are percentages unless otherwise indicated. Regional information, socio-demographic characteristics and labor market histories are measured at entry into unemployment (t_0). P-values are based on a two-tailed t-test on equal means between participants and non-participants.

Table 4.11: Estimation Results: Baseline vs. Extended Sample

	Baseline sample				Extended sample			
	Mean non-participants	OLS (1)	IV (2)	IV (3)	Mean non-participants	OLS (4)	IV (5)	IV (6)
Employed								
in t_{24}	0.814	-0.006	0.368***	0.233**	0.611	0.175***	0.427**	0.254
		(0.017)	(0.118)	(0.108)		(0.020)	(0.203)	(0.155)
in t_{48}	0.731	0.011	0.414***	0.241**	0.591	0.157***	0.327	0.252
		(0.019)	(0.106)	(0.099)		(0.020)	(0.249)	(0.175)
Months in employment								
from t_0 to t_{24}	15.90	-0.797***	5.996***	4.229**	11.57	3.169	8.179*	7.857**
		(0.249)	(1.970)	(1.889)		(0.338)	(4.464)	(3.329)
from t_0 to t_{48}	33.90	-0.746	15.635***	10.725***	25.75	7.164	17.928**	15.490**
		(0.487)	(3.630)	(3.289)		(0.672)	(9.018)	(6.268)
Total income in 1,000 €								
from t_0 to t_{24}	31.15	2.02***	23.24***	11.82*	22.53	11.67***	37.48***	29.50***
		(0.64)	(7.10)	(6.69)		(0.80)	(12.73)	(9.92)
from t_0 to t_{48}	68.47	7.17***	51.25***	24.90**	51.52	27.44***	79.56***	58.42***
		(1.34)	(14.06)	(12.77)		(1.69)	(26.24)	(18.86)
Number of observations		30,397	30,397	30,397		43,804	43,804	43,804
F-statistic for weak identification			30.62	16.89			28.34	10.62
Time fixed effects		✓	✓	✓		✓	✓	✓
LEA fixed effects				✓				✓

Note: Depicted are estimated treatment effects separated for the baseline estimation sample, comprising all individuals who find a new employment within 24 months, and the extended estimation sample without this restriction. All estimations include several control variables for socio-demographic characteristics, short- and long-term labor market history, benefit entitlement, local macroeconomic conditions at entry into unemployment and the initial unemployment duration, and time-fixed effects. ***/**/* indicate statistically significance at the 1%/5%/10%-level. Standard errors are in parentheses and the clustered at the LEA level.

4.7.2 Robustness Analysis

This section provides results of several robustness tests with respect to our baseline estimation results, including time-varying unobserved heterogeneity, an alternative employment definition, the set of control variables, outliers in the treatment intensity (instrument) and the size of the control group. The results presented in Table 4.12 - 4.17 are discussed in detail in Section 4.5.4.

Table 4.12 shows the results of different robustness checks with respect to time-varying unobserved heterogeneity (see Section 4.5.4 for explanation).

Table 4.13 shows the estimation results for an alternative employment definition where we explicitly exclude all job seekers who find part-time employment.

Table 4.14 shows the results of a specification tests excluding potentially endogenous control variables referring to information on the current unemployment spell.

Table 4.15 shows estimation results when excluding LEA districts with the 5% highest/lowest absolute treatment intensity (column 2) and the 5% with the highest/lowest difference in treatment intensities between 2004 and 2005.

Table 4.16 shows estimate results for a randomly reduced control group consisting of 20,000 (column 2), 10,000 (column 3), 5,000 (column 4) and 2,000 (column 5) control individuals.

Table 4.17 shows the estimation results for an extended sample additionally including entries into unemployment in 2007 and 2008. Please note that extending the sample to 2007 and 2008 restricts the analysis to the short-run outcomes only. The reduced return to geographical mobility in the extended sample (compared to the main sample) might be explained by the occurrence of the global financial crisis (as started in 2007) and the resulting reduction in job opportunities.

Table 4.18 shows estimation results of the individual willingness to move using the 1999 and 2009 wave of the German Socio-Economic Panel (SOEP).

Table 4.12: Sensitivity Analysis: Time-varying Unobserved Factors

	Mean non-participants	Main results (1)	Pull factors (2)	Alternative IV Type I (3)	Alternative IV Type II (4)
A) Baseline Results					
Log first daily wage in t_{ue+1}	65.54	0.245***	0.261***	0.253***	0.265***
		(0.088)	(0.088)	(0.092)	(0.090)
Log average daily wage from t_{ue+1} to t_{ue+24}	67.93	0.163**	0.166**	0.161**	0.175**
		(0.079)	(0.078)	(0.081)	(0.080)
No. of job quits from t_{ue+1} to t_{ue+24}	0.928	-1.027***	-0.997***	-1.030***	-1.062***
		(0.309)	(0.314)	(0.318)	(0.316)
Employed in t_{ue+24}	0.733	0.243**	0.244**	0.263**	0.249**
		(0.100)	(0.103)	(0.104)	(0.102)
B) Better Economic Conditions or Upward Job Mobility?					
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution					
within LEA district	0.501	0.224***	0.229***	0.223***	0.234***
		(0.059)	(0.058)	(0.062)	(0.061)
within LEA district and sector	0.512	0.139**	0.142***	0.148**	0.146**
		(0.059)	(0.057)	(0.063)	(0.061)
Control variables					
Socio-demographic characteristics		✓	✓	✓	✓
Labor market history		✓	✓	✓	✓
Regional information		✓	✓	✓	✓
Information on current unemployment spell		✓	✓	✓	✓
Regional information in neighboring districts			✓		
Time fixed effects		✓	✓	✓	✓
LEA fixed effects		✓	✓	✓	✓
No. of observations		30,397	30,397	30,397	30,397
F-statistic for weak identification		16.89	18.00	13.55	7.75

Note: Depicted are estimated treatment effects using 2-SLS estimation. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA level.
Pull-factors: Estimation includes labor market conditions of neighboring districts to control for the complete local labor market, i.e., all possible destinations that could be reached within a commuting distance and hence do not necessarily require a relocation. The idea is that if local labor market conditions deteriorate, distant regions (which require a relocation/treatment) become more attractive. Therefore, by this strategy we implicitly control for time-varying pull factors of distant regions.
Alternative IV, Type I: We only use the lagged number of entries into mobility programs as an instrument, i.e., the numerator of Equation 1, to test whether results are robust with respect to time-varying unobserved factors which potentially affect the number of unemployed.
Alternative IV, Type II: We only use the lagged number of entries into equipment and transition assistance (instead of all six mobility programs) to construct the instrument (the numerator of Equation 1 changes). Given that those two programs are not directly related to the geographical mobility of the unemployed workforce, using this alternative instrument reduces the potential influence of time-varying unobserved regional heterogeneity.

Table 4.13: Sensitivity Analysis: Alternative Employment Definition

	Mean non-participants	OLS (1)	IV (2)	IV (3)
A) Baseline Results				
Log first daily wage in t_{ue+1}	66.35	0.129***	0.504***	0.262***
		(0.016)	(0.116)	(0.081)
Log average daily wage from t_{ue+1} to t_{ue+24}	68.64	0.139***	0.395***	0.192***
		(0.015)	(0.106)	(0.069)
No. of job quits from t_{ue+1} to t_{ue+24}	0.926	-0.127***	-0.854***	-0.936***
		(0.038)	(0.316)	(0.296)
Employed in t_{ue+24}	0.710	0.019	0.359***	0.259**
		(0.020)	(0.126)	(0.102)
B) Better Economic Conditions or Upward Job Mobility?				
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution				
within LEA district	0.501	0.061***	0.375***	0.209***
		(0.011)	(0.078)	(0.056)
within LEA district and sector	0.513	0.059***	0.266***	0.115**
		(0.011)	(0.074)	(0.056)
Control variables				
Socio-demographic characteristics		✓	✓	✓
Labor market history		✓	✓	✓
Regional information		✓	✓	✓
Information on current unemployment spell		✓	✓	✓
Time fixed effects		✓	✓	✓
LEA fixed effects				✓
No. of observations		28,773	28,773	28,773
F-statistic for weak identification			26.72	14.88

Note: Depicted are causal treatment effects using OLS and 2-SLS estimation. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parentheses and clustered at the LEA level.

Table 4.14: Sensitivity Analysis: Addressing the Potential Endogeneity of Control Variables

	Mean non-participants	Reduced spec. (1)	Full spec. (2)
A) Baseline Results			
Log first daily wage in t_{ue+1}	65.54	0.245***	0.245***
		(0.209)	(0.088)
Log average daily wage from t_{ue+1} to t_{ue+24}	67.93	0.199**	0.163**
		(0.188)	(0.079)
No. of job quits from t_{ue+1} to t_{ue+24}	0.928	-1.041***	-1.027***
		(0.336)	(0.309)
Employed in t_{ue+24}	0.733	0.273**	0.243**
		(0.115)	(0.100)
B) Better Economic Conditions or Upward Job Mobility?			
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution			
within LEA district	0.501	0.207***	0.224***
		(0.061)	(0.059)
within LEA district and sector	0.512	0.128**	0.139**
		(0.059)	(0.059)
Control variables			
Socio-demographic characteristics		✓	✓
Short-term labor market history		✓	✓
Long-term labor market history		✓	✓
Information on current unemployment spell			✓
Regional information		✓	✓
Time fixed effects		✓	✓
LEA fixed effects		✓	✓
No. of observations		30,397	30,397
F-statistic for weak identification		15.47	16.89

Note: Depicted are estimated treatment effects using 2-SLS estimation. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parentheses and clustered at the LEA level.

Table 4.15: Sensitivity Analysis: Excluding Outliers in the Instrument

	Mean non-participants	Main results	Excluding LEA districts with 5% highest/lowest	
			Z	ΔZ
		(1)	(2)	(3)
A) Baseline Results				
Log first daily wage in t_{ue+1}	65.54	0.245***	0.232**	0.258***
		(0.088)	(0.093)	(0.098)
Log average daily wage from t_{ue+1} to t_{ue+24}	67.93	0.163**	0.123	0.144
		(0.079)	(0.082)	(0.088)
No. of job quits from t_{ue+1} to t_{ue+24}	0.928	-1.027***	-0.963***	-0.969***
		(0.309)	(0.307)	(0.329)
Employed in t_{ue+24}	0.733	0.243**	0.259**	0.242**
		(0.100)	(0.103)	(0.103)
B) Better Economic Conditions or Upward Job Mobility?				
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution				
within LEA district	0.501	0.224***	0.164***	0.214***
		(0.059)	(0.062)	(0.066)
within LEA district and sector	0.512	0.139**	0.076	0.110*
		(0.059)	(0.061)	(0.064)
Control variables				
Socio-demographic characteristics		✓	✓	✓
Labor market history		✓	✓	✓
Regional information		✓	✓	✓
Information on current unemployment spell		✓	✓	✓
Time fixed effects		✓	✓	✓
LEA fixed effects		✓	✓	✓
No. of observations		30,397	27,107	27,965
F-statistic for weak identification		16.89	9.86	12.46

Note: Depicted are estimated treatment effects using 2-SLS estimation. Results in column 2 and 3 are based on a restricted sample where LEA districts with the 5% highest/lowest treatment intensities (column 2) or the 5% highest/lowest difference in treatment intensities between 2004 and 2005 (ΔZ) (column 3) are excluded. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parentheses and clustered at the LEA level.

Table 4.16: Sensitivity Analysis: Balancing Treatment and Control Group

	Full sample	No. of non-participants: N_{NP} =			
		20,000	10,000	5,000	2,000
	(1)	(2)	(3)	(4)	(5)
First Stage Estimation					
Receiving relocation assistance	0.128***	0.187***	0.338***	0.599***	1.026***
	(0.031)	(0.045)	(0.087)	(0.158)	(0.300)
A) Baseline Results					
Log first daily wage in t_{ue+1}	0.245***	0.131*	0.139**	0.153**	0.195***
	(0.088)	(0.073)	(0.067)	(0.064)	(0.063)
Log average daily wage from t_{ue+1} to t_{ue+24}	0.163**	0.096	0.110*	0.168***	0.174***
	(0.079)	(0.071)	(0.061)	(0.054)	(0.055)
No. of job quits from t_{ue+1} to t_{ue+24}	-1.027***	-0.784***	-0.641**	-0.643***	-0.772***
	(0.309)	(0.284)	(0.250)	(0.242)	(0.232)
Employed in t_{ue+24}	0.243**	0.173**	0.285***	0.231***	0.050
	(0.100)	(0.086)	(0.083)	(0.080)	(0.098)
B) Better Economic Conditions or Upward Job Mobility?					
Relative rank of first daily wage in t_{ue+1} within the overall wage distribution					
within LEA district	0.224***	0.104**	0.084*	0.088**	0.121***
	(0.059)	(0.052)	(0.051)	(0.046)	(0.047)
within LEA district and sector	0.139**	0.051	0.011	0.026	0.097*
	(0.059)	(0.050)	(0.050)	(0.050)	(0.051)
Control variables					
Socio-demographic characteristics	✓	✓	✓	✓	✓
Labor market history	✓	✓	✓	✓	✓
Regional information	✓	✓	✓	✓	✓
Information on current unemployment spell	✓	✓	✓	✓	✓
Time fixed effects	✓	✓	✓	✓	✓
LEA fixed effects	✓	✓	✓	✓	✓
No. of observations	30,397	20,538	10,538	5,538	2,538
F-statistic for weak identification	16.89	17.15	15.08	14.31	11.68

Note: Depicted are estimated treatment effects using 2-SLS estimation.Column 2-5 depict estimation results for randomly selected subsamples of non-participants. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parentheses and clustered at the LEA level.

Table 4.17: Sensitivity Analysis: Extended Estimation Sample

	Mean non-participants	Main results (1)	Extended sample (3)
Short-run outcome variables			
Log first daily wage in t_{ue+1}	65.54	0.245***	0.197**
		(0.088)	(0.095)
Relative wage rank within the wage distribution in t_{ue+1}			
within LEA district	0.501	0.224***	0.120**
		(0.059)	(0.060)
within LEA district and sector	0.512	0.139**	0.055
		(0.059)	(0.060)
Control variables			
Socio-demographic characteristics		✓	✓
Labor market history		✓	✓
Regional information		✓	✓
Information on current unemployment spell		✓	✓
Time fixed effects		✓	✓
LEA fixed effects		✓	✓
No. of observations		30,397	48,489
F-statistic for weak identification		16.89	14.77

Note: Depicted are estimated treatment effects using 2-SLS estimation. Time fixed effects are captured by separate dummies for the calender year and quarter of entry into unemployment. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA level.
Extended sample: Entries into unemployment in 2005-2008.

Table 4.18: Marginal Effects of Logit Estimation on Moving Probability

	Willingness/intention to move	
	Coef.	SE
Age in years	-0.0135**	(0.0057)
Age in years^2 (div. by 100)	0.0001*	(0.0001)
Married	-0.0500***	(0.0131)
German	-0.0223	(0.0181)
Number of children in household		
One child	0.0100	(0.0135)
Two or more children	-0.0100	(0.0141)
Health problems	0.0072	(0.0202)
School leaving degree (Ref: Lower or middle sec. degree)		
None	-0.0047	(0.0303)
Upper sec. degree	0.0891***	(0.0130)
Vocational degree (Ref: None)		
In-firm/external training	-0.0180	(0.0126)
Technical college/university degree	-0.0032	(0.0140)
Unemployment experience in years	0.0004	(0.0028)
High skilled worker	-0.0302	(0.0376)
Occupational group (Ref: Manufacturing)		
Agriculture	-0.1001***	(0.0338)
Service	0.0500***	(0.0152)
Other	0.0071	(0.0155)
Unknown	-0.0822***	(0.0173)
Local unemployment rate	0.0090***	(0.0020)
Gross value added in region	0.0036***	(0.0008)
Living in East-Germany	-0.1428***	(0.0166)
Number of observations	8,777	
Pseudo R^2	0.047	

Source: German Socio-Economic Panel (SOEP), wave 1999 and 2009, own calculations.
Note: The dependent variable is one if individuals reported their willingness/intention to move to a distant region due to family or occupation related reasons, and zero otherwise. Standard Errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard Errors are clustered on individual-level.)

Table 4.19: The Effect of Observed Characteristics on the IV Residuals

	Unconditional Instrument		Adjusted Instrument			
	(1) Z_j		(2) $\hat{V}_j$		(3) $\hat{V}_j$	
	Coef.	SE	Coef.	SE	Coef.	SE
Socio-demographic characteristics						
Age in years	0.003***	(0.000)	-0.000	(0.000)	0.000	(0.000)
Age in years^2 (div. by 100)	-0.003***	(0.001)	0.000	(0.000)	-0.000	(0.000)
School leaving degree (Ref.: None)						
Lower sec. degree	-0.006***	(0.001)	-0.000	(0.001)	-0.001*	(0.001)
Middle sec. degree	0.029***	(0.001)	0.002**	(0.001)	0.000	(0.001)
Upper sec. degree	0.002	(0.002)	0.001	(0.001)	-0.001	(0.001)
Vocational degree (Ref.: None)						
In-firm training	0.017***	(0.001)	0.000	(0.001)	0.000	(0.000)
External training	0.010***	(0.002)	0.000	(0.001)	-0.000	(0.001)
Technical college education	-0.003	(0.003)	0.000	(0.002)	0.000	(0.001)
University degree	0.023***	(0.002)	0.004***	(0.001)	0.001	(0.001)
Children	0.000	(0.001)	-0.000	(0.000)	-0.000	(0.000)
Children $\leq$ 3 years	-0.001	(0.002)	0.000	(0.001)	0.001	(0.001)
Children $\leq$ 10 years	-0.002	(0.001)	0.001	(0.001)	-0.000	(0.000)
Married	0.008***	(0.001)	-0.000	(0.001)	0.000	(0.000)
Lone parent	0.003	(0.003)	-0.000	(0.002)	-0.002	(0.001)
Health problems	-0.003***	(0.001)	-0.001	(0.001)	0.000	(0.000)
Migration background	-0.029***	(0.001)	-0.002***	(0.001)	-0.001***	(0.000)
Professional qualification	0.003***	(0.001)	-0.000	(0.000)	0.000	(0.000)
Any professional experience	-0.004***	(0.001)	-0.001**	(0.001)	-0.000	(0.000)
Labor market history						
Share of jobs inv. commuting in last 5 years	0.002***	(0.001)	0.004***	(0.001)	0.001**	(0.000)
Previous participation in mobility program	0.028***	(0.001)	0.004***	(0.001)	-0.000	(0.001)
Last contact to employment agency (Ref.: more than 3 months ago)						
2-3 months ago	-0.004	(0.004)	-0.001	(0.003)	0.001	(0.002)
1-2 months ago	-0.005	(0.004)	0.001	(0.002)	0.002	(0.001)
within last month	0.000	(0.003)	-0.001	(0.002)	0.001	(0.001)
no previous contact	-0.000	(0.003)	-0.001	(0.002)	0.001	(0.001)
Occupational group of previous job (Ref.: Agriculture)						
Manufacturing	0.001	(0.002)	-0.000	(0.001)	-0.001	(0.001)
Technical occupation	-0.001	(0.002)	-0.001	(0.001)	-0.001	(0.001)
Service	-0.008***	(0.002)	-0.001	(0.001)	-0.001	(0.001)
Other	-0.004	(0.004)	-0.007**	(0.003)	-0.003	(0.002)
Last job was full-time employment	0.022***	(0.003)	0.001	(0.002)	0.000	(0.001)
Log last daily income	-0.008***	(0.002)	0.002	(0.001)	-0.001	(0.001)
Termination of last job: Laid off by employer	0.000	(0.002)	-0.000	(0.001)	-0.000	(0.001)
Months in employment in year						
t-1	-0.001***	(0.000)	-0.000	(0.000)	0.000	(0.000)

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t-2	0.001***	(0.000)	0.000*	(0.000)	0.000	(0.000)
t-3	0.001***	(0.000)	0.000***	(0.000)	0.000	(0.000)
Months in program in year						
t-1	0.000	(0.001)	0.000	(0.000)	-0.000	(0.000)
t-2	0.000	(0.000)	0.000	(0.000)	0.000**	(0.000)
t-3	0.000	(0.000)	0.000	(0.000)	-0.000	(0.000)
Months in unemployment in year						
t-1	0.000	(0.001)	-0.000	(0.001)	-0.000	(0.000)
t-2	-0.002***	(0.000)	-0.000	(0.000)	0.000	(0.000)
t-3	-0.002***	(0.000)	-0.000	(0.000)	0.000	(0.000)
Log average daily wage in year						
t-1	-0.021***	(0.002)	-0.003*	(0.002)	0.001	(0.001)
t-2	0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)
t-3	-0.001***	(0.000)	-0.000	(0.000)	-0.000*	(0.000)
No. of employers in last 24 months	-0.004***	(0.000)	-0.000	(0.000)	0.000	(0.000)
No. of programs in last 4 years	-0.002***	(0.001)	-0.000	(0.001)	0.000	(0.000)
Months in program in last 4 years	-0.003*	(0.002)	0.000	(0.001)	-0.000	(0.001)
Time in UE in last 4 years (in 100 days)	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Time in emp. in last 4 years (in 100 days)	-0.001***	(0.000)	-0.000	(0.000)	0.000	(0.000)
Employed 4 years before entry into UE	0.014***	(0.001)	-0.001	(0.001)	0.001	(0.001)
Daily income 4 years before entry into UE	-0.000***	(0.000)	0.000	(0.000)	-0.000	(0.000)
Total income in last 4 years (in 10,000 €)	0.001***	(0.000)	-0.000	(0.000)	-0.000	(0.000)
No. of employers in last 10 years	-0.001***	(0.000)	-0.000	(0.000)	0.000**	(0.000)
No. of programs in last 10 years	0.002***	(0.001)	-0.001	(0.000)	-0.000	(0.000)
Months in program in last 10 years	0.004***	(0.001)	0.001	(0.001)	0.000	(0.001)
Months in UE in last 10 years	-0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)
Time in emp. in last 10 years (in 100 days)	0.000***	(0.000)	-0.000	(0.000)	-0.000*	(0.000)
No. of UE spells in last 10 years	0.002***	(0.000)	0.000	(0.000)	-0.000	(0.000)
Total time with last employer	0.000	(0.000)	0.000	(0.000)	-0.000	(0.000)
Total income in last 10 years (in 10,000 €)	-0.001***	(0.000)	0.000	(0.000)	0.000	(0.000)
Duration of last employment	-0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Information on current unemployment spell						
Remaining benefit entitlement (Ref.: 0-3 months)						
4-6 months	0.007***	(0.002)	0.002**	(0.001)	0.000	(0.001)
7-9 months	0.009***	(0.002)	0.004***	(0.001)	0.001**	(0.001)
10-12 months	0.007***	(0.001)	0.003***	(0.001)	0.000	(0.001)
more than 12 months	0.015***	(0.002)	0.003**	(0.001)	0.000	(0.001)
Non-compliance with benefit conditions	-0.006***	(0.001)	0.002***	(0.001)	0.001*	(0.000)
Daily unemployment benefits (in 100 €)	-0.014***	(0.004)	-0.008***	(0.003)	0.000	(0.001)
Any vacancy referral	0.000	(0.001)	-0.000	(0.001)	0.000	(0.001)
ALMP Participation during current UE spell	-0.004***	(0.001)	-0.001	(0.001)	-0.000	(0.000)
LEA fixed effects in Equation 4					✓	
No. of observations	30,397		30,397		30,397	
R^2	0.260		0.006		0.003	
Adjusted R^2	0.258		0.004		0.001	

Note: OLS regression of individual characteristics on the unconditional instrument (column 1), respectively the residuals of the instrument without (column 2) and with (column 3) LEA fixed effects. Corresponds to Equation 4.5 in Chapter 4. */**/*** indicates significance at the 10%/5%/1%-level. Standard errors are in parenthesis and clustered at the LEA-level.

5 Usually Unobserved Variables and the Evaluation of Labor Market Policies

The main concern for many evaluation studies is that controlling for individuals' observed characteristics may not be enough to obtain valid treatment effects. We exploit a unique dataset that contains a rich set of administrative information on individuals newly entering unemployment in Germany, as well as several usually unobserved characteristics like personality traits, attitudes, expectations, social networks and intergenerational information. This allows us to empirically assess the effect of including these usually unobserved variables on the propensity score distribution, the matching quality, and the treatment effects obtained using uncounfoundedness-based estimators. Our findings indicate that these variables play a significant role for selection into active labor market programs (ALMP), but do not make a significant difference in estimating treatment effects on wages and employment prospects. This suggests that the usually unobserved variables we analyze are not a threat to the validity of the estimated treatment effects, if comprehensive control variables of the type usually used in modern ALMP evaluations (which include labor market histories) are available. Our results also suggest that rich administrative data may be good enough to draw policy conclusions on the effectiveness of ALMPs.[66]

[66] This chapter is based on joint work with Marco Caliendo and Oscar A. Mitnik (see Caliendo et al., 2017).

5.1 Introduction

Evaluating the *causal* effects on outcomes of an intervention or treatment has become the key empirical objective in many areas of Economics, Statistics, and other fields like Sociology, Political Science, Epidemiology, and Medicine. Among the most exhaustively studied interventions are Active Labor Market Policies (ALMP), both using experimental and nonexperimental methods. After the influential study by LaLonde (1986) raised concerns on the ability of nonexperimental methods to replicate the results of ALMP experiments, a very large literature developed analyzing methodological aspects related to ALMP evaluation, and nonexperimental methods in general.[67] A key ever-present question that nonexperimental ALMP evaluations face is whether the data can account fully for all the factors that explain both the participation in, and the outcomes of, a program. The objective of this chapter is to address this question, relying on unique data on several characteristics usually not observed in the context of ALMP evaluations, for individuals entering unemployment in Germany.

If the assignment to a program is non-random, assumptions are needed to identify the treatment effects of interest. One of the most popular approaches is based on the unconfoundedness or conditional independence assumption (Heckman et al., 1999; Imbens and Wooldridge, 2009). In a binary setting where units are either treated or used as comparisons (controls), the assumption implies that after controlling for differences in observed covariates between the two groups, any remaining differences are as if they had been generated by random assignment to the groups. In the context of ALMP evaluations this implies that researchers need to observe all the variables that affect both treatment participation and labor market outcomes. The main concern is that the unconfoundedness assumption is not realistic in many cases, implying that there may be *unobserved* characteristics that simultaneously explain the particular treatment individuals received and the outcome of interest.[68] In this case, estimators based on the unconfoundedness assumption – e.g. propensity score matching and weighting – become biased, either under- or overestimating the causal effects of the treatment.

Looking back at the last decade, the developments are twofold. On the one hand, many countries now offer access to (very) informative and complete administrative data

[67]See Heckman et al. (1999) for a survey of the ALMP evaluation literature and the early debate on the LaLonde (1986) study; see Kluve (2010) and Card et al. (2010) for an overview of ALMP evaluation in Europe; see Dehejia and Wahba (1999, 2002), Smith and Todd (2005) and Dehejia (2005) on the debate on using propensity score matching to evaluate the training program in the LaLonde study; see Imbens and Wooldridge (2009) for a recent survey of econometric methods used in program evaluation.

[68]Even though the literature uses “selection on observables” as a way of referring to the unconfoundedness assumption, and the term “unobservables” is also commonly used, we prefer to use the term “unobserved” to highlight the fact that the observability of a particular variable will vary for different contexts and data.

– including detailed information on the labor market histories of individuals – increasing the likelihood that the unconfoundedness assumption is satisfied. On the other hand, the recent literature showing the influence of variables such as personality traits or preferences on economic outcomes (e.g. Heckman et al., 2006; Osborne Groves, 2005; Bowles et al., 2001), should be a cause of concern about the validity of the unconfoundedness assumption; these variables might be important on many dimensions in the context of ALMP (e.g. job search behavior, selection into programs, overall labor market performance) but have not been used previously as conditioning variables in this context.

In this chapter we address this concern explicitly. We focus on a class of estimators that rely on comparing treated and control individuals based on the propensity score and exploit a combination of rich administrative and survey data for a fresh inflow sample into unemployment in Germany. The data not only contain "typical" administrative-based information (similar to many other ALMP evaluations, particularly in Europe), but also information on characteristics usually not observed in the context of ALMP evaluations, like personality traits, attitudes, expectations, social networks and intergenerational information.[69] This allows us to empirically assess how estimators based on the unconfoundedness assumption perform when alternatively including or not these usually unobserved variables. The key idea is that even if individuals in the treatment and control groups have similar values of their estimated propensity scores (based on the usually observed variables) they could still differ in the usually unobserved variables. The chapter relates to the prior literature dealing with the sensitivity of unconfoundedness-based estimators. Imbens (2003) and Ichino et al. (2008) have proposed methods to assess the sensitivity of unconfoundedness-based estimators to the presence of unobserved variables. With methodological differences in their approaches, these studies try to assess how large should the effect of hypothetically not observed variables be to invalidate the results obtained from applying propensity score-based estimators in different situations. Lechner and Wunsch (2013) explore, using a German dataset, how sensitive matching estimators are to the inclusion of a variety of usually observed (but rich) characteristics, and find that those rich characteristics can remove selection bias. The chapter also relates to the literature that tries to identify the bias from unobservables by using the amount of selection on observables (e.g. Altonji et al., 2005; Oster, 2017).

Building upon this previous literature, we estimate treatment selection models using alternative sets of variables, for three typical ALMP programs – short-term training, long-term training and wage subsidies. We examine the resulting propensity score distri-

[69]For example Gerfin et al. (2005) for Switzerland, Sianesi (2004) for Sweden, and Lechner et al. (2011) and Biewen et al. (2014) for Germany, use comprehensive administrative data in order to evaluate ALMP programs in (Western) European countries. However, those studies generally lack information about personality traits, attitudes and expectations.

butions, ranks and matching quality. Based on these selection models we estimate average treatment effects on the treated, and compare the effects associated to the alternative variable sets. Our findings indicate that personality traits and other usually unobserved variables play a substantial role for selection into treatment. However, comprehensive control variables (including labor market histories) are able to operate as reasonable proxies for the information provided by the usually unobserved variables. Thus, the differences in treatment effects between including and excluding the usually unobserved variables are in general small. Although, our setting is similar to that of evaluation studies in many countries, it should be noted that evaluating other programs and using different sets of control variables or different evaluation approaches could lead to different conclusions. Nevertheless, our results indicate that the usually unobserved variables we analyze are not a threat to the validity of the treatment effects and suggest that rich administrative data that includes detailed labor market histories may be good enough to draw policy conclusions on the effectiveness of specific active labor market policies.

The chapter is structured as follows. The next section gives a short summary on the identification of treatment effects and the role of potentially unobserved variables. Section 5.3 describes the institutional background and the dataset, and presents some descriptives statistics. Section 5.4 presents the results, while Section 5.5 concludes.

5.2 Unobserved Variables and Treatment Effects

We base our discussion on the well known potential outcomes framework (Roy, 1951; Rubin, 1974) and focus on the usual parameter of interest in most evaluation studies, the average treatment effect on the treated (ATT):

$$\tau_{ATT} = E(Y_i^1 \mid D_i = 1) - E(Y_i^0 \mid D_i = 1). \tag{5.1}$$

Y_i^1 and Y_i^0 are potential outcomes for individual i with and without treatment and D_i is a treatment indicator (equal 1 if individual i received treatment). The last term on the right hand side of equation (1) is not observed and using the realized outcomes of non-participants instead, leads to a bias if participants and non-participants are selected groups who would have different potential outcomes even in the absence of treatment. To correct for this selection bias in non-experimental studies, propensity score matching estimators rely on the conditional independence assumption (CIA), which implies that conditional on the propensity score $P(X_i) = Pr(D_i = 1 \mid X_i)$, where X_i is a set of observed characteristics, the counterfactual outcome is independent of treatment.[70] The CIA is a

[70]In addition to the CIA, we also assume overlap which implies that there are no perfect predictors which determine participation, i.e. $Pr(X_i) < 1$, for all i.

strong assumption and its justification depends crucially on the availability of data which allow the researcher to control for all relevant factors that simultaneously influence the participation decision and the potential outcomes. If there are unobserved variables which affect assignment into treatment and the potential outcomes simultaneously, a *hidden bias* might arise to which matching estimators are not robust (see, e.g. Rosenbaum, 2002, for an extensive discussion). Let us assume that the participation probability is determined by a set of variables $W = (X, U)$, where the variables in X are observed, but the variables in U are not. Then the participation probability can be specified as:

$$P_i = P(X_i, U_i) = P(D_i = 1 \mid X_i, U_i) = F(\beta X_i + \gamma U_i). \tag{5.2}$$

The study is free of any bias if γ is zero. Otherwise, two individuals with the same observed covariates X would differ in their odds of receiving treatment by a factor that involves the parameter γ and the difference in their unobserved covariates U.[71] These unobserved differences with respect to the chance of receiving the treatment would create a *hidden bias* when the outcome is correlated with U, after conditioning on X.

The extent to which γ and U play a role will depend on the empirical context. The importance of the unobserved characteristics U clearly depends on the extent of the observed characteristics. A more informative set of control variables X reduces the likelihood that, after controlling for X the resulting U has an effect on the participation decision. Previous studies suggest that socio-demographic and regional information as well as labor market histories of participants play an important role when evaluating treatment effects (e.g. Mueser et al., 2007; Heckman et al., 1998). Especially, the improved availability and quality of administrative data in recent years has allowed researchers to better understand the effects of certain characteristics on potential treatment effects in a systematic way (e.g. Lechner and Wunsch, 2013; Huber et al., 2013; Biewen et al., 2014). However, at the same time a variety of studies shows the importance of variables previously not extensively considered in economics in general and for ALMP evaluations in particular, like personality traits (Nyhus and Pons, 2005), cognitive and non-cognitive skills (Heckman et al., 2006) or preferences and attitudes (Pannenberg, 2010; Belzil and Leonardi, 2007).

In this context several variables which are usually not observed when evaluating labor market policies, might be of special interest. For example, Mueller and Plug (2006) find for the U.S. that the 'Big Five' personality traits – extraversion, agreeableness, con-

[71] This can be easily seen if, as in Rosenbaum (2002), we assume we have a matched pair of individuals i and j and that F is the logistic distribution. The odds ratio that the individuals receive treatment is given by $\frac{\exp(\beta x_i + \gamma u_i)}{\exp(\beta x_j + \gamma u_j)}$. Successful matching implies that the X-vector cancels out, making the odds ratio equal to $exp[\gamma(u_i - u_j)]$.

scientiousness, neuroticism and openness – have an impact on earnings similar to that found for cognitive abilities. Similar results are found for the Netherlands. Moreover, several empirical studies investigate how an individual's locus of control might be related to labor market performance. Locus of control refers to a general expectation about internal versus external control of reinforcement (Rotter, 1966). People with a more external locus of control believe that much of what happens in life is beyond their control, while people with an internal locus of control see life's outcomes as dependent on their own decisions and behavior. Several studies find a statistically significant effect of the locus of control on individual earnings (e.g. Andrisani, 1977; Heineck and Anger, 2010; Semykina and Linz, 2007) and job search strategies (e.g. Caliendo et al., 2015; McGee, 2015).

Another strand of the literature points out the importance of expectations in general (e.g. Spinnewijn, 2015), but also specifically about ALMP participation (e.g. Black et al., 2003; van den Berg et al., 2009), for the search behavior of unemployed job seekers. Moreover, the intergenerational transmission of human capital (Black et al., 2005) and attitudes (Dohmen et al., 2012), as well as social networks (Montgomery, 1991; Bayer et al., 2008) seem to play an important role in determining an individual's labor market performance. Finally, workers geographically mobility is also a driving factor of their economic outcomes (e.g. Yankow, 2003), while car access seems be of special importance (Gurley and Bruce, 2005). Combining these different strands of literature the natural question that arises is whether these variables also play a role when evaluating the effects of active labor market programs.

5.3 Institutional Background, Data and Descriptives

5.3.1 Institutional Background

Germany has a long tradition of ALMP and the German Social Security Code provides a large set of programs geared towards helping unemployed individuals, like training programs, wage subsidies, job creation schemes, start-up subsidies or benefits to increase the job-seeker's labor market mobility. Table 5.1 shows the entries into different programs in Germany between 2005 and 2011. As they are most relevant (in terms of number of participants) for supporting unemployed job-seekers and very typical for many OECD countries, we investigate the effect of three programs in detail: 1) Short-term training, 2) long-term training and 3) wage subsidies. While the short-term training represents a more recent group of programs shifting the focus towards more 'activating' elements, long-term training and wage subsidies represent more traditional programs, which aim to remove disadvantages in education, work experience or productivity. Since these programs represent very different reintegration strategies and are targeted at different types of unemployed individuals, this potentially allows us to examine the role of usually unobserved variables for these different selection processes. Let us briefly summarize some institutional details for these three programs.

Table 5.1: Entries in ALMP Programs in Germany (in 1,000)

	2005	2007	2009	2011
Entries into unemployment	8,427	8,155	9,253	8,218
Entries into ALMP programs				
Short-term training	901	1,092	1,194	1,201
Long-term training	132	365	618	305
Wage subsidies	134	266	266	187
Job creation schemes	78	70	11	1
Start-up subsidies	91	126	137	137
Mobility assistances[a)]	211	352	21	—

Source: Statistic of the German Federal Employment Agency
[a)] Not separately accounted after 2009.

Short-term training measures, introduced in 1998, have a maximum duration of eight weeks. The courses can either serve as test of the participant's occupation-specific aptitude, or aim to improve the general employability. For example, the courses teach the unemployed how to apply effectively for a new job or how to behave in job interviews, but can also consist of computer or language classes. Some of the courses impart knowledge on starting a business to founders of start-ups, while others are concerned with the special needs of certain 'hard-to-place' job-seekers. Caseworkers can also use them to attain additional information on the participant's abilities and willingness to work. Courses are conducted full- or part-time and last from two days up to eight weeks; an individual's time spent in short-term training programs is limited to twelve weeks in total. While in a short-term training program an unemployed person cannot earn additional wages; however she continues to receive unemployment benefits and coverage of the costs associated to participation (e.g. transportation, child care, see Wolff and Jozwiak, 2007).

Long-term training programs have been a well established part of the German labor market policy for many decades. These programs can last from three months to up to three years. Historically, a caseworker would assign an unemployed individual to a specific course aimed at improving her occupational skills, and facilitating reintegration into the labor market. Previous studies find positive effects only in the very long-run (e.g. Fitzenberger et al., 2008; Lechner et al., 2011) or even partly negative effects on employment (e.g. Lechner and Wunsch, 2008). With the 'Hartz reforms' at the beginning of the century, the German government reduced the usage of (long) vocational training programs. From 2003 onwards caseworkers no longer choose a specific course for the unemployed but hand out a training voucher to the job-seeker who is then allowed to find an appropriate training program for herself (see Bernhard and Kruppe, 2012; Doerr et al., 2017).

Wage subsidies are one of the oldest instruments used to reintegrate unemployed individuals into the labor market. The aim of the subsidy is to reduce the labor costs for the firm, potentially bridging any deficiencies in a worker's productivity. Wage subsidies (or temporary employment with a wage subsidy) can also be used as a screening device, lowering uncertainty and, hopefully, creating stable employer-employee relationships. Whether or not an unemployed person is supported with a targeted wage subsidy is a decision that is made by her caseworker. In addition, the caseworker determines the properties of the subsidy (restricted by the legal framework and guidelines): up to 50 percent of the monthly wage can be covered by the subsidy for at most 12 months. Extensions are possible if the wage subsidy aims at the integration of older or handicapped workers. Employers of subsidized workers agree to employ workers who are younger than 50 years for a follow-up period after the subsidy ends. This follow-up period is usually as long as the subsidized period itself. In case the worker is dismissed for reasons that are not attributable to her performance, the employer has to return a portion of the subsidy. Previous research indicates relatively large favorable effects on the employment prospects of hard-to-place workers using a matching approach (e.g. Bernhard and Wolff, 2008; Jaenichen and Stephan, 2011). However, Schünemann et al. (2015) cast doubts on the findings of the prior literature due to methodological concerns. They argue that propensity score matching based on typically observed individual characteristics, including socio-demographic information and labor market histories, is unlikely to be sufficient for the evaluation of wage subsidies programs, since the receipt of the subsidy is conditional on being employed, which is not an exogenous factor. Exploiting a regression discontinuity design, their study does not find any significant impact of wage subsidies on job finding rates. This is a very important issue to which we will return when interpreting our findings.

5.3.2 Data and Estimation Sample

This chapter is based on the *IZA/IAB Linked Evaluation Dataset* which combines survey information and administrative data on individuals who entered unemployment between June 2007 and May 2008 in Germany (see Caliendo et al., 2011). The dataset contains a 9% random sample, from the monthly unemployment inflows of approximately 206,000 individuals identified in the administrative records, who are selected for interview. From this gross sample of individuals aged between 16 and 54 years, representative samples of about 1,450 individuals are interviewed each month so that after one year twelve monthly cohorts were gathered (see Arni et al., 2014, for details on representativeness etc.). The first wave of interviews takes place shortly after the entry into unemployment. Besides the extensive set of individual-level characteristics and labor market outcomes, the individuals are asked a variety of non-standard questions about search behavior, social networks, psychological factors, cognitive and non-cognitive skills, subjective assessments on future outcomes, and attitudes. For the 88% of individuals who agreed, these survey data were then merged to administrative information from the *Integrated Employment Biographies* (IEB) provided by the Institute for Employment Research (IAB).[72] The IEB integrates different sources, e.g., employment history, benefit recipient history, training participation 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 characteristics including education, family status and health restrictions. The data do not contain information about working hours or periods in self-employment, working as a civil servant, or time spent in inactivity. Altogether, this amounts to a total of 15,274 realized interviews with a time lag from seven to fourteen weeks between the unemployment registration and the interview.

We restrict our estimation sample to all individuals who are still unemployed and do not participate in any ALMP program when the interview takes place. We define job seekers as participants if they attend short- or long-term training, or receive a wage subsidy within the first twelve months after the entry into unemployment, and as non-participants if they do not participate in any ALMP program within this period. This leaves us with 4,934 non-participants, while 1,803 individuals participate in short-term training, 783 in long-term training and 542 receive a wage subsidy.[73]

5.3.3 Descriptive Statistics

We observe every job-seeker in our sample for a period of 30 months after entering unemployment. To evaluate the influence of usually unobserved variables on the treatment

[72] This chapter is based on a weakly anonymized sample of the Integrated Employment Biographies by the IAB (V.901).

[73] The choice of the period for the split is arbitrary and could be debated (see Sianesi, 2004); nevertheless it is a standard procedure in the evaluation of ALMP. In our case, choosing 12 months as the treatment period covers about 89% of all individuals who participate in an ALMP program within our complete observation period of 30 months and ensures that we observe individuals for a sufficiently long time window after the treatment. Moreover, increasing the treatment period, has the disadvantage that the non-participation in later periods is to some extent simply the consequence of a successful job search in earlier periods. Therefore, it becomes less clear whether the estimated effects are causal to the program participation. Alternatively, duration models would allow us to control for the exact timing of the treatment, however additional distributional assumptions would be necessary (see for example Card et al., 2010, for an overview of potential estimation strategies when evaluating 199 worldwide ALMP programs).

effects we focus on labor market outcomes which are typically used in the evaluation of ALMP programs. In particular we concentrate our analysis on the employment probabilities at the end of our observation period after 30 months and the cumulative earnings within the observation period. The upper part of Table 5.2 shows the differences between participants and non-participants with respect to these labor market outcomes. We observe no (statistically) significant differences with respect to their employment probability between non-participants and, respectively, participants in short- and long-term training, while for recipients of a wage subsidy, the raw employment probabilities are higher after 30 months. However, the cumulative earnings are significantly lower for participants in short- and long-term training, but higher for recipients of wage subsidies.[74]

Additionally, the lower part of Table 5.2 shows differences with respect to the main covariates of interest – the *usually unobserved variables.* The first category, *personality traits*, include the 'Big Five' factors (except for agreeableness due to missing items) (see Digman, 1990, for an overview) and locus of control. It can be seen that participants in short-term training show a higher level of neuroticism, a lower internal locus of control and a lower level of openness. The latter is also true for participants in long-term training, while they additionally have a lower level of extraversion and are more conscientious. For recipients of wage subsidies, the only difference to non-participants can be observed with respect to their level of conscientiousness. Second, the *intergenerational variables* contain information on the father of the survey participant. It should be noted that for participants in short-term training and wage subsides, father's education is significantly lower and the father was more likely to be a blue-collar worker when the individuals were 15 years old. Moreover, we account for differences with respect to *social networks* and proxies for *labor market flexibility*, like being a car owner or having problems with childcare. Overall, there are only minor differences with respect to these variables. Individuals participating in short-term training have fewer good friends and participants in long-term training are less likely to have good contacts to their neighbors. Finally, we include variables measuring the individuals *life satisfaction* and *ALMP expectations.* All groups of participants report a lower life satisfaction and a higher ex ante probability of participating in a program.

When considering typical covariates, like socio-demographics and labor market histories, it should be noted that ALMP participants are in general more likely to be female, less likely to have health problems and spent more time in employment in the past. Except for long-term training, participants have also a lower level of education, they are less likely to hold an upper secondary school leaving or an university degree, and earn lower (daily) wages before entering unemployment. Moreover, recipients of wage subsidies

[74] It should be noted that the lower cumulative earnings of participants in training programs are induced by 'locking-in' effects, i.e. lower employment in early periods due to a reduction of participants' search effort before and during program participation (e.g. van Ours, 2004; Jespersen et al., 2008).

Table 5.2: Selected Descriptive Statistics by Treatment Status

	Non-participants	Short-term training	Long-term training	Wage subsidies
No. of observations	4,934	1,803	783	542
Labor market outcomes				
Regular employed 30 months after entry (t+30)	0.55	0.56	0.56	***0.68***
Cumulated earnings in € up to t+30	24,490	***22,075***	***20,615***	***29,273***
Usually unobserved variables				
A. Personality traits[a)]				
Openness	5.06	*5.00*	*4.97*	5.10
Conscientiousness	6.23	6.25	*6.30*	**6.34**
Extraversion	5.20	5.15	***5.07***	5.19
Neuroticism	3.77	*3.83*	3.78	3.71
Locus of Control	5.05	***4.95***	*5.00*	5.00
B. Intergenerational variables				
Father has upper sec. school leaving degree	0.16	***0.11***	0.15	**0.12**
Father was employed when person aged 15	0.84	0.83	0.85	0.86
Father's current age				
$\leq$60 years	0.37	0.36	***0.29***	**0.31**
$>$60 years	0.34	0.33	***0.41***	0.33
Father was blue-collar worker when person aged 15	0.33	**0.36**	0.35	**0.38**
C. Social network				
Number of good friends outside family: less than two	0.09	***0.12***	0.11	0.09
Contacts to neighbors:[b)] good (1-3)	0.73	0.72	*0.70*	0.74
D. Labor market flexibility				
Childcare situation:[b)] bad (4-6)	0.06	0.06	0.07	0.05
Car ownership	0.60	0.61	**0.65**	***0.67***
E. Life satisfaction:[c)]				
Low (0-3)	0.10	0.11	0.10	*0.12*
High (7-10)	0.57	***0.52***	***0.50***	***0.48***
F. Expected ALMP probability:[c)]				
Low (0-3)	0.45	***0.30***	***0.25***	***0.37***
High (7-10)	0.31	***0.43***	***0.55***	***0.39***
Socio-demographic characteristics				
Female	0.46	***0.51***	***0.53***	0.47
Age in years	34.99	34.89	***36.88***	***37.75***
Married (or cohabiting)	0.38	0.39	*0.42*	0.40
German citizenship	0.95	0.94	0.95	0.95
Living West-Germany	0.67	**0.70**	**0.70**	***0.61***
Two children or more	0.13	0.13	***0.17***	0.14
Upper sec. school leaving degree	0.27	***0.18***	0.28	***0.22***
University degree	0.20	***0.13***	0.21	*0.17*
Health restriction or disability	0.08	**0.06**	*0.06*	***0.04***
Labor market history				
Employment status before UE: regular employed	0.64	0.65	**0.68**	***0.70***
Last daily wage in €	46.04	**43.51**	*48.71*	44.79
Last job was full-time employment	0.96	**0.94**	0.95	0.95
Months in regular employment				
in last 6 months	4.07	**4.21**	4.04	**4.32**
in last 2 years	15.02	***16.12***	*15.59*	***16.33***
in last 10 years	49.28	***52.43***	***52.95***	***54.98***
No. of employers				
in last 2 years	1.56	1.56	1.57	**1.65**
in last 10 years	2.90	2.94	2.99	***3.30***

Notes: All numbers denote shares unless otherwise indicated, measured at the entry into unemployment. Italic/bold/italic+bold numbers indicate statistically significant differences between each group of participants and non-participants at the 10/5/1%-level based on a two-tailed t-test on equal means. The full set of socio-demographics and labor market histories is shown in Table 5.7 in the Appendix.

[a)] Personality traits are measured with different items on a 7-point Likert-scale.

[b)] Contacts to neighbors and childcare situation are measured on a 1-6 scale decreasing from good to bad and categorized into two groups.

[c)] Life satisfaction and expected ALMP probabilities are measured on a 0-10 scale increasing from low to high and categorized into three groups.

have a higher probability of living in East-Germany, while participants in both training programs predominately live in West-Germany (compared to non-participants).

5.4 Empirical Results

5.4.1 Estimation Strategy

The objective of this chapter is to examine how estimators based on the unconfoundedness assumption perform when alternatively including – or not – usually unobserved variables. The implementation of propensity score matching and weighting estimators is a two-step-procedure where in a first step the participation model is estimated. The resulting participation probabilities are then used in a second step to match participants with similar non-participants. In the next subsections we evaluate the effect of the usually unobserved variables on each of these steps. To conduct our analysis in a systematic way, we start by estimating different propensity score specifications for each ALMP program. Since labor market histories can be expected to play a key role in this context, we define two types of baseline specifications. The first only includes socio-demographic characteristics, household characteristics, local economic conditions and variables related to unemployment entry, while the second adds detailed information on short- and long-term labor market history-related variables. For each of the two baseline specifications, we then subsequently include each group of usually unobserved variables and evaluate their impact on the propensity score based on various measures.

Afterwards, we focus on three propensity score specifications and analyze implications on the propensity score distributions, the matching quality and the treatment effects in more detail. The choice of these models is motivated by their particular relevance in the context of ALMP evaluations. First, in the *standard* model we include all variables that have been typically used when evaluating these programs, including socio-demographic characteristics (and related variables), as well as short- and long-term labor market histories. This specification provides a reference model including variables which are consistently found to be key drivers of selection into training (e.g. Dolton and Smith, 2011; Lechner and Wunsch, 2013). Second, in the *auxiliary* model we explore the effects of replacing the labor market histories with the full set of usually unobserved variables. With this model we assess the extent to which the usually unobserved covariates U provide information that is similar (or not) to that provided by the labor market histories. This is of special interest, as U contains many characteristics that are typically assumed to be relatively stable over time, e.g., personality traits (see e.g. Heineck and Anger, 2010; Cobb-Clark and Schurer, 2013) and intergenerational variables, and can therefore be expected to influence the labor market outcomes of interest in similar ways as the employment and earning histories. Thus, the auxiliary model allows us to see whether both sets of variables can serve as a proxy for each other, and provides evidence for whether the usual claim in the ALMP evaluation literature that labor market histories can account for unobservables is actually justified. Finally, the *extended* model adds to the standard model the usually unobserved variables introduced in the auxiliary model and therefore exploits all available information. The key intuition behind our approach is that we identify the ATT imposing the assumption that the CIA holds alternatively for the usually observed covariates X (standard), or for the set X plus the set of usually unobserved covariates U (extended). These specifications are not, however, tests of the CIA assumption, nor is ours an exercise in model selection. Comparing the estimated treatment effects allows us to determine the sign and magnitude of potential hidden bias due the exclusion of the usually unobserved variables, but not whether the CIA holds for either model. A detailed depiction of the specifications and the full list of covariates is shown in Table 5.7 in the Appendix.

5.4.2 Relevance for Propensity Score Estimation

We start the analysis by estimating the propensity score for each program using a logit model, as is standard in the literature. For the three treatments the control group contains only individuals which do not participate in any ALMP program within a period of 12 months after the entry into unemployment.[75]

As discussed in the previous section, we sequentially include the six groups of usually unobserved variables for two types of baseline specifications. First, only with socio-demographic and related variables and second, additionally including labor market histories. Finally, we jointly include all usually unobserved variables, which results in 16 specifications per treatment. For each model, three types of summary statistics are presented in Table 5.3:[76] i) *hitrate* indicates the share of correct predictions from the estimated model;[77] ii) *F-test* presents the p-values for *Wald* tests of the joint significance of groups of usually unobserved variables on the participation probability; and iii) *SSVY-test*

[75] The alternative to estimating several reduced-form binary logit models would be estimating a multinomial logit model on the full set of potential treatment choices. However, we concentrate on binary choice models since these are more popular among typical evaluation studies. Moreover, Lechner (2002) shows that the results for matching estimators based on different models are relatively similar using Swiss data.

[76] Full estimation results including marginal effects for each variable are presented in Tables 5.8–5.10 in the Appendix.

[77] To calculate the hitrate we classify an observation as 1 if the estimated propensity score is larger than the sample average of individuals receiving the treatment (i.e. $\hat{P}(X) > \bar{P}$) and 0 otherwise (i.e. $\hat{P}(X) \leq \bar{P}$) (see Heckman and Smith, 1999; Caliendo and Kopeinig, 2008).

Table 5.3: Summary of Propensity Score Estimation: Sequential Inclusion of Usually Unobserved Variables

	Baseline specification: Socio-demographic characteristics, Household characteristics, Regional and seasonal information			*Baseline specification:* Socio-demographic characteristics, Household characteristics, Regional and seasonal information, Short-term labor market histories, Long-term labor market histories		
		$P-$value			$P-$value	
	Hitrate	F-test[1)]	SSVY-test[2)]	Hitrate	F-test[1)]	SSVY-test[2)]
Short-term training						
Baseline specification and ...	0.570		0.657	0.602		0.489
Personality traits	0.577	0.002	0.462	0.602	0.000	0.952
Intergenerational variables	0.569	0.439	0.354	0.602	0.512	0.894
Social network	0.572	0.033	0.570	0.603	0.045	0.480
Labor market flexibility	0.569	0.529	0.270	0.601	0.752	0.475
Life satisfaction	0.571	0.000	0.312	0.603	0.000	0.828
ALMP expectations	0.594	0.000	0.251	0.614	0.000	0.498
All of them	0.596	0.000	0.278	0.617	0.000	0.254
Long-term training						
Baseline specification and ...	0.574		0.010	0.626		0.406
Personality traits	0.585	0.002	0.657	0.635	0.005	0.096
Intergenerational variables	0.575	0.818	0.110	0.628	0.876	0.360
Social network	0.579	0.174	0.018	0.623	0.177	0.554
Labor market flexibility	0.578	0.270	0.019	0.628	0.374	0.417
Life satisfaction	0.584	0.002	0.569	0.626	0.001	0.679
ALMP expectations	0.660	0.000	0.064	0.674	0.000	0.595
All of them	0.662	0.000	0.797	0.675	0.000	0.509
Wage subsidies						
Baseline specification and ...	0.603		0.894	0.656		0.353
Personality traits	0.602	0.256	0.748	0.653	0.233	0.355
Intergenerational variables	0.608	0.383	0.203	0.660	0.294	0.620
Social networks	0.604	0.713	0.855	0.655	0.707	0.389
Labor market flexibility	0.607	0.067	0.670	0.655	0.111	0.586
Life satisfaction	0.607	0.005	0.398	0.655	0.006	0.434
ALMP expectations	0.614	0.000	0.537	0.662	0.000	0.475
All of them	0.623	0.000	0.306	0.669	0.000	0.215

Notes: Full estimation results are available in the appendix. $P-$values refer to 1) a F-test on joint significance when separately including each block of usually unobserved variables and 2) the specification test presented in Shaikh et al. (2009) using a normal kernel with bandwidth $0.05n^{-1/8}$. Full estimation results for the main specifications can be found in Table 5.8-5.10 in the Appendix.

presents p-values for the specification test (proposed by Shaikh et al., 2009) for whether the estimated propensity score models are misspecified.[78]

Briefly summarizing the estimation results, it should be noted that ALMP expectations and life satisfaction have a statistically significant impact on the participation probability in all three programs. Unsurprisingly, a high expectation of ALMP participation, but also a low level of life satisfaction, increases the likelihood of receiving a treatment. Moreover, for both types of training programs, we find a significant effect of personality traits, while for short-term training also social networks seem to influence the participation decision. Among the personality traits, the results presented in the Appendix suggest that having an internal locus of control reduces the participation probability in short-term training, while a low level of extraversion increases the participation probability in long-term training.

When comparing the two different baseline specifications, it can be seen that for long-term training the model without labor market histories is more likely to be misspecified – as indicated by the low p-value associated to the SSVY-test – and that for this model the addition of the usually unobserved variables has a larger impact (8.8 vs. 4.9 percentage points) on the hitrate. The differences according to the choice of the baseline specification are less pronounced for short-term training and wage subsidies. Moreover, comparing the baseline specification of the second model (including socio-demographic characteristics and labor market histories) and the full model, it can be seen that including all usually unobserved variables increases the share of observations correctly predicted between 1.3 percentage points for wage subsidies and 4.9 percentage points for long-term training, while the major part of the increased hitrate, especially for long-term training, can be explained by the inclusion of ALMP expectations.

In the following, we focus on the three specific models (standard, auxiliary and extended) discussed in Section 5.4.1. First of all, it should be noted that for none of these three models we find evidence of misspecification. Moreover, Figure 5.1 presents the propensity score distributions for each specification, separately for the three types of treatments and by participation status. The figure shows the importance of the usually unobserved variables in explaining program participation: the distributions are in general affected by the introduction of these variables, except for non-participants when used as comparison group for the long-term training and wage subsidies treatment.

To further explore the relationship between the three propensity score specifications, Table 5.4 provides several correlation measures between them. When comparing

[78] We thank an anonymous reviewer for suggesting the use of this test, which is given by: $\hat{V}_n = \frac{1}{n(n-1)} \sum_{i=1}^{n} \sum_{j \neq i} K\left(\frac{\hat{P}(X_i)-\hat{P}(X_j)}{h}\right) \hat{\varepsilon}_i \hat{\varepsilon}_j$, with kernel K, bandwidth h and $\hat{\varepsilon}_i = D_i - \hat{P}(X_i)$. We also thank Edward Vytlacil for sharing the Gauss code used by Shaikh et al. (2009), which we adapted to Stata.

Table 5.4: Consequences for Propensity Scores and Ranks

	Standard v. auxiliary	Extended v. standard
	(1)	(2)
Short-term training		
Propensity score correlation: Pearson's r	0.616	0.839
Rank correlation: Spearman's ρ	0.619	0.834
Absolute value of score difference: treated and matched controls$^{a)}$		
Mean	0.092	0.083
Median	0.078	0.071
Maximum	0.453	0.359
Distribution comparison: Wilcoxon signed-ranks test		
Participants	-1.832	9.356
	{0.067}	{0.000}
Non-participants	0.554	-6.968
	{0.580}	{0.000}
Long-term training		
Propensity score correlation: Pearson's r	0.463	0.740
Rank correlation: Spearman's ρ	0.503	0.749
Absolute value of score difference: treated and matched controls$^{a)}$		
Mean	0.084	0.099
Median	0.069	0.078
Maximum	0.541	0.418
Distribution comparison: Wilcoxon signed-ranks test		
Participants	-5.165	10.592
	{0.000}	{0.000}
Non-participants	4.061	-11.416
	{0.000}	{0.000}
Wage subsidies		
Propensity score correlation: Pearson's r	0.655	0.929
Rank correlation: Spearman's ρ	0.688	0.929
Absolute value of score difference: treated and matched controls$^{a)}$		
Mean	0.075	0.041
Median	0.055	0.031
Maximum	0.565	0.286
Distribution comparison: Wilcoxon signed-ranks test		
Participants	3.910	4.747
	{0.000}	{0.000}
Non-participants	-5.806	-6.269
	{0.000}	{0.000}

Notes: $P-$values are shown in brackets.
$^{a)}$This refers to: (1) the propensity score difference for the standard specification between participants and matched non-participants in the auxiliary specification, respectively (2) the propensity score difference for the extended specification between participants and matched non-participants in the standard specification.

the extended and the standard specification, the correlation between propensity scores, as well as between corresponding ranks is very high and similar for all programs, while much lower when comparing the standard and the auxiliary specifications. Additionally, we also consider the absolute difference in propensity scores in the extended (standard) model for individuals who have been matched in the standard (auxiliary) model. It can be seen that the average difference in the extended model is more than twice as large for short- and long-term training than for wage subsidies. This highlights the importance of the usually unobserved variables for propensity score estimation, especially for the two training programs. Finally, we employ a Wilcoxon signed-ranks test which compares the rank of the individual propensity score differences for the two specifications of interest. In contrast to a simple sign test, it accounts not only for the direction but also for the size of the differences (e.g. Wilcoxon, 1945). When comparing the standard and auxiliary specification for long-term training, it can be seen that including the usually unobserved variables instead of the labor market history shifts the propensity score distribution to the right for participants, and respectively to the left for non-participants, while it is exactly the other way around for wage subsidies.

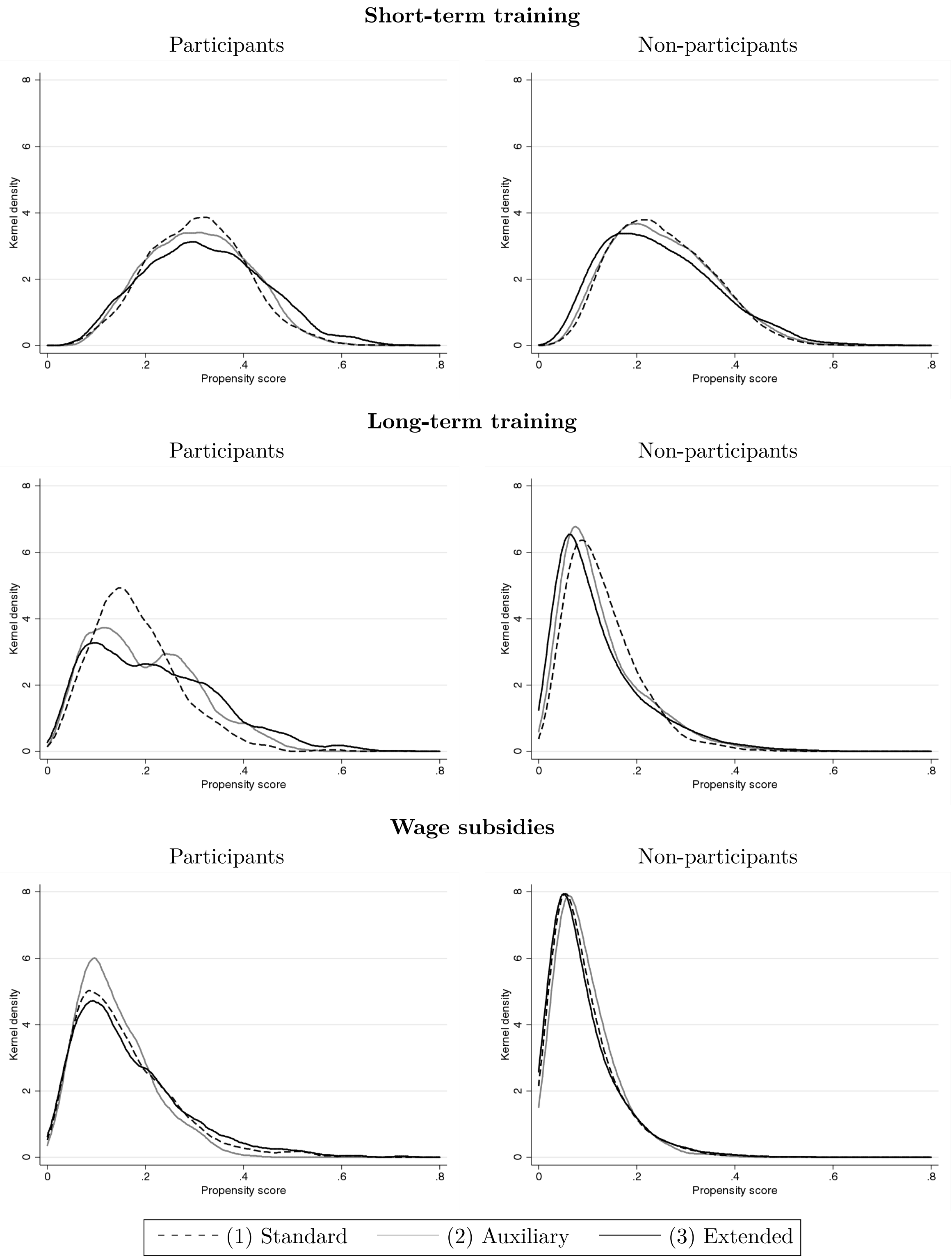
It is useful to relate the characteristics of the different programs to their selection process. For all programs the results related to the usually unobserved variables are intuitive: training programs, especially long-term training, require a high degree of commitment by the trainees, to endure a program that can be as long as three years. It is not surprising then that personality traits matter for selection into training programs (as indicated by their joint significance). Wage subsidies aim to help individuals find employment in markets where the demand for labor may be weak for the particular skills of these individuals. Therefore, it is not unexpected that the labor market histories of the individuals have the main explanatory power in the selection process.

5.4.3 Consequences for Matching Quality

An important indicator of the quality of the matching, and of the propensity score specification, is the balancing in the distribution of covariates between participants and non-participants. One suitable indicator for balancing is the mean standardized bias (Rosenbaum and Rubin, 1983), which assesses the distance of the covariates before and after matching.[79] In Table 5.5 we present the mean standardized bias (MSB) for groups of variables and overall, for the different specifications. In our setting, it is especially interesting to assess the matching quality for the usually unobserved variables under the standard

[79] For each covariate X, it is defined as: $SB(x) = 100(\bar{x}_c - \bar{x}_t)/\sqrt{\frac{1}{2}(s^2_{xc} + s^2_{xt})}$ with $\bar{x}_c$ being the mean of the control group, $\bar{x}_t$ the mean of the treatment group, s^2_{xc} the variance of the control group and s^2_{xt} the variance of the treatment group.

Figure 5.1: Propensity Score Distribution



Note: Depicted are epanechnikov kernel densities (bandwith=0.06) of the propensity score after matching on the four propensity score specifications.

specification, which does not include these variables; it is a useful way to summarize the degree to which matching only on the socio-demographic characteristics and labor market history can proxy for matching on the usually unobserved variables.

The first column of Table 5.5 presents the *raw* MSB, i.e. prior to matching, while the next three columns present the MSB when matching with the alternative specifications of the propensity score. Using the extended propensity score specification reduces the overall MSB down to 2.1 for short-term training, to 3.0 for long-term training, and to 3.5 for wage subsidies, all very low values. However, on closer examination we find substantial differences with respect to the different programs and groups of control variables. For all types of programs, we find the largest raw bias with respect to ALMP expectations (MSB ranging from 17.2 for wage subsides up to 47.3 for long-term training), while, except for life satisfaction, it is on a moderate level for the other groups of usually unobserved variables. Overall, the standard specification is not very successful in reducing the MSB for ALMP expectations (for all programs) and life satisfaction (especially for short- and long-term training). When considering the auxiliary model it can be seen that, especially for wage subsides, conditioning on the usually unobserved variables does not reduce the mean bias for labor market histories which indicates that both groups of variables can not be considered as good proxies for each other. These results are in line with our previous findings which have shown that expectations and measures for life satisfaction, but also labor market histories, have a strong impact on the selection into all types of programs. More importantly, estimating the propensity score using only the standard variables does not appear to be successful in eliminating the differences in the usually unobserved variables, which appear as very important for the selection into treatment process.

5.4.4 Consequences for Treatment Effects

In this section we present the consequences of using the alternative propensity score specifications for the estimation of the treatment effects of each program. There are several possible estimators for the Average Treatment on the Treated (ATT) parameters we are interested in obtaining (e.g. Imbens and Wooldridge, 2009). For the sake of clarity, we focus our analysis on a particular estimator, kernel matching, which is heavily used in evaluation studies. When relying on kernel matching estimators researchers need to specify a kernel function and a bandwidth parameter.[80] We specify an Epanechnikov kernel, and a bandwidth of 0.06. In the Appendix we conduct a sensitivity analysis where we specify

[80]In contrast to the choice of the bandwidth parameter, where a trade-off between a small variance and an unbiased estimate of the true conditional mean function arises, the choice of the kernel type appears to be relatively less important in practice (see the discussion in Caliendo and Kopeinig, 2008; Galdo et al., 2008).

Table 5.5: Consequences for Matching Quality: Mean Standardized Bias

	Unconditional raw difference	Propensity score specification		
		Standard (1)	Auxiliary (2)	Extended (3)
Short-term training				
Socio-demographic characteristics	5.10	2.33	1.98	2.29
Labor market histories	7.85	2.06	4.06	1.91
Personality traits	5.83	6.13	2.33	2.58
Intergenerational variables	5.42	2.25	1.33	2.12
Social network	6.70	4.59	1.79	3.39
Labor market flexibility	2.34	3.21	2.96	1.08
Life satisfaction	8.71	9.35	0.78	0.56
ALMP expectations	29.7	30.0	1.91	0.81
Total	6.59	3.32	2.57	2.11
Long-term training				
Socio-demographic characteristics	4.80	3.55	3.05	3.12
Labor market history	10.5	2.80	9.01	2.70
Personality traits	6.95	5.80	2.91	3.24
Intergenerational variables	7.95	1.66	4.94	5.26
Social network	6.50	5.98	4.97	1.57
Labor market flexibility	7.69	5.23	4.46	1.83
Life satisfaction	8.96	12.1	2.26	1.88
ALMP expectations	47.3	45.4	1.89	2.81
Total	7.96	4.59	4.94	3.02
Wage subsidies				
Socio-demographic characteristics	7.19	3.85	4.23	3.29
Labor market history	15.5	4.24	13.6	4.44
Personality traits	5.99	5.75	3.65	3.42
Intergenerational variables	8.56	3.20	2.38	1.77
Social network	0.62	1.99	1.17	1.06
Labor market flexibility	8.85	2.12	3.10	5.71
Life satisfaction	14.3	5.83	2.75	4.56
ALMP expectations	17.2	16.6	3.23	1.51
Total	9.95	4.29	6.71	3.54
Propensity score specification				
Personality traits			✓	✓
Inter-generational variables			✓	✓
Social network			✓	✓
Labor market flexibility			✓	✓
Life satisfaction			✓	✓
ALMP expectations			✓	✓
Socio-demographic characteristics		✓	✓	✓
Labor market history		✓		✓

Notes: Reported are the mean standardized bias for each block of covariates calculated over the absolute standardized bias over all covariates in the block. The standardized bias is calculated as the difference of sample means for participants and non-participants as a percentage of the square root of the average of sample variances in both groups (Rosenbaum and Rubin, 1983).

alternative estimators (inverse probability weighting, IPW, nearest neighbor and radius matching), and bandwidth parameters ($bw = 0.02$; $bw = 0.2$) for the kernel estimators. The estimation results are qualitatively similar for all types of estimators.

Table 5.6 presents the differences in mean outcomes (raw gap) as well as the ATT from using the kernel estimator. We use the same three specifications for the propensity score discussed above. As outcomes of interest we analyze the employment probability 30 months after the entry into unemployment and the cumulated earnings within 30 months. The left panel of the table shows the ATTs, while the right panel calculates the difference in ATTs. Shortly summarizing the estimated effects of the different programs we find no effect of short- and long-term training on the employment probability after 30 months, and we find negative effects of those programs on unconditional earnings. Regarding wage subsidies we find a positive and significant effect on the employment probability after 30 months and on cumulated earnings, although these positive results may suffer from an upward bias, as we discuss further below. Our estimation results on long-term training are in line with previous studies, that find negative effects on employment probabilities in the short-run (e.g Lechner and Wunsch, 2008; Doerr et al., 2017) (which most likely shows up as a negative effect on unconditional earnings). Also for wage subsidies we find similar positive effects, at least after 30 months, as Bernhard and Wolff (2008) or Jaenichen and Stephan (2011). With respect to short-term training, Biewen et al. (2014) report a short locking-in period after the treatment which is in line with the negative effect on cumulated earnings. However, in the long-run we find no positive effect on employment probabilities (e.g. Wolff and Jozwiak, 2007) nor a reduction of the unemployment duration (e.g. Hujer et al., 2006) as found by prior studies, for different time periods and target groups, in this literature.

Our main interest is in the comparison of the estimated treatment effects using alternative propensity score specifications. First of all, it should be noted that for all outcome variables and treatments the estimated ATTs conditioning on the standard set of covariates differ significantly from the unconditional raw differences. However, when comparing the propensity score specifications, it can be seen that the overall differences across specifications are relatively small. When comparing the standard with the auxiliary specification, we only find statistically significant (small) differences for long-term training in one of the two outcomes variables, suggesting in that case that the labor market histories cannot serve completely as proxies for the usually unobserved variables. However, except for employment 30 months after entry for wage subsidies participants, we do not find a statistically significant – or an economically relevant – difference between the standard and the extended specifications for the different programs and outcomes. This supports the idea that a large part of the usually unobserved characteristics, especially those that are constant over time, can be captured by controlling for the prior labor market performance.

Table 5.6: Matching Estimation Results: Consequences for the Average Treatment Effects on the Treated (ATT)

	Unconditional	Propensity score specification			Differences		
	raw difference	Standard (1)	Auxiliary (2)	Extended (3)	Standard v. uncond.	Standard v. auxiliary	Extended v. standard
Short-term training							
Regular employed in t+30	0.008	-0.002	-0.004	-0.004	-0.0096***	0.0025	-0.0024
	(0.014)	(0.013)	(0.013)	(0.014)	(0.0036)	(0.0035)	(0.0028)
Cumulated earnings in € up to t+30	-2,416***	-1,905***	-2,129***	-1,886***	511***	224	19
	(647)	(585)	(489)	(535)	(174)	(157)	(133)
No. of observations	6,737	6,737	6,737	6,737			
Long-term training							
Regular employed in t+30	0.006	-0.009	-0.025	-0.017	-0.0143***	0.0164***	0.0089
	(0.019)	(0.021)	(0.021)	(0.022)	(0.0048)	(0.0062)	(0.0059)
Cumulated earnings in € up to t+30	-3,876***	-5,280***	-5,656***	-5,618***	-1,404***	-376	-338
	(938)	(823)	(846)	(859)	(331)	(327)	(323)
No. of observations	5,717	5,717	5,717	5,717			
Wage subsidies							
Regular employed in t+30	0.128***	0.095***	0.100***	0.087***	-0.0331***	-0.0051	-0.0075*
	(0.022)	(0.022)	(0.022)	(0.023)	(0.0063)	(0.0058)	(0.0045)
Cumulated earnings in € up to t+30	4,783***	3,924***	3,723***	3,691***	-859***	201	233
	(1,105)	(805)	(830)	(839)	(284)	(262)	(194)
No. of observations	5,476	5,476	5,476	5,476			
Propensity score specification							
Personality traits			✓	✓			
Inter-generational variables			✓	✓			
Social network			✓	✓			
Labor market flexibility			✓	✓			
Life satisfaction			✓	✓			
ALMP expectations			✓	✓			
Socio-demographic characteristics		✓	✓	✓			
Labor market history		✓		✓			

Note: Depicted are estimated average treatment effects on the treated as the difference in mean outcomes between participants and matched non-participants using epanechnikov kernel propensity score matching with bandwidth 0.06. Standard errors are in parentheses and based on bootstrapping with 999 replications. Standard errors for the differences in ATT's are based on bootstrapped robust Hausman tests with 999 replications (see Cameron and Trivedi, 2010, for details). ***/**/* indicate statistically significance at the 1/5/10%-level.

The results for the ATTs seem surprising given the importance for the selection process that the usually unobserved variables seem to have, as indicated by the propensity score marginal effects and distributions, and the measures of matching quality.[81] It is clear that the distinct selection processes suggested by the propensity score specifications are not reflected in differences in the estimated treatment effects. Overall, our results make it clear that the variables in the standard model are able to capture most of the information contained in the usually unobserved variables which are relevant for the labor market outcomes we analyze. This point is probably even stronger if some of the usually unobserved variables are stable over time as, for example, personality traits are expected to be. In general, it is reasonable to expect that the higher is the correlation between the usually unobserved variables and the labor market history, the smaller the additional value of the usually unobserved variables will be.

Regarding wage subsidies it is important to reiterate that, as discussed by Schünemann et al. (2015), the selection process into the program is expected to be more complex since participation additionally requires the presence of an employer who hires the job seeker. Therefore, propensity score matching based on typical individual characteristics might not be sufficient to model the selection process and hence the standard model would tend to overestimate the actual treatment effect. Our findings indeed show a small reduction of the estimated ATT when accounting for usually unobserved variables in the extended model. However, it should be noted that, in contrast to Schünemann et al. (2015) who use a regression discontinuity design, we still find a positive effect which suggests that additional information, e.g. employer characteristics, might be necessary in order to obtain a valid treatment effect for wage subsidies.

5.5 Conclusion

The aim of this chapter was to investigate the effect of usually unobserved variables, like personality traits, attitudes, expectations, social networks and intergenerational information, on the selection into active labor market policy programs and on the estimated average treatment effects. The results present a clear picture. The usually unobserved variables matter in terms of the selection process into treatment, in a different manner for the individuals treated under each of the programs. This is consistent with the three programs representing distinct reintegration strategies targeted to different types of unemployed individuals. Even though we find that the usually unobserved variables matter

[81] Even though our methodological approach is not based on linear regression, our results are consistent with those of Oster (2017) who shows that the omission of unobserved variables, in the context of linear models, does not necessarily generate large changes in the coefficients associated to a treatment, even if their omission generates omitted variable bias.

for selection, when estimating the effects of ALMP programs on labor market outcomes in a second step, the overall influence of including or excluding them is rather small.

The relatively small overall impact on treatment effects of the usually unobserved variables seems to be explained by the comprehensive control variables in the standard propensity score specifications, including labor market history variables. Assuming that the usually unobserved variables are constant over time, they not only affect selection into programs and future labor market outcomes, but they are probably correlated with past labor market performance. Thus, conditioning on labor market histories implicitly captures a large part of the information contained in the usually unobserved variables. Our results show that given our set of usually unobserved characteristics, the influence of these variables on the effect of ALMP programs in Germany is generally limited when informative administrative data are available. This suggests that lacking these usually unobserved characteristics does not affect in a fundamental way the assessment of public policies: As long as a large enough set of covariates is available any expected biases associated to not observing some of the personality traits, expectations and socio-cultural characteristics, are likely to be sufficiently small as to not fundamentally affect policy conclusions.

Moreover, it is necessary to be prudent in generalizing our results outside the specific setting: The effects can clearly differ among different types of programs, different countries and populations of interest, as well as for other types of unobserved variables. Nevertheless, it shows that valid concerns about the role of unobserved variables, when using a "selection on observables" assumption for the estimation of treatment effects, may be less relevant when observable information is available that is sufficiently correlated to the unobservable variables. This clearly seems to be the case in settings, like in many European countries these days, where policy evaluation is based on detailed administrative data. Finally, the chapter provides a sort of "road-map" for researchers interested in systematically assessing the sensitivity of their results to the inclusion of alternative sets of control variables, which can be used in the context of any observational study relying on the unconfoundedness assumption.

5.6 Appendix

Table 5.7: Overview - Control Variables and Propensity Score Specifications

	Propensity score specification		
	Standard (1)	Auxiliary (2)	Extended (3)
1) Usually unobserved variables			
A. Personality traits		✓	✓
Openness, Conscientiousness, Extraversion, Neuroticism, Locus of control			
B. Intergenerational variables		✓	✓
Father has A-level qualification (upper sec. degree), Father's current age, Father was employed when person aged 15, Father was blue-collar worker when person aged 15			
C. Social Network		✓	✓
Number of good friends outside the family, Contact to neighbors			
D. Labor market flexibility		✓	✓
Childcare situation, Car-ownership			
E. Life satisfaction		✓	✓
Life satisfaction: low, medium, high			
F. ALMP expectations		✓	✓
Expected ALMP participation probability: low, medium, high			
2) Socio-demographic/ baseline variables			
A. Individual and household characteristics	✓	✓	✓
Gender, Age, Migration background, School leaving degree, Level of higher education, Marital status, German citizenship, Number of children, Health problems, Searching for full- or part-time employment, Employment status of partner			
B. Regional and seasonal information			
Living in West-Germany, Local unemployment rate, Month of entry into unemployment, Time between entry into UE and interview	✓	✓	✓
3) Labor market history			
A. Short-term labor market history	✓		✓
Employment status before entry into unemployment, Last daily wage, Last job was full-time employment, Laid off by last employer, Time with last employer, Duration of last unemployment spell			
Months in employment/ unemployment/ ALMP program/ out of labor force in last 6 months/ 24 months,			
Number of employers/ program participation/ unemployment spells/ out of labor force spells in last 24 months			
B. Long-term labor market history	✓		✓
Months in employment/ unemployment/ ALMP program/ out of labor force in last 10 years,			
Number of employers/ program participation/ unemployment spells/ out of labor force spells in last 10 years			

Table 5.8: Propensity Score Estimation: Short-term Training

	Standard		Auxiliary		Extended	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Personality traits$^{a)}$						
Openness (standardized)			-0.007	(0.006)	-0.005	(0.006)
Conscientiousness (standardized)			0.005	(0.006)	0.003	(0.006)
Extraversion (standardized)			-0.007	(0.006)	-0.008	(0.006)
Neuroticism (standardized)			-0.007	(0.006)	-0.008	(0.006)
Locus of control (standardized)			-0.017***	(0.006)	-0.020***	(0.006)
Intergenerational variables						
Father has upper sec. school leaving degree			-0.023	(0.017)	-0.020	(0.017)
Father was employed when person aged 15			-0.001	(0.016)	-0.000	(0.016)
Father's current age (Ref.: already passed away)						
$\leq$ 60 years			-0.017	(0.017)	-0.017	(0.017)
$>$ 60 years			-0.001	(0.014)	-0.005	(0.014)
Father was blue-collar worker when person aged 15			-0.001	(0.013)	0.002	(0.013)
Social network$^{b)}$						
No. of good friends outside family: Less than two			0.045**	(0.019)	0.041**	(0.018)
Contacts to neighbors: Good (1-3)			-0.002	(0.012)	-0.000	(0.012)
Labor market flexibility						
Childcare situation: Bad (4-6)			0.010	(0.025)	0.000	(0.025)
Car-ownership			0.020*	(0.012)	0.017	(0.012)
Life satisfaction$^{c)}$						
Life satisfaction (Ref.: Medium (4-6))						
Low (0-3)			-0.003	(0.019)	-0.006	(0.019)
High (7-10)			-0.035***	(0.011)	-0.037***	(0.011)
ALMP expectations$^{c)}$						
Expected ALMP probability (Ref.: Medium (4-6))						
Low (0-3)			-0.085***	(0.012)	-0.078***	(0.012)
High (7-10)			0.054***	(0.015)	0.053***	(0.015)
Socio-demographic characteristics						
Female	0.037***	(0.013)	0.045***	(0.013)	0.033**	(0.013)
Age (Ref.: 16-24 years)						
25-34 years	-0.013	(0.019)	-0.022	(0.017)	-0.020	(0.019)
35-44 years	-0.024	(0.021)	-0.045**	(0.020)	-0.045**	(0.023)
45-55 years	-0.012	(0.023)	-0.039*	(0.022)	-0.040	(0.025)
Married or cohabiting	0.019	(0.016)	0.023	(0.016)	0.018	(0.016)
German citizenship	-0.013	(0.029)	-0.008	(0.029)	-0.009	(0.029)
Migration background	0.003	(0.020)	-0.007	(0.019)	-0.004	(0.019)
Children (Ref.: None)						
One child	-0.005	(0.015)	-0.000	(0.015)	-0.002	(0.015)
Two children or more	0.001	(0.019)	0.001	(0.019)	-0.001	(0.019)
School leaving degree (Ref.: None)						
Lower sec. degree	-0.018	(0.035)	-0.009	(0.035)	-0.008	(0.034)
Middle sec. degree	-0.035	(0.034)	-0.019	(0.035)	-0.020	(0.034)
Upper sec. degree	-0.098***	(0.030)	-0.079**	(0.032)	-0.073**	(0.032)
Higher education (Ref.: None)						
Int. or ext. vocational training	0.023	(0.019)	0.036*	(0.019)	0.028	(0.019)
University degree	-0.029	(0.022)	-0.017	(0.022)	-0.015	(0.023)
Month of entry into unemployment (Ref.: June)						
July	-0.015	(0.030)	-0.018	(0.030)	-0.022	(0.030)
August	-0.029	(0.030)	-0.036	(0.028)	-0.042	(0.029)
September	-0.026	(0.030)	-0.015	(0.030)	-0.023	(0.031)
October	0.007	(0.029)	0.006	(0.028)	0.001	(0.029)
November	-0.034	(0.027)	-0.038	(0.026)	-0.038	(0.027)
December	0.005	(0.032)	-0.004	(0.031)	0.001	(0.032)
January	-0.038	(0.031)	-0.042	(0.030)	-0.048	(0.031)
February	0.024	(0.033)	0.031	(0.033)	0.016	(0.033)
March	-0.008	(0.032)	0.010	(0.033)	-0.010	(0.032)
April	-0.074***	(0.026)	-0.057**	(0.027)	-0.074***	(0.026)
May	-0.039	(0.028)	-0.023	(0.029)	-0.044	(0.028)

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Time between entry into UE and interview (Ref.: 7 weeks)						
8 weeks	0.009	(0.040)	-0.004	(0.039)	-0.006	(0.040)
9 weeks	0.040	(0.044)	0.024	(0.043)	0.023	(0.043)
10 weeks	0.028	(0.044)	0.013	(0.043)	0.014	(0.044)
11 weeks	0.030	(0.046)	0.022	(0.046)	0.018	(0.046)
12 weeks	0.041	(0.051)	0.026	(0.050)	0.027	(0.050)
13 weeks	0.006	(0.052)	-0.012	(0.051)	-0.016	(0.051)
14 weeks	0.036	(0.051)	0.028	(0.051)	0.019	(0.050)
Health restriction or disability	-0.063***	(0.020)	-0.069***	(0.019)	-0.068***	(0.019)
Job search (Ref.: Full- or part-time employment)						
Full-time employment only	0.088***	(0.013)	0.086***	(0.013)	0.082***	(0.013)
Part-time employment only	0.044**	(0.020)	0.064***	(0.020)	0.050**	(0.021)
Employment status partner (Ref.: No partner)	ref.		ref.		ref.	
Full-time employed	-0.017	(0.015)	-0.006	(0.015)	-0.005	(0.015)
Part-time employed	-0.011	(0.025)	-0.008	(0.024)	-0.005	(0.025)
Education	-0.021	(0.023)	-0.016	(0.022)	-0.012	(0.023)
Unemployment	-0.050*	(0.028)	-0.043	(0.027)	-0.041	(0.027)
Other	0.002	(0.025)	0.015	(0.025)	0.011	(0.025)
Region (Ref.: West-Germany: UE rate 0-3%)						
West-Germany: UE rate 4-6%	0.085	(0.083)	0.063	(0.084)	0.062	(0.085)
West-Germany: UE rate 7-9%	0.072	(0.081)	0.063	(0.084)	0.056	(0.084)
West-Germany: UE rate $\geq$ 10%	0.054	(0.078)	0.045	(0.082)	0.038	(0.081)
East-Germany: UE rate 9-12%	0.054	(0.080)	0.043	(0.083)	0.041	(0.084)
East-Germany: UE rate 13-14%	0.065	(0.081)	0.061	(0.085)	0.056	(0.085)
East-Germany: UE rate 15-16%	0.014	(0.071)	0.004	(0.074)	-0.001	(0.074)
East-Germany: UE rate $\geq$ 17%	0.072	(0.083)	0.066	(0.087)	0.059	(0.086)
Short-term labor market history						
Employment status before UE (Ref.: Regular employed)						
Subsidized employed	0.014	(0.022)			0.017	(0.022)
School, apprentice, military, etc.	-0.007	(0.019)			-0.004	(0.019)
Parental leave	0.086**	(0.036)			0.084**	(0.035)
Months employed in last 6 months	-0.004	(0.006)			-0.004	(0.006)
Months unemployed in last 6 months	0.000	(0.007)			0.001	(0.007)
Month out of labor force in last 6 months	-0.002	(0.007)			-0.002	(0.007)
Months employed in last 24 months	-0.001	(0.002)			-0.002	(0.002)
Months unemployed in last 24 months	-0.011***	(0.002)			-0.011***	(0.002)
Months out of labor force in last 24 months	-0.003	(0.002)			-0.003	(0.002)
No. of employers in last 24 months	-0.011	(0.007)			-0.011	(0.007)
No. of unemployment spells in last 24 months	0.016*	(0.009)			0.014	(0.009)
No. of ALMP programs in last 24 months	0.035***	(0.011)			0.033***	(0.011)
No. of out of labor force spells in last 24 months	-0.023*	(0.012)			-0.024*	(0.012)
Last daily income in €	-0.001***	(0.000)			-0.000**	(0.000)
Last job: Full-time employment	-0.057**	(0.025)			-0.058**	(0.024)
Last job: Laid off by employer	0.003	(0.018)			0.003	(0.018)
Long-term labor market history						
Months employed in last 10 years	0.002***	(0.001)			0.002***	(0.001)
Months unemployed in last 10 years	0.000***	(0.000)			0.000***	(0.000)
Months out of labor force in last 10 years	0.002***	(0.001)			0.002***	(0.001)
No. of employers in last 10 years	0.003	(0.003)			0.003	(0.003)
No. of unemployment spells in last 10 years	-0.015***	(0.004)			-0.015***	(0.004)
No. of ALMP programs in last 10 years	0.017**	(0.007)			0.015**	(0.007)
No. of out of labor force spells in last 10 years	0.000	(0.005)			0.002	(0.005)
Time with last employer in 100 days	0.000	(0.001)			0.000	(0.001)
Duration of last unemployment spell in 100 days	0.017	(0.067)			0.026	(0.066)
Months in ALMP programs in last 10 years	0.000	(0.001)			0.000	(0.001)
Observations	6,737		6,737		6,737	
log-Likelihood	-3729.545		-3716.352		-3653.668	
Mean value	0.268		0.268		0.268	
Pseudo-R^2	0.047		0.050		0.066	

Note: Depicted are average marginal effects based on logit models estimating the participation probability. ***/**/* indicate statistically significance at the 1%/5%/10%-level. Standard errors in parenthesis.
$^{a)}$Personality traits are measured with different items on a 7-Point Likert-scale.
$^{b)}$Contacts to neighbors and the childcare situation are measured on a scale from 1 (very good) to 6 (very bad).
$^{c)}$Life satisfaction and expected ALMP probabilities are measured on a scale from 0 (very low) to 10 (very high).

Table 5.9: Propensity Score Estimation: Long-term Training

	Standard		Auxiliary		Extended	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Personality traits$^{a)}$						
Openness (standardized)			-0.009*	(0.005)	-0.008	(0.005)
Conscientiousness (standardized)			0.009*	(0.005)	0.008	(0.005)
Extraversion (standardized)			-0.017***	(0.005)	-0.016***	(0.005)
Neuroticism (standardized)			-0.005	(0.005)	-0.005	(0.005)
Locus of control (standardized)			-0.003	(0.005)	-0.004	(0.005)
Intergenerational variables						
Father has upper sec. school leaving degree			-0.007	(0.014)	-0.005	(0.014)
Father was employed when person aged 15			-0.002	(0.014)	0.001	(0.014)
Father's current age (Ref.: already passed away)						
$\leq$ 60 years			0.002	(0.015)	0.002	(0.015)
$>$ 60 years			0.015	(0.012)	0.013	(0.012)
Father was blue-collar worker when person aged 15			-0.006	(0.011)	-0.007	(0.011)
Social Network$^{b)}$						
No. of good friends outside family: Less than two			0.010	(0.016)	0.008	(0.016)
Contacts to neighbors: Good (1-3)			-0.014	(0.010)	-0.014	(0.010)
Labor Market Flexibility						
Childcare situation: Bad (4-6)			0.005	(0.020)	-0.010	(0.019)
Car-ownership			0.020*	(0.010)	0.018*	(0.010)
Life satisfaction$^{c)}$						
Life satisfaction (Ref.: Medium (4-6))						
Low (0-3)			-0.006	(0.017)	-0.010	(0.017)
High (7-10)			-0.030***	(0.009)	-0.032***	(0.009)
ALMP expectations$^{c)}$						
Expected ALMP probability (Ref.: Medium (4-6))						
Low (0-3)			-0.039***	(0.009)	-0.038***	(0.009)
High (7-10)			0.106***	(0.017)	0.102***	(0.017)
Socio-demographic characteristics						
Female	0.021*	(0.011)	0.030***	(0.011)	0.022*	(0.012)
Age (Ref.: 16-24 years)						
25-34 years	0.047***	(0.017)	0.039**	(0.016)	0.043**	(0.018)
35-44 years	0.079***	(0.022)	0.052***	(0.020)	0.063***	(0.024)
45-55 years	0.090***	(0.025)	0.061***	(0.022)	0.074***	(0.027)
Married or cohabiting	-0.015	(0.013)	-0.002	(0.013)	-0.012	(0.013)
German citizenship	0.031	(0.025)	0.030	(0.025)	0.029	(0.025)
Migration background	0.040**	(0.018)	0.026	(0.017)	0.030*	(0.018)
Children (Ref.: None)						
One child	0.006	(0.013)	0.008	(0.013)	0.005	(0.014)
Two children or more	0.028	(0.017)	0.035**	(0.017)	0.024	(0.018)
School leaving degree (Ref.: None)						
Lower sec. degree	0.032	(0.035)	0.044	(0.036)	0.042	(0.036)
Middle sec. degree	0.041	(0.037)	0.053	(0.038)	0.048	(0.037)
Upper sec. degree	0.034	(0.037)	0.049	(0.038)	0.045	(0.038)
Higher education (Ref.: None)						
Int. or ext. vocational training	-0.012	(0.016)	-0.008	(0.016)	-0.014	(0.016)
University degree	-0.014	(0.020)	-0.009	(0.019)	-0.014	(0.020)
Month of entry into unemployment (Ref.: June)						
July	-0.038*	(0.022)	-0.038*	(0.022)	-0.035	(0.022)
August	-0.016	(0.025)	-0.028	(0.023)	-0.020	(0.024)
September	0.018	(0.029)	0.023	(0.028)	0.024	(0.029)
October	-0.001	(0.025)	-0.004	(0.024)	-0.000	(0.025)
November	0.015	(0.026)	0.003	(0.024)	0.019	(0.026)
December	0.005	(0.028)	-0.005	(0.026)	0.013	(0.028)
January	-0.032	(0.024)	-0.039*	(0.022)	-0.032	(0.023)
February	0.009	(0.028)	0.010	(0.028)	0.008	(0.028)
March	-0.018	(0.025)	-0.006	(0.026)	-0.010	(0.026)
April	-0.003	(0.025)	0.004	(0.026)	0.001	(0.025)
May	0.026	(0.028)	0.034	(0.028)	0.027	(0.027)

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Time between entry into UE and interview (Ref.: 7 weeks)						
8 weeks	0.000	(0.036)	-0.008	(0.035)	-0.014	(0.034)
9 weeks	0.011	(0.038)	-0.005	(0.037)	-0.009	(0.036)
10 weeks	0.022	(0.042)	0.014	(0.041)	0.011	(0.040)
11 weeks	0.003	(0.039)	-0.002	(0.040)	-0.009	(0.038)
12 weeks	-0.014	(0.038)	-0.020	(0.039)	-0.025	(0.038)
13 weeks	-0.017	(0.041)	-0.025	(0.041)	-0.036	(0.039)
14 weeks	-0.014	(0.040)	-0.017	(0.041)	-0.024	(0.040)
Health restriction or disability	-0.030*	(0.016)	-0.038**	(0.016)	-0.030*	(0.017)
Job search (Ref.: Full- or part-time employment)						
Full-time employment only	0.055***	(0.013)	0.052***	(0.012)	0.047***	(0.012)
Part-time employment only	0.006	(0.015)	0.020	(0.016)	0.010	(0.016)
Employment status partner (Ref.: No partner)						
Full-time employed	-0.006	(0.013)	-0.002	(0.013)	-0.000	(0.013)
Part-time employed	-0.042**	(0.018)	-0.036**	(0.018)	-0.035*	(0.019)
Education	-0.011	(0.021)	-0.014	(0.021)	-0.007	(0.021)
Unemployment	0.009	(0.027)	0.006	(0.026)	0.010	(0.026)
Other	-0.010	(0.022)	-0.004	(0.022)	-0.004	(0.022)
Region (Ref.: West-Germany: UE rate 0-3%)						
West-Germany: UE rate 4-6%	-0.060	(0.054)	-0.066	(0.049)	-0.068	(0.048)
West-Germany: UE rate 7-9%	-0.079	(0.049)	-0.075	(0.046)	-0.082*	(0.045)
West-Germany: UE rate $\geq$ 10%	-0.062	(0.054)	-0.055	(0.052)	-0.063	(0.050)
East-Germany: UE rate 9-12%	-0.113***	(0.042)	-0.114***	(0.038)	-0.115***	(0.040)
East-Germany: UE rate 13-14%	-0.084*	(0.050)	-0.080*	(0.047)	-0.078	(0.048)
East-Germany: UE rate 15-16%	-0.078	(0.051)	-0.077	(0.048)	-0.076	(0.049)
East-Germany: UE rate $\geq$ 17%	-0.055	(0.058)	-0.048	(0.056)	-0.050	(0.055)
Short-term labor market history						
Employment status before UE (Ref.: Regular employed)						
Subsidized employed	-0.030*	(0.017)			-0.029*	(0.017)
School, apprentice, military, etc.	-0.007	(0.018)			-0.008	(0.018)
Parental leave	0.106***	(0.035)			0.108***	(0.035)
Months employed in last 6 months	-0.007	(0.005)			-0.007	(0.005)
Months unemployed in last 6 months	0.005	(0.006)			0.004	(0.006)
Month out of labor force in last 6 months	-0.006	(0.006)			-0.008	(0.006)
Months employed in last 24 months	0.001	(0.001)			0.000	(0.002)
Months unemployed in last 24 months	-0.006***	(0.002)			-0.005***	(0.002)
Months out of labor force in last 24 months	0.000	(0.002)			0.000	(0.002)
No. of employers in last 24 months	-0.002	(0.006)			-0.001	(0.006)
No. of unemployment spells in last 24 months	0.007	(0.008)			0.005	(0.008)
No. of ALMP programs in last 24 months	0.035***	(0.009)			0.034***	(0.009)
No. of out of labor force spells in last 24 months	-0.025**	(0.011)			-0.029***	(0.011)
Last daily income in €	-0.000	(0.000)			0.000	(0.000)
Last job: Full-time employment	0.005	(0.023)			0.002	(0.023)
Last job: Laid off by employer	0.013	(0.015)			0.012	(0.015)
Long-term labor market history						
Months employed in last 10 years	-0.000	(0.000)			0.000	(0.000)
Months unemployed in last 10 years	-0.000*	(0.000)			-0.000	(0.000)
Months out of labor force in last 10 years	-0.000	(0.000)			-0.000	(0.000)
No. of employers in last 10 years	0.001	(0.003)			0.000	(0.003)
No. of unemployment spells in last 10 years	-0.008**	(0.004)			-0.009**	(0.004)
No. of ALMP programs in last 10 years	0.005	(0.006)			0.004	(0.006)
No. of out of labor force spells in last 10 years	-0.003	(0.005)			-0.001	(0.005)
Time with last employer in 100 days	0.001	(0.001)			0.001	(0.001)
Duration of last unemployment spell in 100 days	0.071	(0.053)			0.085	(0.053)
Months in ALMP programs in last 10 years	0.001	(0.001)			0.001	(0.001)
Observations	5,717		5,717		5,717	
log-Likelihood	-2144.481		-2099.245		-2040.801	
Mean value	0.137		0.137		0.137	
Pseudo-R^2	0.061		0.081		0.106	

Note: Depicted are average marginal effects based on logit models estimating the participation probability. ***/**/* indicate statistically significance at the 1%/5%/10%-level. Standard errors in parenthesis.
$^{a)}$Personality traits are measured with different items on a 7-Point Likert-scale.
$^{b)}$Contacts to neighbors and the childcare situation are measured on a scale from 1 (very good) to 6 (very bad).
$^{c)}$Life satisfaction and expected ALMP probabilities are measured on a scale from 0 (very low) to 10 (very high).

Table 5.10: Propensity Score Estimation: Wage Subsidies

	Standard		Auxiliary		Extended	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Personality traits$^{a)}$						
Openness (standardized)			0.003	(0.005)	0.006	(0.005)
Conscientiousness (standardized)			0.008	(0.005)	0.007	(0.005)
Extraversion (standardized)			-0.005	(0.005)	-0.005	(0.005)
Neuroticism (standardized)			-0.007	(0.004)	-0.007	(0.004)
Locus of control (standardized)			-0.003	(0.005)	-0.003	(0.005)
Intergenerational variables						
Father has upper sec. school leaving degree			-0.014	(0.013)	-0.012	(0.013)
Father was employed when person aged 15			0.012	(0.013)	0.014	(0.013)
Father's current age (Ref.: already passed away)						
$\leq$ 60 years			-0.004	(0.014)	-0.003	(0.015)
$>$ 60 years			-0.016	(0.010)	-0.018*	(0.010)
Father was blue-collar worker when person aged 15			0.000	(0.010)	0.000	(0.010)
Social network$^{b)}$						
No. of good friends outside family: Less than two			-0.010	(0.013)	-0.009	(0.013)
Contacts to neighbors: Good (1-3)			0.000	(0.010)	0.002	(0.010)
Labor market flexibility						
Childcare situation: Bad (4-6)			-0.012	(0.018)	-0.018	(0.017)
Car-ownership			0.022**	(0.010)	0.018*	(0.010)
Life satisfaction$^{c)}$						
Life satisfaction (Ref.: Medium (4-6))						
Low (0-3)			0.011	(0.016)	0.008	(0.016)
High (7-10)			-0.026***	(0.008)	-0.026***	(0.008)
ALMP expectations$^{c)}$						
Expected ALMP probability (Ref.: Medium (4-6))						
Low (0-3)			-0.024***	(0.009)	-0.019**	(0.009)
High (7-10)			0.020	(0.013)	0.021*	(0.013)
Socio-demographic characteristics						
Female	0.003	(0.009)	0.013	(0.010)	0.005	(0.010)
Age (Ref.: 16-24 years)						
25-34 years	0.013	(0.014)	0.007	(0.012)	0.013	(0.014)
35-44 years	0.031*	(0.018)	0.019	(0.017)	0.032	(0.021)
45-55 years	0.086***	(0.025)	0.067***	(0.024)	0.077***	(0.029)
Married or cohabiting	-0.012	(0.011)	-0.009	(0.011)	-0.011	(0.011)
German citizenship	0.003	(0.024)	0.003	(0.024)	0.001	(0.024)
Migration background	0.012	(0.017)	0.012	(0.017)	0.015	(0.017)
Children (Ref.: None)						
One child	0.004	(0.011)	0.006	(0.012)	0.006	(0.012)
Two children or more	0.028*	(0.016)	0.032*	(0.017)	0.030*	(0.017)
School leaving degree (Ref.: None)						
Lower sec. degree	-0.039	(0.026)	-0.039	(0.026)	-0.038	(0.026)
Middle sec. degree	-0.047*	(0.024)	-0.046*	(0.024)	-0.046*	(0.025)
Upper sec. degree	-0.050**	(0.025)	-0.054**	(0.024)	-0.046*	(0.026)
Higher education (Ref.: None)						
Int. or ext. vocational training	0.021	(0.017)	0.026	(0.017)	0.020	(0.017)
University degree	0.010	(0.019)	0.007	(0.018)	0.011	(0.019)
Month of entry into unemployment (Ref.: June)						
July	-0.034	(0.022)	-0.027	(0.022)	-0.038*	(0.022)
August	-0.008	(0.026)	-0.000	(0.026)	-0.010	(0.026)
September	-0.014	(0.025)	-0.005	(0.025)	-0.012	(0.026)
October	-0.009	(0.024)	0.001	(0.024)	-0.007	(0.025)
November	-0.035*	(0.019)	-0.033*	(0.018)	-0.037*	(0.020)
December	-0.033	(0.023)	-0.035*	(0.020)	-0.034	(0.023)
January	-0.062***	(0.018)	-0.060***	(0.017)	-0.065***	(0.018)
February	-0.013	(0.025)	-0.003	(0.026)	-0.015	(0.026)
March	-0.015	(0.025)	0.002	(0.027)	-0.014	(0.026)
April	-0.045**	(0.018)	-0.038**	(0.019)	-0.049***	(0.018)
May	-0.014	(0.024)	0.000	(0.025)	-0.015	(0.024)

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Time between entry into UE and interview (Ref.: 7 weeks)						
8 weeks	-0.022	(0.026)	-0.034	(0.027)	-0.029	(0.026)
9 weeks	0.004	(0.033)	-0.010	(0.033)	-0.003	(0.032)
10 weeks	-0.001	(0.033)	-0.019	(0.032)	-0.008	(0.033)
11 weeks	-0.008	(0.032)	-0.019	(0.034)	-0.016	(0.032)
12 weeks	0.026	(0.043)	0.013	(0.045)	0.019	(0.043)
13 weeks	-0.029	(0.032)	-0.040	(0.033)	-0.040	(0.031)
14 weeks	-0.019	(0.032)	-0.029	(0.034)	-0.027	(0.032)
Health restriction or disability	-0.048***	(0.012)	-0.053***	(0.011)	-0.048***	(0.013)
Job search (Ref.: Full- or part-time employment)						
Full-time employment only	0.060***	(0.012)	0.060***	(0.012)	0.056***	(0.012)
Part-time employment only	0.012	(0.014)	0.021	(0.014)	0.017	(0.015)
Employment status partner (Ref.: No partner)						
Full-time employed	0.012	(0.012)	0.013	(0.012)	0.015	(0.012)
Part-time employed	0.006	(0.018)	0.009	(0.018)	0.007	(0.018)
Education	0.015	(0.020)	0.014	(0.020)	0.019	(0.021)
Unemployment	0.002	(0.023)	-0.002	(0.022)	0.003	(0.023)
Other	-0.022	(0.015)	-0.021	(0.015)	-0.021	(0.016)
Region (Ref.: West-Germany: UE rate 0-3%)						
West-Germany: UE rate 4-6%	-0.086**	(0.036)	-0.078**	(0.035)	-0.090**	(0.036)
West-Germany: UE rate 7-9%	-0.086**	(0.036)	-0.072*	(0.037)	-0.089**	(0.037)
West-Germany: UE rate $\geq$ 10%	-0.081**	(0.038)	-0.068*	(0.039)	-0.084**	(0.039)
East-Germany: UE rate 9-12%	-0.071*	(0.043)	-0.059	(0.044)	-0.072	(0.045)
East-Germany: UE rate 13-14%	-0.058	(0.046)	-0.044	(0.047)	-0.061	(0.048)
East-Germany: UE rate 15-16%	-0.066	(0.043)	-0.048	(0.046)	-0.066	(0.045)
East-Germany: UE rate $\geq$ 17%	-0.049	(0.049)	-0.033	(0.051)	-0.046	(0.052)
Short-term labor market history						
Employment status before UE (Ref.: Regular employed)						
Subsidized employed	0.019	(0.017)			0.020	(0.017)
School, apprentice, military, etc.	0.007	(0.017)			0.008	(0.017)
Parental leave	0.033	(0.033)			0.038	(0.034)
Months employed in last 6 months	-0.000	(0.005)			-0.000	(0.005)
Months unemployed in last 6 months	0.004	(0.005)			0.004	(0.005)
Month out of labor force in last 6 months	-0.003	(0.006)			-0.004	(0.006)
Months employed in last 24 months	-0.001	(0.001)			-0.001	(0.001)
Months unemployed in last 24 months	-0.005***	(0.002)			-0.005***	(0.002)
Months out of labor force in last 24 months	-0.001	(0.002)			-0.001	(0.002)
No. of employers in last 24 months	-0.005	(0.005)			-0.006	(0.005)
No. of unemployment spells in last 24 months	0.015**	(0.007)			0.015**	(0.007)
No. of ALMP programs in last 24 months	0.018**	(0.008)			0.016**	(0.008)
No. of out of labor force spells in last 24 months	-0.020*	(0.010)			-0.022**	(0.011)
Last daily income in €	-0.001***	(0.000)			-0.001***	(0.000)
Last job: Full-time employment	-0.003	(0.020)			-0.002	(0.020)
Last job: Laid off by employer	-0.011	(0.013)			-0.009	(0.013)
Long-term labor market history						
Months employed in last 10 years	0.001**	(0.000)			0.001**	(0.000)
Months unemployed in last 10 years	-0.000	(0.000)			-0.000	(0.000)
Months out of labor force in last 10 years	0.001	(0.000)			0.001	(0.000)
No. of employers in last 10 years	0.007***	(0.002)			0.007***	(0.002)
No. of unemployment spells in last 10 years	-0.009***	(0.003)			-0.010***	(0.003)
No. of ALMP programs in last 10 years	0.012**	(0.005)			0.012**	(0.005)
No. of out of labor force spells in last 10 years	-0.004	(0.004)			-0.003	(0.004)
Time with last employer in 100 days	-0.001	(0.001)			-0.002	(0.001)
Duration of last unemployment spell in 100 days	0.027	(0.045)			0.035	(0.045)
Months in ALMP programs in last 10 years	0.000	(0.001)			0.000	(0.001)
Observations	5,476		5,476		5,476	
log-Likelihood	-1614.328		-1654.380		-1592.547	
Mean value	0.099		0.099		0.099	
Pseudo-R^2	0.087		0.064		0.099	

Note: Depicted are average marginal effects based on logit models estimating the participation probability. ***/**/* indicate statistically significance at the 1%/5%/10%-level. Standard errors in parenthesis.
a) Personality traits are measured with different items on a 7-Point Likert-scale.
b) Contacts to neighbors and the childcare situation are measured on a scale from 1 (very good) to 6 (very bad).
c) Life satisfaction and expected ALMP probabilities are measured on a scale from 0 (very low) to 10 (very high).

Table 5.11: Consequences for the Average Treatment Effects on the Treated (ATT) for Different Matching/Weighting Estimators - Short-term Training

	Unconditional raw difference	Propensity score specification Standard (1)	Auxiliary (2)	Extended (3)
Regular employed 30 months after entry (t+30)				
Regression	0.008	-0.007	-0.001	-0.005
	(0.014)	(0.013)	(0.013)	(0.013)
Nearest Neighbor (1:1)		-0.031	-0.007	0.019
		(0.022)	(0.022)	(0.022)
Radius 1 (caliper=0.1)		-0.0002	-0.002	-0.003
		(0.013)	(0.013)	(0.013)
Radius 2 (regression adjustment)		-0.026	-0.009	0.006
		(0.020)	(0.020)	(0.020)
Inverse Probability Weighting		-0.004	0.0005	-0.003
		(0.013)	(0.013)	(0.014)
Kernel 1 (bw=0.02)		-0.004	-0.002	-0.003
		(0.013)	(0.013)	(0.014)
Kernel 2 (bw=0.06)		-0.004	-0.002	-0.004
		(0.013)	(0.013)	(0.014)
Kernel 3 (bw=0.2)		-0.0001	0.002	-0.001
		(0.013)	(0.013)	(0.013)
Cumulated earnings in € up to t+30				
Regression	-2,416***	-2,231***	-1,831***	-1,945***
	(647)	(555)	(526)	(535)
Nearest Neighbor (1:1)		-2,312**	-1,570	-1,482
		(988)	(995)	(990)
Radius 1 (caliper=0.1)		-2,015***	-2,143***	-1,996***
		(520)	(540)	(522)
Radius 2 (regression adjustment)		-2,213***	-1,908**	-1,617*
		(827)		(834) (841)
Inverse Probability Weighting		-2,105***	-1,787***	-1,874***
		(550)	(523)	(528)
Kernel 1 (bw=0.02)		-2,118***	-1,879***	-1,840***
		(552)	(535)	(548)
Kernel 2 (bw=0.06)		-2,129***	-1,905***	-1,886***
		(548)	(527)	(531)
Kernel 3 (bw=0.2)		-2,198***	-2,161***	-2,097***
		(534)	(519)	(515)
No. of observations	6,737	6,737	6,737	6,737
Propensity score specification				
Personality traits			✓	✓
Inter-generational variables			✓	✓
Social network			✓	✓
Labor market flexibility			✓	✓
Life satisfaction			✓	✓
ALMP expectations			✓	✓
Socio-demographic characteristics		✓	✓	✓
Labor market history		✓		✓

Note: Depicted are estimated average treatment effects on the treated as the difference in mean outcomes between participants and matched non-participants using, OLS regression, inverse probability weighting (IPW), one-to-one nearest neighbor matching, radius with a caliper of 0.1, respectively regression adjustment (see Huber et al., 2015) and epanechnikov kernel propensity score matching with bandwidths 0.02, 0.06 and 0.2. Standard errors are in parentheses and based on bootstrapping with 999 replications. ***/**/* indicate statistically significance at the 1/5/10%-level.

Table 5.12: Consequences for the Average Treatment Effects on the Treated (ATT) for Different Matching/Weighting Estimators - Long-term Training

	Unconditional raw difference	Propensity score specification		
		Standard (1)	Auxiliary (2)	Extended (3)
Regular employed 30 months after entry (t+30)				
Regression	0.006	-0.027	-0.010	-0.021
	(0.019)	(0.021)	(0.021)	(0.021)
Nearest Neighbor (1:1)		-0.001	-0.036	0.003
		(0.032)	(0.033)	(0.034)
Radius 1 (caliper=0.1)		-0.006	-0.020	-0.014
		(0.020)	(0.021)	(0.021)
Radius 2 (regression adjustment)		-0.008	-0.036	-0.0005
		(0.028)	(0.029)	(0.030)
Inverse Probability Weighting		-0.026	-0.010	-0.018
		(0.021)	(0.021)	(0.021)
Kernel 1 (bw=0.02)		-0.027	-0.009	-0.019
		(0.022)	(0.021)	(0.022)
Kernel 2 (bw=0.06)		-0.025	-0.009	-0.017
		(0.021)	(0.021)	(0.022)
Kernel 3 (bw=0.2)		-0.012	-0.002	-0.010
		(0.02)	(0.02)	(0.02)
Cumulated earnings in € up to t+30				
Regression	-3,876***	-5,763***	-5,226***	-5,627***
	(938)	(829)	(804)	(823)
Nearest Neighbor (1:1)		-4,357***	-5,807***	-4,638***
		(1,556)	(1,595)	(1,637)
Radius 1 (caliper=0.1)		-4,961***	-5,298***	-5336***
		(790)	(814)	(827)
Radius 2 (regression adjustment)		-4,939***	-5,700***	-4,560***
		(1,375)	(1,367)	(1,352)
Inverse Probability Weighting		-5,794***	-5,406***	-5,761***
		(844)	(815)	(846)
Kernel 1 (bw=0.02)		-5,788***	-5,282***	-5,610***
		(877)	(855)	(896)
Kernel 2 (bw=0.06)		-5,656***	-5,280***	-5,618***
		(845)	(823)	(859)
Kernel 3 (bw=0.2)		-4,826***	-4,566***	-4,989***
		(795)	(784)	(797)
No. of observations	5,717	5,717	5,717	5,717
Propensity score specification				
Personality traits			✓	✓
Inter-generational variables			✓	✓
Social network			✓	✓
Labor market flexibility			✓	✓
Life satisfaction			✓	✓
ALMP expectations			✓	✓
Socio-demographic characteristics		✓	✓	✓
Labor market history		✓		✓

Note: Depicted are estimated average treatment effects on the treated as the difference in mean outcomes between participants and matched non-participants using, OLS regression, inverse probability weighting (IPW), one-to-one nearest neighbor matching, radius with a caliper of 0.1, respectively regression adjustment (see Huber et al., 2015) and epanechnikov kernel propensity score matching with bandwidths 0.02, 0.06 and 0.2. Standard errors are in parentheses and based on bootstrapping with 999 replications. ***/**/* indicate statistically significance at the 1/5/10%-level.

Table 5.13: Consequences for the Average Treatment Effects on the Treated (ATT) for Different Matching/Weighting Estimators - Wage Subsidies

	Unconditional raw difference	Propensity score specification Standard (1)	Auxiliary (2)	Extended (3)
Regular employed 30 months after entry (t+30)				
Regression	0.128*** (0.022)	0.092*** (0.022)	0.093*** (0.022)	0.085*** (0.022)
Nearest Neighbor (1:1)		0.113*** (0.037)	0.096** (0.038)	0.092** (0.04)
Radius 1 (caliper=0.1)		0.105*** (0.021)	0.111*** (0.021)	0.098*** (0.022)
Radius 2 (regression adjustment)		0.090*** (0.032)	0.104*** (0.032)	0.087*** (0.032)
Inverse Probability Weighting		0.096*** (0.022)	0.09*** (0.022)	0.082*** (0.022)
Kernel 1 (bw=0.02)		0.097*** (0.022)	0.087*** (0.023)	0.084*** (0.023)
Kernel 2 (bw=0.06)		0.100*** (0.022)	0.095*** (0.022)	0.087*** (0.023)
Kernel 3 (bw=0.2)		0.119*** (0.021)	0.114*** (0.021)	0.109*** (0.021)
Cumulated earnings in € up to t+30				
Regression	4,783*** (1,105)	3,386*** (833)	3,861*** (787)	3,670*** (810)
Nearest Neighbor (1:1)		4,450*** (1,634)	3,528** (1,769)	3,966** (1,703)
Radius 1 (caliper=0.1)		4,168*** (772)	4,129*** (801)	4024*** (800)
Radius 2 (regression adjustment)		3,708*** (1,409)	3,632** (1,477)	3,647*** (1,450)
Inverse Probability Weighting		3,460*** (838)	3,769*** (797)	3,522*** (831)
Kernel 1 (bw=0.02)		3,749*** (852)	3,677*** (844)	3,604*** (883)
Kernel 2 (bw=0.06)		3,723*** (830)	3,924*** (805)	3,691*** (839)
Kernel 3 (bw=0.2)		4,450*** (801)	4,359*** (771)	4,245*** (785)
No. of observations	5,476	5,476	5,476	5,476
Propensity score specification				
Personality traits			✓	✓
Inter-generational variables			✓	✓
Social network			✓	✓
Labor market flexibility			✓	✓
Life satisfaction			✓	✓
ALMP expectations			✓	✓
Socio-demographic characteristics		✓	✓	✓
Labor market history		✓		✓

Note: Depicted are estimated average treatment effects on the treated as the difference in mean outcomes between participants and matched non-participants using, OLS regression, inverse probability weighting (IPW), one-to-one nearest neighbor matching, radius with a caliper of 0.1, respectively regression adjustment (see Huber et al., 2015) and epanechnikov kernel propensity score matching with bandwidths 0.02, 0.06 and 0.2. Standard errors are in parentheses and based on bootstrapping with 999 replications. ***/**/* indicate statistically significance at the 1/5/10%-level.

6 The Gender Wage Gap and the Role of Reservation Wages

. *This chapter examines the importance of differences in reservation wages for the gender wage gap. Based on two waves of rich survey data for a sample of newly unemployed individuals in Germany, we perform a decomposition analysis including measures for reservation wages, detailed information on education, socio-demographics, labor market history, as well as personality traits. In order to address the potential endogeneity of reservation wages we exploit a generated instrumental variable strategy that relies on heteroscedasticity of the error terms. Our findings indicate that the gender wage gap becomes small and statistically insignificant once we control for reservation wages. Moreover, we perform a subgroup analysis that provides valuable insights about the importance of potentially unobserved characteristics that affect reservation wages and realized wages simultaneously. Reasons for differences in reservation wages could arise from productivity differences, the fact that women anticipate discrimination and different unobserved traits or preferences.*[82]

[82]This chapter is based on joint work with Marco Caliendo and Wang-Sheng Lee (Caliendo et al., 2017).

6.1 Introduction

The decomposition of gender and racial wage gaps can arguably be considered to be the Holy Grail in labor economics. In the case of the gender wage gap, despite numerous attempts by economists in the past, there typically still remains a sizeable unexplained gap (e.g. Altonji and Blank, 1999; Blau and Kahn, 2006). Early studies already identified the institutional wage structure (e.g. Blau and Kahn, 2003), gender differences in experience and tenure (e.g. Blau and Kahn, 1997), occupations (e.g. Groshen, 1991; Macpherson and Hirsch, 1995), qualifications (e.g. Blau and Kahn, 1997), college major (e.g Brown and Corcoran, 1997; Machin and Puhani, 2003), promotion rates (e.g. Booth et al., 2003) and the penalty on women for having children (e.g. Waldfogel, 1997) as driving forces of the gender wage gap. In more recent years, new classes of explanations why women may choose alternative career paths have been proposed (see the discussion in Bertrand, 2010). These include gender differences in psychological attributes and risk preferences (e.g. Croson and Gneezy, 2009), attitudes towards competition (e.g. Lavy, 2013; Manning and Saidi, 2010) and negotiation (e.g. Babcock and Lascheyer, 2003), as well as differences in personality traits (e.g. Mueller and Plug, 2006). However, to date, most of these recent findings have been based on laboratory experiments and real world evidence is generally lacking. Therefore, more empirical evidence will be important in determining whether these explanations will have a lasting impact in the study of gender wage gaps (see Bertrand, 2010).

Closely related to the gender gap in realized wages, another strand of the literature provides explanations for gender differences in reservation wages. The reservation wage can be viewed as a measure of a person's eagerness or reluctance to accept employment and plays a key role in traditional job search theory (see Mortensen, 1986; Mortensen and Neumann, 1988) by determining the unemployment duration and the speed at which job-seekers will be reintegrated into the labor market (e.g. Rogerson et al., 2005). Gender differences in reservation wages might be related to different preferences for non-working time (e.g. Bowlus, 1997; Bowlus and Grogan, 2009), search frictions (e.g. Bowlus, 1997; Sulis, 2011; Kunze and Troske, 2012) and differences in productivity (e.g. Flabbi, 2010). Moreover, the wage gap can also emerge because heterogeneous firms can have different pay policies and offer different wages to men and women (e.g. Becker, 1971; Blackaby et al., 2005; Flabbi, 2010). Women could potentially anticipate such discriminatory behavior and hence adjust their reservation wages downwards to increase their future employment prospects. It is therefore possible that gender differences with respect to reservation wages might be simply a realization of anticipated discrimination against women in the labor market. Finally, differences in reservation wages could also express different preferences or personality traits, like the tendency for males to be overconfident (see Barber and Odean, 2001), the fact that women generally tend to be more risk averse (Eckel and Grossmann, 2008; Pannenberg, 2010) or women's preferences for occupations with higher social prestige (e.g. Kleinjans and Fullerton, 2013) and workplace flexibility (e.g. Goldin, 2014).

This chapter combines these two strands of the literature in order to search for new explanations for the gender wage gap. We do so by examining the importance of gender differences in reservation wages in explaining the gender gap in realized wages for a sample of newly unemployed job applicants in Germany. The key research question we focus on is if any observed wage gap between men and women is simply an empirical realization of an initial gender gap in reservation wages. In particular, the novel contribution of the chapter is including the reservation wage into the decomposition of the gender gap in realized wages. By having data on both reservation wages and realized wages on the same individual in a panel data set, we can determine the extent to which gender differences in aspirations and expectations regarding wages can be a self-fulfilling prophecy and lead to gender differences in actual wages. Although there has been previous work that attempts to decompose gender wage differentials that accounts for gender differences in reservation wages (e.g. Bowlus, 1997; Bowlus and Grogan, 2009), most studies do not have actual information on reservation wages and must infer them from observed outcomes in the data, such as the lowest observed wage. Previous empirical work involving reservation wages has generally been concerned with macro-labor issues such as unemployment insurance and unemployment rates (e.g. Feldstein and Poterba, 1984; Shimer and Werning, 2007). Others have been concerned with estimating the determinants of reservation wages, e.g., Brown et al. (2010) use the BHPS data to examine the role of health in determining reservation wages and similarly, Prasad (2004) and Humpert and Pfeifer (2013) use data from the German Socio-Economic Panel (SOEP) to analyze the determinants of reservation wages of German workers.

Having access to panel data on reservation wages and realized wages for the same individual comes at the price that we can only draw conclusions for a specific sample of job-seekers entering unemployment shortly before they were interviewed for the first time but found a job within one year. Although this might raise concerns about the external validity of our results, it should be noted that this allows us to focus on a very homogeneous sample of unemployed job-seekers, which is probably the most relevant group when utilizing the concept of reservation wages. Nevertheless, it is possible that there are unobserved differences between men and women that influence reservation wages and realized wages simultaneously. For example, if women value job flexibility more than men, they may report a lower reservation wage and subsequently choose to accept a job with lower wages that allows for flexible hours. We conduct two types of sensitivity analysis – one based on an instrumental variable strategy, the other based on a subgroup analysis – indicating that potential endogeneity of reservation wages only has a minor impact on our decomposition results.

Previewing our main findings, we find as is typical in the literature that men earn more than women. Although, the inclusion of standard explanatory variables reduces the gender gap in realized wages somewhat, the gap still remains statistically significant. In this context, labor market histories appear to be an important driving factor of the gender wage gap, while socio-demographic characteristics, personality traits, search behavior and expectations have only a small impact. However, the striking result implies that the inclusion of reservation wages halves the gender gap, making the remaining difference economically small and statistically insignificant. As the finding implies that reservation wages play an important role for the gender gap in realized wages, we also take a closer look at the determinants of reservation wages in an attempt to better understand how this initial gender gap in reservation wages arises. The rest of this chapter is organized as follows. Section 6.2 describes the data in more detail and shows observed differences between men and women. Section 6.3 presents the decomposition of the realized gender wage gap and discusses the role of reservations wages, while Section 6.4 investigates potential explanations for gender differences in reservation wages. Finally, Section 6.5 concludes.

6.2 Data, Descriptive Statistics and the Reservation Wage

6.2.1 The IZA Evaluation Dataset S(urvey)

This chapter is based on the IZA Evaluation Dataset S(urvey) which contains survey information on individuals who entered unemployment between June 2007 and May 2008 in Germany (see Caliendo et. al, 2011, for details). The initial dataset contains a 9% random sample from the monthly unemployment inflows identified in the administrative records who are selected for an interview. From this gross sample of individuals aged between 16 and 54 years, representative samples of about 1,450 individuals are interviewed each month so that twelve monthly cohorts are gathered after one year. The first wave of interviews takes place shortly after the entry into unemployment. Besides the extensive set of individual-level characteristics and labor market outcomes, the individuals are asked a variety of non-standard questions regarding search behavior, social networks, psychological factors, cognitive and non-cognitive skills, subjective assessments on future outcomes, and attitudes. Altogether, a total of 17,396 interviews were obtained in this initial round of the survey. One year later, 8,915 individuals were interviewed again for a second wave (see Arni et al., 2014, for details on the representativeness of the dataset and panel attrition).

For the analysis we restrict the sample to individuals who are still unemployed at the moment of the first interview, taking place between 7 and 14 weeks after the entry into unemployment, and are actively searching for full-time employment. This means that we exclude all individuals who do not actively search for a new job (since we only observe reservation wages for those who do) or contemplate part-time employment (which are nearly exclusively women). We expect that women who search for full-time employment are more similar to men (who also search for full-time employment) with respect to unobserved characteristics and potential selection issues are less likely to bias our results compared to other gender wage gap studies. As we observe a few implausible values for some job seekers, we further exclude those individuals whose reported hourly reservation wages and benefit levels are in the lowest or highest percentile of the distribution in order to get rid of these outliers, as well as those with missing values for reservation wages or any of the control variables. Hence, our main estimation sample is based on 1,974 individuals (1,235 men and 739 women) who are employed in wave 2.

6.2.2 Observed Gender Differences and the Reservation Wage

Table 6.1 summarizes gender differences in realized wages, reservation wages and individual characteristics. As is typical in the literature, we observe that women earn 1.35 € per hour less than men one year after entering unemployment. Since previous studies already identified several driving factors of the gender wage gap, we divide our set of control variables into five groups that are expected to represent different classes of explanations for the gender wage gap: 1) *baseline variables* mainly account for differences in socio-demographic characteristics, 2) *personality traits* reflect psychological reasons for why women might choose different career paths, 3) *education* and 4) *labor market histories* represent traditional explanations for differences with respect to human capital accumulation, while 5) *expectations and search characteristics* are related to actual choices that women might take during the current unemployment spell. Selected descriptive statistics for the four groups of control variables are presented in Panel A of Table 6.1. All variables are measured at the first interview shortly after entry into unemployment.

First of all, there are no gender differences with respect to age, migration background or marital status. However, we observe a lower share of women with children than men. For example, 76.7% of the women are without children whereas this is the case for only 69.6% of the men. Similarly, only 6.6% of the women have two or more children, as compared to 12.5% of the men. These differences are likely to be due to our focus on individuals who are searching for full-time employment only. With respect to the 'Big Five' personality traits (see Digman, 1990, for an overview) women report significantly higher levels of openness, conscientiousness, extraversion and neuroticism, while they also have a lower internal locus of control (see e.g. Caliendo et al., 2015). Moreover, the educational variables include information on the school leaving degree and the type of vocational training. In our sample, women generally have higher school leaving degrees than men. For example, 37.6% of women hold a (specialized) upper secondary school degree, whereas only 26.5% of men do.

The fourth group of control variables summarizes the individual labor market history using several measures, such as the employment status before entering unemployment and the time spent in employment in the past. Generally, women are less likely to enter unemployment from regular jobs (69.8% of women compared to 73.7% of men) and have less work experience relative to their age.[83] Furthermore, we observe significantly higher unemployment benefits for men than for women, but no significant gender differences in unemployment benefit receipt. Finally, the last group of covariates contains some non-standard information on job search behavior and expectations. There are no reported differences with respect to job search intensity and the search channels used but men seem to be more optimistic about their future employment prospects and are less likely to expect to participate in an ALMP program.

At the same time, we also observe a gender gap in reservation wages of about 1.05 € per hour. The reservation wage is defined as the lowest wage rate at which a job-seeker would accept a job offer. We measure an individual's reservation wage in several steps. First, individuals are asked for their expected monthly income in a prospective job and how many hours they expect to work at such a job per week. The hourly reservation wage is then defined as the ratio of the expected income divided by 4.33 and the expected weekly working hours. Second, individuals are also asked if they are willing to work for less than the expected wage. If so, they are asked for the minimum amount they would be willing to work for and the expected weekly hours of work. For all individuals who are willing to work for less than the expected wage, we replace the reservation wage by this minimum wage if it is lower than the expected wage defined before.[84] Panel B of Table 6.1 presents means of the generation process of the reservation wages and realized wages separated by gender. Women expect a significant lower monthly income but at the same time also want to work fewer hours per week for such a job. Figure 6.1a presents the distribution of reservation wages in wave 1 by gender, and Figure 6.1b graphs the distribution of realized wages in wave 2 for the same sample. It is interesting to note that the graphs are somewhat similar in that in both cases, the female distribution is to the left of the male distribution and, according to the Kolmogorov-Smirnov test, the differences of the two distributions are statistically significant. Figure 6.1c presents the relative distribution of reservation wages and previous wages and 6.1d the relative distribution of realized wages and reservation wages. As can be seen from the corresponding Kolmogorov-Smirnov tests, there are no distributional differences with respect to these two ratios. The graphs suggest that the

[83] This is measured by the months in employment standardized by an individual's age minus 18.

[84] This is similarly defined as the ratio of the minimum monthly income and the expected working hours divided by 4.33.

Table 6.1: Selected Descriptive Statistics by Gender

	Men	Women	*P*-Value
No. of observations	1,235	739	
Hourly realized wage in wave 2 in €	9.28	7.93	0.000
A. Observed individual-level characteristics			
1) Baseline variables			
Living in West-Germany	0.671	0.655	0.458
Migration background	0.121	0.116	0.736
Age in years	36.53	36.21	0.505
Married (or cohabiting)	0.386	0.318	0.002
Children			
One child	0.180	0.166	0.451
Two (or more) children	0.125	0.066	0.000
Local UE Rate at Interview			
below 5%	0.168	0.185	0.337
15+%	0.111	0.115	0.781
2) Personality traits			
Openness[a]	4.995	5.104	0.043
Conscientiousness[a]	6.196	6.383	0.000
Extraversion[a]	5.136	5.287	0.002
Neuroticism[a]	3.498	3.853	0.000
Locus of control[a]	5.138	5.047	0.009
Life satisfaction: High (7-10)[b]	0.517	0.533	0.498
3)Education			
School leaving degree			
Lower secondary school	0.319	0.165	0.000
Middle secondary school	0.399	0.447	0.039
Specialized upper secondary school	0.265	0.376	0.000
Vocational training			
Internal or external professional training, others	0.677	0.682	0.815
Technical college or university degree	0.272	0.249	0.260
4)Labor market history			
Unemployment benefit recipient	0.824	0.829	0.768
Level of unemployment benefits in €	693.77	573.09	0.000
Lifetime months in employment (div. by age-18)	9.315	7.681	0.000
Employment status before unemployment			
Regular Employed	0.737	0.698	0.064
Subsidized employment	0.078	0.055	0.060
School, apprentice, military, etc.	0.125	0.168	0.008
5) Job search & expectations			
Number of own job applications	16.42	16.63	0.813
Applied for vacancies for which you would have to move	0.342	0.299	0.050
Job search by contacting friends, acquaintances, family, etc.	0.857	0.848	0.617
Expected ALMP probability: High (7-10)[b]	0.319	0.359	0.071
Expected employment probability: Very probable	0.577	0.482	0.000
B. Soliciting the reservation wage			
Hourly reservation wage in wave 1 in €	8.05	7.00	0.000
Step 1:			
Expected monthly net income in €	1,668.90	1,377.34	0.000
Expected weekly hours of work	42.46	40.23	0.000
Step 2:			
Willing to work for less than expected wage	0.742	0.743	0.952
Monthly minimum net income in €[c]	1,390.76	1,115.47	0.000
Expected weekly hours of work for min. income[c]	40.09	37.91	0.000
Accepting a wage below the reservation wage	0.336	0.317	0.375
Difference between reservation wage and accepted wage	1.23	0.93	0.406

Note: All numbers are shares unless indicated otherwise. Variables are measured at entry into unemployment. p-values are based on t-tests on mean equality. The full list of explanatory variables is depicted in the notes of Table 6.2.
[a] Openness, conscientiousness, extraversion, neuroticism and locus of control are measured with different items on a 7-Point Likert-Scale.
[b] Life satisfaction and expected ALMP probabilities are measured on a 0-10 scale increasing from low to high and categorized into three groups.
[c] Observed for those individuals who are willing to work for less than the expected income, i.e. 916 men and 549 women.

relative development from pre-unemployment earnings through reservation wages to actual realized wages in a new job is similar for men and women. However, a potential concern for our analysis might be that reservation wages are only observed at the first interview while actual wages are realized about one year later. Therefore, we need to assume that reservation wages do not change differently for men and women over the course of time. In order to test this assumption, we exploit the fact that the dataset provides information on the development of reservation wages over time for those individuals who are unemployed in more than one of the survey waves. The findings suggest that reservation wages are relatively constant during the first year of the unemployment spell and that there are no significant differences between men and women with respect to the adjustment of reservation wages over time (see Appendix 6.6 for details).

6.3 The Gender Gap in Realized Wages

6.3.1 Blinder-Oaxaca Decomposition

The most common approach employed in the literature on gender gaps is the decomposition proposed by Blinder (1973) and Oaxaca (1973). In the standard Blinder-Oaxaca (BO) decomposition, separate regressions are estimated for group A ($Y_i = \beta_A X_i + \epsilon_i$) and for group B ($Y_i = \beta_B X_i + \epsilon_i$), where X are individual level characteristics that help explain differences in Y. The average gap in outcomes ($\bar{Y}_A - \bar{Y}_B$) can be expressed as the sum of two components: $\beta_A(\bar{X}_A - \bar{X}_B) + (\beta_A - \beta_B)\bar{X}_B$. The first part is attributed to differences in average characteristics between the two groups (i.e., the explained component). The second part is due to differences in average returns to the individual characteristics, which may reflect discrimination (i.e., the unexplained gap).

Much has been written about how best to express the appropriate counterfactual and whether one should use group A or group B as the reference group when performing the decomposition in order to examine the extent to which characteristics matter. As our benchmark approach, we adopt a straightforward way of estimating the gender gap in employment and wages. We refer to this as the pooled regression decomposition approach as this approach simply uses the coefficient on a group indicator from an OLS regression in order to obtain a single measure of the unexplained gap in wages between men and women. This pooled coefficient can essentially be viewed as a weighted average of the two different ways of doing a BO decomposition (see Elder et al., 2009). The unexplained effect in a decomposition has a similar interpretation to a treatment effect in the program evaluation literature, with one key difference being that the explained effect is of interest in a decomposition but considered to be selection bias that needs to be controlled for in the program evaluation literature (see e.g. Fortin et al., 2011).

6.3.2 Decomposition of the Gender Gap and the Role of Reservation Wages

To conduct our empirical analysis in a systematic way, we start decomposing the raw gender wage gap, of about 11.9%, using the BO approach discussed before. Therefore, we separately include the different groups of control variables defined in Section 6.2.2.[85] The decomposition results of the realized wage gap are presented in Panel A of Table 6.2. First, we include the baseline variables – socio-demographic characteristics and local unemployment rates – in column (1) into the decomposition analysis. It can be seen that this explains only about 16.0% of the raw gender gap. In a second step, we account for differences with respect to personality traits as these variables are identified as potential explanations for wage differentials in the previous literature (see e.g. Mueller and Plug, 2006). However, these variables can only explain 10.1% of the wage gap. Third, since women are on average better educated than men, and a higher level of education is associated with higher earnings, conditioning the decomposition on the educational level slightly increases the unexplained part of the wage gap. Earlier studies have shown that women's increasing level of education lead to a substantial decline of the gender wage gap (see Weichselbaumer and Winter-Ebmer, 2005, for an overview). In a fourth step, we include the labor market histories which account for more than half of the gender wage gap. This is in line with previous findings that point out the importance of work experience when decomposing the gender wage gap (see e.g. Light and Ureta, 1995). As a fifth group, we take into account job search characteristics and expectations which can explain 11.8% of the wage differential. Finally, when all groups of control variables are jointly included in column (6), we can explain about 58% of the unconditional wage gap and it drops from 11.9% to 5.0% but remains statistically significant. Comparing columns (4) and (6) shows that once we control for labor market histories, the additional effect of the other control variables seems to be relatively small. The strong impact of labor market histories is not very surprising given that past realizations of labor market outcomes also depend on unobserved factors that are important for the current wage (see Caliendo et al., 2017).

When interpreting our findings it should be taken into account that – due to the empirical setting and the data gathering process – we focus on a specific sample of individuals freshly entering unemployment and finding a job within one year. In comparison to previous studies, we can see that the unexplained gap in realized wages in our sample is only about half of the full population gap (e.g. Bauer and Sinning, 2010) and one third of the gap for graduates (e.g. Machin and Puhani, 2003). Assuming that our sample of

[85]The full set of control variables can be found in the notes of Table 6.2.

Table 6.2: Decomposition of the Gender Gap in Realized Wages and the Role of Reservation Wages

	Raw gap	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log hourly realized wage in wave 2									
Unexplained (total)	-0.119***	-0.100***	-0.107***	-0.127***	-0.056***	-0.105***	-0.050**	-0.034*	-0.024
	(0.022)	(0.021)	(0.0213)	(0.021)	(0.022)	(0.021)	(0.021)	(0.019)	(0.0194)
Explained (total)		-0.019**	-0.012*	0.008	-0.063***	-0.014***	-0.069***	-0.085***	-0.095***
		(0.008)	(0.006)	(0.009)	(0.012)	(0.005)	(0.015)	(0.012)	(0.015)
%-share explained (total)		16.0	10.1	-6.7	52.9	11.8	58.0	71.4	79.8
Explained by									
Log reservation wage in wave 1								-0.086***	-0.049***
								(0.012)	(0.008)
No. of observations	1,974	1,974	1,974	1,974	1,974	1,974	1,974	1,974	1,974
Control variables									
1) Baseline variables		✓					✓		✓
2) Personality traits			✓				✓		✓
3) Education				✓			✓		✓
4) Labor market history					✓		✓		✓
5) Job search & expectations						✓	✓		✓

Note: Depicted are estimation results of a Blinder-Oaxaca decomposition. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are shown in parenthesis.
1) Baseline variables: Marital status, German citizenship, age, migration background, number of children, place of residence (East- or West-Germany), local unemployment rate, month of entry into unemployment
2) Personality traits: Openness, conscientiousness, extraversion, neuroticism, locus of control, life satisfaction
3) Education: School leaving degree (none, lower, middle or upper secondary education), vocational training (none, professional training – internal or external, technical college or university degree)
4) Labor market history: Time between entry into unemployment and interview, unemployment benefit recipient, level of unemployment benefits, lifetime months in unemployment (div. by age-18), lifetime months in employment (div. by age-18), employment status before unemployment
5) Job search & expectations: Number of own job applications (since entry into unemployment), application for jobs involving a relocation, searching for a new job via friends or acquaintances, expected probability to find a job in next 6 months, expected probability of program participation

unemployed job-seekers represent individuals who earn relatively low wages, a potential explanation for these differences includes an increasing gender gap within the wage distribution (see Arulampalam et al., 2007). Moreover, also the fact that we observe both men and women at the beginning of an employment spell,[86] as well as a trend of increasing gender equality over the last years could explain the smaller wage gap in our sample (see e.g. Jarrel and Stanley, 2004).[87]

In order to analyze the importance of reservation wages for the decomposition of the gender gap in realized wages, we finally include two additional specifications. In column (7) we include the reservation wage as the only control variable beside the gender dummy, while in column (8) we add the reservation wage as an additional control to our full specification. The striking result is that with the reservation wage included in the full specification we can now explain 79.8% of the raw gap and more importantly, no significant gender gap remains. In light of the fact that numerous previous wage decomposition studies have not accounted for the gender gap in reservation wages, one possible interpretation is that the reservation wage is a key omitted variable that has been missing in previous decomposition exercises. The importance of reservation wages is further emphasized by the fact that controlling only for reservation wages, without including any other covariates, still accounts for 71.4% of the gender wage gap.

6.3.3 Addressing the Potential Endogeneity of Reservation Wages

So far, we have seen that the gender gap in realized wages becomes statistically insignificant once we control for reservation wages. However, a potential concern when including reservation wages into the decomposition of realized wages could be that reservation wages are correlated with unobserved characteristics that simultaneously also affect realized wages. For example, it is possible that individuals with higher abilities also set higher reservation wages as well as earn correspondingly higher wages. Although a conventional solution to such endogeneity problems is to use instrumental variable (IV) methods, in practice, it is

[86] Previous studies, e.g. Machin and Puhani (2003), Bauer and Sinning (2010), typically compare men and women at different points during an employment spell, while others, e.g. Arulampalam et al. (2007), include control variables for tenure. Assuming that gender differences in promotion, tenure decisions and job changes are determinants of the gender wage gap, this might explain that those studies find larger gender gaps in observed wages.

[87] Whenever possible, we re-estimate our findings for less selective samples, e.g. including also those who contemplate part-time employment or, when decomposing the gender gap in reservation wages, including job-seekers who are not employed, respectively not observed, in wave 2. All estimates are very similar to our baseline findings presented in the chapter. Results are available upon request by the authors.

difficult to find a variable that is correlated with reservation wages but has no influence on the realized wage.[88]

Therefore, as an attempt to examine if the potential endogeneity of reservation wages is an issue in our context, we adopt a recently developed instrumental variable approach that relies on the presence of heteroskedastic error terms for identification proposed by Lewbel (2012). It is assumed that the estimated model is given as:

$$\log W = \delta \log R + \beta_1 X + \epsilon_1 \qquad \epsilon_1 = \alpha_1 U + V_1 \tag{6.1}$$

$$\log R = \beta_2 X + \epsilon_2 \qquad \epsilon_2 = \alpha_2 U + V_2. \tag{6.2}$$

Here, W characterizes the realized wage, R the reservation wage and X our set of control variables, while U denotes the unobserved characteristics that affect both, an individuals realized wage and the reservation wage. V_1 and V_2 are idiosyncratic error terms. The Lewbel IV approach involves taking a vector Z of observed exogenous variables and utilizing the estimated residuals to generate instruments for reservation wages R. The presence of an external instrument as in the classical IV approach is not required. Given that $E[X\epsilon_1] = 0$ and $E[X\epsilon_2] = 0$ the identification approach requires

$$cov(Z, \epsilon_2^2) \neq 0, \tag{6.3}$$

$$cov(Z, \epsilon_1 \epsilon_2) = 0 \tag{6.4}$$

and the model can be estimated by Two Stage Least Squares (2SLS). In the first-stage, the endogenous variable is regressed on Z and the estimated residuals are used to construct the instruments $[Z - E(Z)]\epsilon_2$ which represents the product of the heteroskedastic residuals with the mean-centered exogenous variables. According to equation 6.3, identification requires that the error terms in the first-stage regression are heteroskedastic. Lewbel (2012) suggests using the estimate of the sample covariance between Z and squared residuals from the first stage regression linear regression on X to test for this requirement, using the Breusch-Pagan test for heteroscedasticity (see Breusch and Pagan, 1979). The results of the test (see Panel A of Table 6.3) show that the null of homoskedastic errors is clearly rejected in each case with a $p-$value equal to 0.01 or less, while the first stage F-test results also suggest that the generated instruments employed are not weak instruments.

Moreover, as implied in equation 6.4, another crucial assumption of this estimation procedure is that the covariates Z which are used to construct the instrument are exogenous with respect to reservation wages and realized wages. There are no formal approaches

[88] Some other studies have previously used benefit amounts as an instrument for reservation wages (e.g. Jones, 1988) but this is not an option in our context because unemployment benefits in Germany are directly related to previous net income.

for the optimal selection of Z and the resulting estimates are potentially sensitive to the choice of included covariates Z. As such, the coefficient on reservation wages and the estimated gender gap in wages could be sensitive to the composition of Z. We address this issue by analyzing the sensitivity of this generated IV approach with respect to the choice of Z in order to provide evidence for its plausibility.

Table 6.3: Sensitivity Analysis: Generated Instrumental Variable Approach

Log hourly realized wage in wave 2	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS estimation						
Female		-0.016	-0.018	-0.033	-0.024	-0.016
		(0.021)	(0.021)	(0.021)	(0.018)	(0.018)
Log reservation wage in wave 1		0.726^{***}	0.681^{***}	0.597^{***}	0.384^{***}	0.369^{***}
		(0.092)	(0.084)	(0.087)	(0.077)	(0.063)
No. of observations		1,974	1,974	1,974	1,974	1,974
R^2		0.278	0.289	0.307	0.341	0.343
Adjusted R^2		0.269	0.277	0.294	0.323	0.322
$F-$statistic for weak identification		9.85	10.80	12.58	10.80	10.27
Breusch-Pagan test χ^2		8.54	16.16	21.70	22.40	23.75
		{0.003}	{0.000}	{0.000}	{0.000}	{0.000}
Sargan-Hansen test J		43.81	44.95	39.82	60.91	68.57
		{0.006}	{0.039}	{0.264}	{0.161}	{0.209}
B. OLS estimation						
Female	-0.034^{*}	-0.032^{*}	-0.030	-0.050^{***}	-0.029	-0.024
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Log reservation wage in wave 1	0.685^{***}	0.613^{***}	0.587^{***}	0.496^{***}	0.399^{***}	0.390^{***}
	(0.027)	(0.039)	(0.039)	(0.041)	(0.043)	(0.044)
No. of observations	1,974	1,974	1,974	1,974	1,974	1,974
R^2	0.257	0.285	0.294	0.313	0.347	0.350
Adjusted R^2	0.256	0.275	0.282	0.299	0.329	0.329
Control variables						
Baseline variables		✓	✓	✓	✓	✓
Personality traits			✓	✓	✓	✓
Education				✓	✓	✓
Labor market history					✓	✓
Job search & expectations						✓

Note: Depicted are regression results of the gender gap in realized wage in wave 2 using 2SLS with generated instrumental variables (Panel A), respectively OLS (Panel B). */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are shown in parenthesis, respectively p-values in curly brackets. For the full set of explanatory variables see notes of Table 6.2.

We start the analysis in Table 6.3 by including only the baseline variables. The idea is to exploit only a small set of covariates which can be reasonably assumed to be exogenous with respect to reservation wages and realized wages. Next, we consecutively include the other sets of control variables where the ordering reflects our expectations about the importance of potential endogeneity issues. We start by adding the personality traits (column 2), which are usually assumed to be relatively stable over the adult life (see e.g. Costa Jr. and McCrae, 1994; Cobb-Clark and Schurer, 2013). This is followed by adding the education variables (column 3) and the labor market history variables (column 4). Finally, we complete the analysis by adding the job search characteristics and expectations variables (column 5). As search theory suggests that search intensity and reservation wages are simultaneously determined, the inclusion of this latter set of variables in X or Z is potentially problematic. Therefore, we include this group only in the very last specification. The results show that the variation with respect to the estimated gender gap is very small across the different specifications. Although, we cannot directly test for the exogeneity of variables in X or Z, we argue that the robustness of the estimated coefficients suggests that our key finding is not biased by the potential endogeneity of the covariates. It should be also noted that the results of the Sargan-Hansen test (see Sargan, 1958; Hansen, 1982) show that the overidentifying restrictions are not rejected when we use richer specifications for our set of covariates (columns 3-5), suggesting that the excluded instruments are valid.

Overall, the IV results indicate that the size of the gender wage gap is largely independent of the choice of the covariates that we use in order to generate the instrumental variables. This can be interpreted as evidence that the potential endogeneity of the covariates has only a negligible impact on the empirical results in our context. Moreover, when comparing the 2SLS (Panel A) and OLS (Panel B) estimates, it can be seen that results are very similar. With the reservation wage included in the wage decomposition, there is no gender gap in observed wages. This indicates that potential endogeneity of reservation wages has in general only a small impact on our decomposition results presented in Table 6.2.

6.4 Why Do Women Have Lower Reservation Wages?

6.4.1 The Gender Gap in Reservation Wages

Having established the importance of reservation wages for the gender wage gap, we now examine the gender gap in reservation wages more closely in Table 6.4. The raw gender gap in reservation wages is about 12.5%, implying that women expect significantly lower wages than men. Again, we sequentially add the five groups of control variables in columns (1)–(5). Although, the gender gap in reservation wages seems to be slightly larger for all specifications, the overall pattern looks very similar to the one in realized wages. The baseline variables, as well as the personality traits have only a small impact in reducing the gender gap in reservation wages, while, since women in our sample are generally better educated but have lower reservation wages the gap increases slightly by adding the educational variables in column (3). Again, the labor market history variables in column (4) seem to have the highest explanatory power, while the impact of job search characteristics and expectations is rather small. In general, we can explain a reasonably large part of the reservation wage gap for those individuals who are employed in the second wave. When we include all four groups of covariates, a gender gap in reservation wages of 6.7% remains. Therefore, we can explain about 46.4% of the original gap in reservation wages. Finally, we also include the last wage before entering unemployment as a control variable in columns (7) and (8), since this is used as a proxy for reservation wages in many empirical studies. While this reduces the reservation wage gap a bit further, a significant gap of 5.2% still remains unaccounted for.

6.4.2 Heterogeneity in the Gender Gaps

Since the observed characteristics included in the decomposition of the reservation wage gap are only partially successful in explaining the gender differences, we now exploit the gender gap in realized wages and reservation wages for different subgroups – based on education, labor market experience and personality – that are expected to be differently affected by related unobserved factors that potentially explain the unexplained part in the reservation wage gap. The idea is to find subgroups where there is neither a gender gap in reservation wages nor realized wages. To the extent that the same unobserved factors affect realized wages and reservation wages, this exercise will allow us to potentially identify unobserved factors that play an important role in the evolution of the two gender wage differentials. Table 6.5 present the subgroup estimates for decomposing the wage and reservation wage gaps. The results for realized wages correspond to the specification (6) in Table 6.2 where we include all covariates except for the reservation wage, while the estimates for reservation wages correspond to specification (8) where we include all four groups of covariates and the last realized wage before unemployment.

First, as discussed before, employer-preferences, e.g., taste-based discrimination against women, is one potential explanation for the gender gap in reservation wages when the discriminatory behavior is anticipated by individuals in the labor market. Assuming that these expectations are related to one's own labor market experiences, it is useful to distinguish between people with low/high labor market experience. We expect women who have spent only a short time in employment to be less likely to have experienced discrimination in the past and hence also to be less likely to expect discrimination in future jobs. Our measure of experience is computed using the ratio of months spent in employment and the individual age in years minus 18 in order to disentangle potential age and experience effects. Based on using median experience as the dividing line, we estimate

Table 6.4: Decomposition of the Gender Gap in Reservation Wages

	Raw gap	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log hourly reservation wage in wave 1									
Unexplained (total)	-0.125***	-0.112***	-0.111***	-0.130***	-0.070***	-0.115***	-0.067***	-0.065***	-0.052***
	(0.016)	(0.015)	(0.016)	(0.015)	(0.015)	(0.016)	(0.014)	(0.013)	(0.013)
Explained (total)		-0.013*	-0.014***	0.005	-0.055***	-0.010***	-0.058***	-0.060***	-0.073***
		(0.007)	(0.005)	(0.007)	(0.010)	(0.004)	(0.012)	(0.009)	(0.013)
%-share explained (total)		10.4	11.2	-4.0	44.0	8.0	46.4	48.0	58.4
Explained by									
Log realized wage before unemployment								-0.133**	-0.084**
								(0.053)	(0.035)
No realized wage before unemployment								0.073	0.047
								(0.051)	(0.033)
No. of observations	1,974	1,974	1,974	1,974	1,974	1,974	1,974	1,974	1,974
Control variables									
1) Baseline variables		✓					✓		✓
2) Personality traits			✓				✓		✓
3) Education				✓			✓		✓
4) Labor market history					✓		✓		✓
5) Job search & expectations						✓	✓		✓

Note: Depicted are estimation results of a Blinder-Oaxaca decomposition. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are shown in parenthesis. For the full set of explanatory variables see notes of Table 6.2.

gender gaps for those with low experience and those with high experience. Columns (3) and (4) in Table 6.5 show that there is no gender gap in reservation wages for those with low experience and also no corresponding gender gap in observed wages in wave 2, while for those with more labor market experience, a significant gender gap in reservation wages and realized wages emerges. The strong correlation between the size of the gender gap and the level labor market experience indicates, on the one hand, that women's expectations about employer-preferences play an important role for reservation wages and actual realized wages. However, on the other hand, for our sample of individuals recently starting a new job after a period of unemployment, there is no evidence for actual discrimination against women among those with little labor market experience. It should be noted that this is in line with the findings of experimental studies showing that discrimination against women is more important when applying for high-skilled jobs (e.g. Petit, 2007) or promotion decisions (e.g. Baert et al., 2016).

Second, we expect that an individual's productivity influences not only actual wage offers but also wage expectations. As educational qualifications are likely to capture some of these productivity differences, we also examine the gender gap for individuals with A-level qualifications or higher (column 5), and those with less than A-level qualifications (column 4). Once again we find a pairing of there being no gender gap in reservation wages and observed wages for those with higher than A-level qualifications, reinforcing the notion that the two gaps are closely related and indicating that having a higher level of education is associated with men and women being more similar, as productivity differences are expected to be less pronounced within the group of high educated workers.

Third, we also examine subgroups based on job-seeker's personality. In particular, we divide the estimation sample into individuals with respect to their level of openness, which is part of the so called 'Big Five' personality traits. As shown by Mueller and Plug (2006), openness is associated with substantially higher earnings for men and women. One reason for that might be that individuals who are open to experiences have preferences for different types of jobs or behave differently in wage negotiations. In our sample it can be seen that the gender wage gap for individuals with low levels of openness (6.3%, column 6) is above that of the full sample, while for those with high levels of openness (column 7) there is no significant gender gap in realized wages. Moreover, the gender gap in reservation wages for this subgroup is very similar to the gender gap in realized wages in terms of size and statistical significance. This indicates that differences in personality traits, that might be associated with gender differences in job preferences (see Goldin, 2014; Kleinjans and Fullerton, 2013) and the behavior in salary negotiations (see Solnick, 2001), are another important driving factor for reservation wages that could explain our baseline findings.

Table 6.5: Subgroup Analysis: OLS Estimates for Realized Wages and Reservation Wages

		LM experience[(a)]		A-level		Openness[(b)]	
	Full sample	Low	High	No	Yes	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Log hourly realized wage in wave 2							
Female	-0.050**	-0.018	-0.102***	-0.065**	-0.026	-0.063**	-0.025
	(0.021)	(0.018)	(0.032)	(0.026)	(0.036)	(0.028)	(0.033)
No. of observations	1,974	989	985	1,369	605	1,073	901
R^2	0.305	0.386	0.277	0.248	0.371	0.302	0.351
Adjusted R^2	0.283	0.345	0.229	0.213	0.304	0.260	0.305
B. Log hourly reservation wage in wave 1							
Female	-.052***	-0.015	-0.097***	-0.077***	-0.015	-0.065***	-0.030
	(0.012)	(0.019)	(0.017)	(0.015)	(0.025)	(0.017)	(0.019)
Log realized wage before unemployment	0.316***	0.338***	0.302***	0.320***	0.315***	0.303***	0.328***
	(0.016)	(0.026)	(0.020)	(0.019)	(0.032)	(0.021)	(0.026)
No realized wage before unemployment	2.076***	2.304***	1.877***	2.046***	2.163***	2.002***	2.133***
	(0.106)	(0.170)	(0.137)	(0.124)	(0.212)	(0.141)	(0.169)
No. of observations	1,974	989	985	1,369	605	1,073	901
R^2	0.535	0.553	0.573	0.484	0.549	0.537	0.549
Adjusted R^2	0.520	0.522	0.544	0.459	0.499	0.508	0.516
Control variables							
1) Baseline variables	✓	✓	✓	✓	✓	✓	✓
2) Personality traits	✓	✓	✓	✓	✓	✓	✓
3) Education	✓	✓	✓	✓	✓	✓	✓
4) Labor market history	✓	✓	✓	✓	✓	✓	✓
5) Job search & expectations	✓	✓	✓	✓	✓	✓	✓

Note: Depicted are regression results of the gender gap in realized wage in wave 2 (upper part) and reservation wages in wave 1 (lower part) using OLS. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are shown in parenthesis. The decomposition of the realized wage gap does explicitly not include the reservation wage as control variable, while the decomposition of realized wages include previous wages as a covariate. For the full set of explanatory variables see notes of Table 6.2.

[(a)] Labor market (LM) experience refers to the lifetime months in employment divided by age-18. Low (High) level of LM experiences refers to values below (above) the median.

[(b)] Openness is measured with different items on a 7-Point Likert-Scale. Low (High) level of openness refers to values below (above) the median.

The results of the subgroup analysis have important implications for the interpretation of our initial decomposition analysis by allowing us to identify potential reasons for why men and women set different reservation wages. First, women could anticipate discrimination in the labor market and adjust their reservation wages accordingly. This argument is supported by the fact that we find no gender gap in reservation wages for women with little labor market experience (relative to their age). Second, there might be gender differences with respect to productivity. We find evidence for this as both gaps become statistically insignificant when focusing on high educated workers which are expected to be more similar in terms of productivity. Finally, we find no gender gap in wages and reservation wages for individuals who are similar in terms of personality traits (a high level of openness), which is expected to proxy for gender differences with respect to job preferences and wage bargaining behavior.

6.5 Conclusion

The economic literature typically finds a persistent wage gap between men and women. In this chapter, based on a sample of newly unemployed persons seeking work in Germany, we find that the gender wage gap becomes small and statistically insignificant once we control for reservation wages in a wage decomposition exercise. Although, our estimation sample comprises a very specific group of individuals, unemployed job-seekers are arguably the most relevant group when utilizing the concept of reservation wages. Moreover, we focus on a very homogeneous group of workers which has the advantage that potential endogeneity of reservation wages has only minor impact on our empirical findings. The latter is supported by the heteroskedasticity-based instrumental variable approach, as well as the subgroup analysis which shows strong correlations between the gender gap in reservation wages and realized wages.

As the gender gap in actual wages appears to mirror the gender gap in reservation wages, there is a clear need to better understand why there are gender differences in the way reservation wages are set in the first place. We believe that the exploratory results in the chapter can help to better understand what the driving forces behind this gender gap are. First, differing expectations can be important in explaining the reservation wage gap and might arise for various reasons. Our empirical findings that the gender gap in reservation wages appears to increase with labor market experience suggests that expectations are changing over time in a non-symmetric fashion for men and women. A potential explanation implies that women who had experienced discrimination in the past set relatively low reservation wages which translates into the gender gap in realized wages. This kind of self-fulfilling prophecy could potentially cause a gender gap in realized wages even in the absence of actual discrimination. Second, the gender gap in reservation wages could reflect productivity differences between men and women. As reservation wages reflect a worker's own valuation of their time while employed, high productivity workers are likely to set relatively higher reservation wages. On the other hand, lower productivity workers will tend to receive fewer wage offers and experience longer unemployment spells (e.g. by virtue of signaling lower observable ability in a job interview). This will lead them to lower their reservation wages over time in order to increase their employment prospects. Third and finally, the gender gap in reservation wages might exist because men and women have different personality traits or preferences which results in gender differences with respect to the value of non-market time and different job characteristics, like flexible time at work and on the continuity of work hours. Future research might want to focus on designing survey questions that better elicit information on the nature of such differing expectations or preferences to help disentangle between these factors.

6.6 Appendix: Properties of Reservation Wages

As we use reservation wages measured in wave 1 to perform a wage decomposition one year later in wave 2, an assumption that we need to make is that reservation wages do not change differently for men and women over the course of time. Unfortunately, it will not be possible to check the plausibility of the assumption for our analysis sample as questions regarding reservation wages are not asked once an individual is employed and our wage decompositions are based on those who are employed in wave 2. Instead, we will rely on examining various samples of individuals in our data who continue to remain in an unemployed state over time to determine if and when reservation wages might change over time. Therefore, we utilize the fact the IZA Evaluation Dataset contains two more waves of interviews. The interim wave takes place about 6 months after the entry into unemployment, while wave 3 of the survey is conducted about 36 months after entering unemployment.[89]

6.6.1 Descriptive Statistics

In Table 6.6, we specifically present descriptive statistics on changes in reservation wages over time for three different groups of individuals: (i) unemployed in waves 1 and 2; (ii) unemployed in wave 1 and interim wave; (iii) unemployed in waves 1, 2 and 3. This allows us to to look at the time trend of reservation wages over a period of between 7 to 14 weeks after the entry into unemployment till three years later for those that remain unemployed. We can see that in Panel A (the sample that focuses on changes between waves 1 and 2 and which is most relevant for our purposes), the reservation wages decreases very slightly over the 12 month period between waves 1 and 2. This is also the case for the sample in Panel B for whom we observe changes over the 6 month period between wave 1 and the interim wave. The samples for the analysis are smaller in Panels C but are suggestive that it is between waves 2 and 3 (one to three years after entering unemployment) that larger changes in reservation wages begin to occur. Hence, within our observation period covering wave 1 and 2, there is no evidence for gender differences with respect to the development of reservation wages.

6.6.2 Fixed-Effect Estimation

As an additional sensitivity analysis for potential gender differences over the period of the unemployment spell, we regress the reservation wage of those individuals who report the information at more than one interview on the actual unemployment duration at the

[89]The interim wave is restricted to three entry cohorts only comprising a total of 2,548 individuals, while in the third wave 5,786 individuals are interviewed again.

moment of the interview. This allows us to include individual fixed effects into our analysis. As shown in Table 6.7, there is evidence for a non-linear relationship between the unemployment duration and the reservation wage, however, as indicated by the insignificant interaction terms in column 4 and 5, there are no gender differences with respect to elasticity of reservation wages during the unemployment spell.

In summary, our findings suggest that 1) the development of reservations over time and 2) the evolution from previous wages to reservation wages to actual realized wages is similar for men and women allowing us to reasonably include reservation wages (measured at the entry into unemployment) into the decomposition of subsequently realized wages.

Table 6.6: Descriptive Statistics: Reservation Wages during the Unemployment Spell

	Men	Women	*P*-Value
Panel A: Unemployed in wave 1 and wave 2			
No. of observations	508	308	
Hourly reservation wage (in Euro)			
Wave 1	7.61	6.27	0.00
Wave 2	7.60	6.19	0.00
Difference (wave 2 - wave 1)	-0.01	-0.08	0.65
Panel B: Unemployed in wave 1 and interim wave			
No. of observations	216	141	
Hourly reservation wage (in Euro)			
Wave 1	7.76	6.41	0.00
Interim wave	7.73	6.37	0.00
Difference (interim wave - wave 1)	-0.03	-0.04	0.93
Panel C: Unemployed in wave 1, wave 2 and wave 3			
No. of observations	74	48	
Hourly reservation wage (in Euro)			
Wave 1	8.21	6.18	0.01
Wave 2	8.19	5.97	0.00
Wave 3	7.73	6.77	0.12
Difference (wave 2 - wave 1)	-0.01	-0.21	0.64
Difference (wave 3 - wave 1)	-0.47	0.58	0.00
Difference (wave 3 - wave 2)	-0.46	0.80	0.01

Note: Depicted are average reservation wages by gender for different samples based on availability of reservation wages in the four waves of the survey.

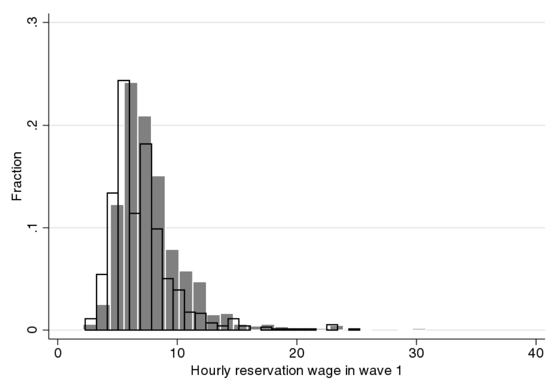
Table 6.7: Fixed-Effect Estimation: The Impact of the Unemployment Duration on Reservation Wages

	(1)	(2)	(3)	(4)	(5)
Unemployment duration in months	-0.0002	0.0001	-0.0038**	-0.0036**	-0.0046**
	(0.0003)	(0.0004)	(0.0017)	(0.0017)	(0.0022)
Unemployment duration^2			0.0001**	0.0001**	0.0001**
			(0.00004)	(0.00004)	(0.00005)
Female × unemployment duration				-0.0001	0.0022
				(0.0007)	(0.0034)
Female × unemployment duration^2					-0.0005
					(0.0001)
P-value for joint significance					
Unemployment duration(^{2})			0.077	0.076	0.083
Female × unemployment duration(^{2})					0.775
No. of observations	2,652	2,652	2,652	2,652	2,652
No. of individuals	1,221	1,221	1,221	1,221	1,221
Individual fixed effects	✓	✓	✓	✓	✓
Control variables					
1) Baseline variables		✓	✓	✓	✓
2) Personality traits		✓	✓	✓	✓
3) Education		✓	✓	✓	✓
4) Labor market history		✓	✓	✓	✓
5) Job search & expectations		✓	✓	✓	✓

Note: Depicted are estimated effects of the actual unemployment duration on reservation wages. */**/*** indicate statistically significance at the 10%/5%/1%-level. Standard errors are shown in parenthesis. $P-$values refer to the $F-$statistic for $Wald$ tests of joint significance. Only time-varying covariates are included.

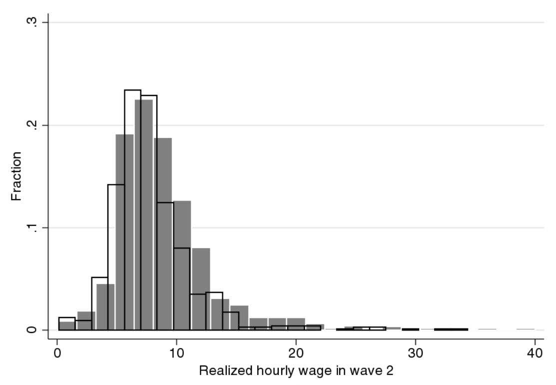
Figure 6.1: Distribution of Reservation Wages, Realized Wages and Previous Wages by Gender

(a) Reservation wage



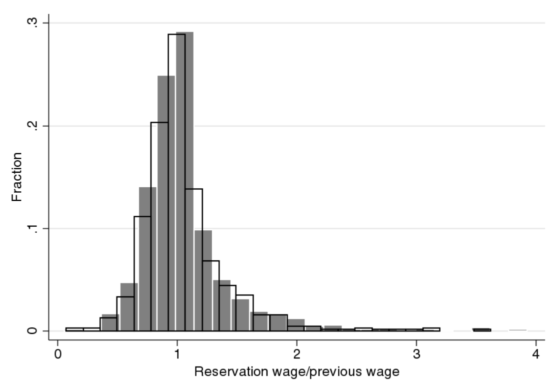
Kolmogorov-Smirnov test: $D = 0.170$; $p = 0.000$

(b) Realized wages



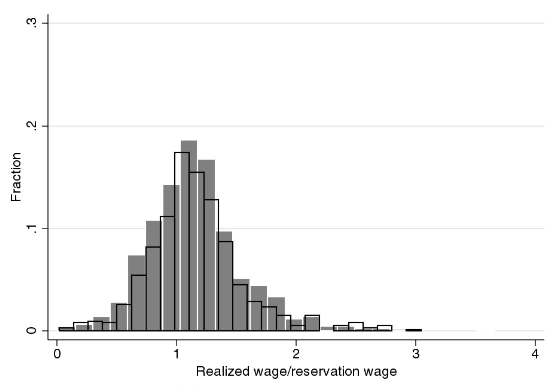
Kolmogorov-Smirnov test: $D = 0.145$; $p = 0.000$

(c) Reservation wage/previous wage



Kolmogorov-Smirnov test: $D = 0.022$; $p = 0.988$

(d) Realized wage/reservation wage



Kolmogorov-Smirnov test: $D = 0.038$; $p = 0.481$

Men Women

Note: (a) Reservation wages and (b) realized wages, as well as (d) the ratio of reservation wages and realized wages are depicted for all individuals of the main sample (n=1,974), while (c) the ratio of reservation wages in wave 1 and previous wages is only observed for a subsample (n=1,719).

7 Summary and Overall Conclusion

The thesis offers a new perspective on ALMP programs in Germany and provides new evidence with respect to the interplay of the job search behavior and the system of labor market policies. In particular, the existing literature is extended with respect to four dimensions. First, I analyze the impact of subjective beliefs about ALMP programs (measured before participation) on the effectiveness of realized treatments. Second, the effect of subsidies which aim to improve the geographical mobility of unemployed workers on the search behavior and subsequent labor market outcomes is evaluated. Third, the relevance of usually unobserved variables for the evaluation of traditional labor market policies, like training and wage subsidies, is specified. And finally, the thesis examines the importance of gender differences in reservation wages among unemployed workers for the evolution of the gender gap in realized wages.

Chapter 2 relates the job seekers expectations about upcoming ALMP programs when entering unemployment to the effectiveness of long-term training measures realized later during the unemployment spell. A combination of rich survey and administrative data provides detailed information about the expected participation probability, as well as the expected effect of a training program on the subsequent labor market performance. The empirical results show that training programs are more effective with respect to the subsequent employment probability if the participants expect participation ex ante, while expected treatment effects are unrelated to the actual labor market outcomes of participants the training program. An extensive sensitivity analysis shows that the empirical results are highly robust with respect to several types of observed and unobserved heterogeneity. In particular, this includes the job seekers ability to predict future economic outcomes, differences with respect to motivation and the timing of the treatment. A subsequent analysis of the effect mechanisms shows that job seekers who expect to participate also receive more information by their caseworker and show a higher willingness to adjust their search behavior in association with an upcoming ALMP program. Finally, structural estimates of the job seekers value functions considering expectations about job finding probabilities, earnings and treatment effects, suggest that the adjustment of the job search behavior related to ALMP programs is associated with additional costs. These costs can be influenced by the employment agency through different information treatments and are directly related to the subsequent labor market performance of participants.

The findings of Chapter 2 show that the effectiveness of long-term training programs, which require a high level of participants' commitment and create large costs for the society, can be improved by providing more detailed information about the possibility of a future treatment early during the unemployment spell. A potential mechanism which has been already introduced in Germany is given by the voucher system. Potential participants receive a training voucher which defines the maximum duration, the target of a program and its costs. Since job seekers can redeem the voucher within three months, but also face the possibility to not redeem the voucher, the system can be expected to reduce the uncertainty for potential participants and therefore improve the program effectiveness. However, since the survey data show that only a small number of job seekers receive the training voucher early during the unemployment spell there seems to be substantial room for improvements with respect to the information policies of the employment agency.

Chapter 3 and 4 evaluate a class of innovative programs which have not received any attention in the previous literature. These programs aim to improve the geographical mobility of unemployed workers by providing financial support when searching for, respectively accepting, jobs in geographically distant regions. To estimate the causal impact of these programs on the job seekers behavior, subsequent job finding prospects and the returns to labor market mobility, it is exploited that local employment agencies have a degree of autonomy when deciding about the regional-specific policy mix, i.e. the allocation of their budget to different ALMP programs. This creates exogenous variation with respect to two channels. First, it affects the probability that caseworkers inform job seekers about the availability of the subsidy programs and second, also with respect to the approval of the subsidy which is at the caseworker's discretion. Based on this setting, the findings discussed in Chapter 3 show that the policy style of the employment agency indeed affects the job search behavior of unemployed workers. Job seekers who are assigned to agencies with higher preferences for mobility programs increase their search radius without affecting the total number of job applications. This shift of the search effort to distant regions leads to a higher probability to find a regular job and higher wages. Moreover, it reduces also the dependence of other governmental support, e.g. start-up subsidies, which create substantially higher costs compared to mobility assistance.

Based on these results, Chapter 4 analyzes the effect of actually participating in one of the subsidy programs and moving to geographically distant region on long-term labor market outcomes. It is shown that participants earn significantly higher wages, end up in more stable jobs and face a higher long-run employment probability. The analysis on the relative rank within the regional wage distribution also shows that the positive wage effect is not only a manifestation of regional differences with respect to price levels. Moreover, an extensive sensitivity analysis shows that the findings of Chapter 3, as well as Chapter 4, are highly robust with respect to unobserved regional heterogeneity which could potentially affect the policy mix of the local employment agency. Considering the results of both chapters, they point out the large potential of mobility assistance for the reintegration of unemployed workers into the labor market. Since, the findings of Chapter 3 show that job seekers indeed change their job search behavior due to the availability of the subsidies, it is unlikely that the program creates large deadweight effects stemming from the fact that some participants might have moved even in the absence of the program. The results are even more remarkable in the light of the relatively low program costs compared to other ALMP measures, like vocational training (which are about six times larger per participant), with less positive effects. However, despite the promising effects of the subsidies on the individual level, it should be noted that the analysis does not consider general equilibrium effects which might become more important if policy makers decide to extent the scope of the subsidy to encourage more job seekers to move to distant regions. Since predominately young workers with relatively high level of education move from depressed to prosperous region, it can be expected that there are detrimental effects on the region of origin due to the outflow of high qualified workers.

Chapter 5 examines the sensitivity of estimators based on the unconfoundedness assumption with respect to the inclusion of variables which are usually unobserved to the researcher when evaluating ALMP programs. A unique dataset which combines administrative records and survey data allows us to observe detailed information on typical covariates like socio-demographic characteristics and labor market histories, as well as usually unobserved variables including personality traits, attitudes, expectations, intergenerational information, as well as indicators about social networks and labor market flexibility. It can be expected that these variables affect the job search behavior, the selection into ALMP programs, as well as labor market outcomes simultaneously. The findings show that, although our set of usually unobserved variables indeed has a significant effect on the selection into ALMP programs, the overall impact when estimating treatment effects is rather small. This suggest that lacking information on these variables does not affect the assessment of typical labor market policies when rich data on employment histories are available. However, it should be noted that effects can clearly differ among different types of programs, different countries and populations of interest, as well as for other types of unobserved variables. Nevertheless, the findings suggest that valid concerns about the role of unobserved variables, when using a "selection on observables" assumption for the estimation of treatment effects, may be less relevant when observable information is available that is sufficiently correlated with the unobservable variables.

Finally, Chapter 6 analyzes the importance of gender differences in reservation wages among the unemployed for the evolution of the gender gap in realized wages. Based on two waves of rich survey data for newly unemployed job seekers we access to panel data on reservation wages measured at the entry into unemployment and realized wages about one year later for the same individual. These extraordinary rich data allow us to determine the extent to which gender differences in aspirations and expectations regarding wages can be a self-fulfilling prophecy and lead to gender differences in actual wages. In particular, when including reservation wages in a wage decomposition exercise, the gender gap in realized wages becomes small and statistically insignificant. The strong connection between gender differences in reservation wages and realized wages raises the question how these differences in reservation wages are set in the first place. Since traditional covariates cannot sufficiently explain the gender gap in reservation wages, we perform subgroup analysis to better understand what the driving forces behind this gender gap are.

The empirical findings show that there are several characteristics which are associated with the appearance of both gender gaps. First, the gender gap in reservation wages appears to increase with labor market experience suggests that expectations are changing over time in a non-symmetric fashion for men and women. A potential explanation implies that set relatively low reservation wages if they had experienced discrimination in the past which translates into the gender gap in realized wages. Therefore, the gender wage gap could emerge even in the absence of actual discrimination due to a self-fulfilling prophecy. Second, the educational level is identified as a driving factor of the reservation wage gap which suggests that the gender gap in reservation wages could reflect productivity differences between men and women. As reservation wages reflect a worker's own valuation of their time while employed, high productivity workers are likely to set relatively higher reservation wages, while workers with a lower productivity reduce their reservation wages to increase their employment prospects. Finally, the gender gap in reservation wages is associated with the job seekers personality traits reflecting differences with respect to the value of non-market time and different job characteristics, like flexible time at work and on the continuity of work hours. Future research might want to focus on designing survey questions that better elicit information on the nature of such differing expectations or preferences to help disentangle between these factors. Moreover, it should be noted that the findings allow us to draw conclusions only for a specific subsample of job-seekers entering unemployment shortly before they were interviewed for the first time but found a job within one year.

In summary, the findings of the thesis provide new insights with respect to the interplay of labor market policies and the job search behavior of unemployed workers. This is essential for the assessment of ALMP programs since traditional post-treatment evaluations, as it is typical performed in the economic literature, do often not provide satisfactory explanations for the actual effect mechanisms of a policy scheme. However, as the results show, a deeper understanding of these mechanisms provides important insights which allow policy makers to improve the design and the allocation of the labor market policies. For instance, as shown in Chapter 2 caseworkers can improve post-treatment outcomes with pre-treatment information strategies, while Chapter 3 and 4 show that informing job seekers about the availability of innovative programs, in this case subsidies which aim to improve the job seekers' labor market mobility, leads to behavioral changes that have positive effects on long-term labor market outcomes. However, in this context, the findings of Chapter 5 show that related variables, like personality traits, expectations, as well as information on social networks or intergenerational characteristics, which are usually unobserved to researchers evaluating ALMP programs and can be expected to be related to the effect mechanisms are not a threat to the unconfoundedness assumption. This supports the validity of previous studies evaluating common ALMP programs and relying on rich administrative data when estimating treatment effects on subsequent labor market outcomes. Finally, focusing on the reservation wage as a specific measure of the search behavior, Chapter 6 shows that there exist gender differences which suggest that the promotion of labor market programs targeting female job seekers might be justified.

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German Summary

Anhaltend hohe Arbeitslosigkeit stellt eine der zentralen Herausforderungen für den sozialen Zusammenhalt moderner Gesellschaften dar. Aus diesem Grund wenden viele Industrienationen einen beachtlichen Teil ihrer Staatsausgaben zur Bekämpfung der Arbeitslosigkeit und den damit verbundenen Konsequenzen auf. In den letzten Jahren ist hierbei eine Verschiebung von passiven Maßnahmen, wie beispielsweise Transferleistungen, hin zu aktivierenden arbeitsmarktpolitischen (AAMP) Maßnahmen zu beobachten. Obwohl sich bereits eine Vielzahl ökonomischer Studien mit den Effekten traditioneller Maßnahmen auf den nachfolgenden Erfolg am Arbeitsmarkt beschäftigt, ist ein besseres Verständnis des Einflusses auf das Arbeitssuchverhalten und das daraus resultierende Zusammenspiel mit dem Erfolg am Arbeitsmarkt notwendig um die Gestaltung und Zuweisung von AAMP Maßnahmen zu optimieren. Darüber hinaus weisen die Ergebnisse vorheriger Studien daraufhin, dass eine Vielzahl traditioneller Maßnahmen, wie öffentliche Beschäftigungsprogramme oder Trainingsmaßnahmen, nicht den gewünschten Erfolg erzielen. Dies unterstreicht die Notwendigkeit, zum einen die Wirkungsmechanismen aktiver Arbeitsmarktpolitik besser zu verstehen, und zum anderen innovative wirkungsvollere Maßnahmen zu entwickeln.

Basierend auf theoretischen Überlegungen und der empirischen Analyse deutscher Arbeitsmarktdaten, liefert die vorliegende Dissertation neue Erkenntnisse hinsichtlich verschiedener Faktoren im Bereich der Evaluierung von AAMP Programmen. Die Datenbasis für die empirische Analyse liefern die Integrierten Erwerbsbiografien (IEB) des Instituts für Arbeitsmarkt- und Berufsforschung (IAB) für arbeitslose Personen. Diese bieten umfassende Informationen bezüglich sozialversicherungspflichtiger Beschäftigung, Löhnen, Transferleistungen und der Teilnahme an AAMP Maßnahmen. Während die empirische Analyse in Kapitel 4 direkt auf diesen administrativen Daten basiert, wird in den übrigen Kapiteln auf Befragungsdaten des IZA Evaluationsdatensatzes zurückgegriffen. Dieser enthält eine Vielzahl von Informationen bezüglich Persönlichkeitsmerkmalen, des Arbeitssuchverhaltens, sowie persönlicher Erwartungen und Präferenzen für Personen mit Eintritten in Arbeitslosigkeit im Zeitraum Juni 2007 bis Mai 2008. Da insgesamt Beobachtungen für 4 Befragungswellen vorliegen bietet der Datensatz die ideale Basis zur Analyse des Arbeitssuchverhaltens. Darüber hinaus lassen sich die umfangreichen Befragungsdaten direkt mit den administrativen Aufzeichnungen verknüpfen welche detailgenaue Informationen zum Erwerbsverlauf liefern.

Kapitel 2 analysiert den Einfluss subjektiver Erwartungen auf die Effektivität von spezifischen Trainingsmaßnahmen. Diese Erwartungen betreffen die Wahrscheinlichkeit einer zukünftigen Programmteilnahme und den erwarteten Einfluss auf den Erfolg am Arbeitsmarkt. Traditionelle Evaluationsstudien fokussieren sich typischerweise auf den Vergleich von Teilnehmern und Nicht-Teilnehmern bezüglich des nachfolgenden Erfolgs am Arbeitsmarkt. Darüber hinaus sind in den letzten Jahren einige Studien erschienen, welche sich mit sogenannten Antizipationseffekten von Arbeitsmarktprogrammen beschäftigen. Diese bezeichnen eine Anpassung des Suchverhaltens von Arbeitslosen basierend auf der Erwartung einer bevorstehenden Programmteilnahme. Ziel der Analyse ist die Verknüpfung dieser beiden Literaturstränge. Es ist zu erwarten, dass die subjektive Einschätzung bezüglich der bevorstehenden Programmteilnahme auch den individuellen Erfolg am Arbeitsmarkt nach der Realisierung der Maßnahme beeinflussen. Dies ist der Fall, wenn das Verhalten der Arbeitssuchenden vor der Programmteilnahme ihr Verhalten während, beziehungsweise nach, der Maßnahme beeinflusst. Dieser intertemporelle Zusammenhang lässt sich beispielsweise durch Lerneffekte im Hinblick auf das Arbeitssuchverhalten erklären.

Die empirische Analyse zeigt, dass Trainingsmaßnahmen einen positiveren Einfluss auf Personen haben, welche die Wahrscheinlichkeit einer Programteilnahme bereits zum Eintritt in die Arbeitslosigkeit als hoch einstufen, verglichen mit Personen die diese Wahrscheinlichkeit als gering einschätzen. Im Gegensatz dazu besteht kein Zusammenhang zwischen den subjektiven Erwartungen bezüglich des Effekts der Maßnahme und dem tatsächlichen Arbeitsmarkterfolg von Teilnehmern. Eine umfassende Sensitivitätsanalyse zeigt, dass die Ergebnisse nicht durch andere beobachtbare und nicht-beobachtbare Charakteristika von Teilnehmern mit verschiedenen Erwartungen erklärt werden können. Dies betrifft im Besonderen die Fähigkeit den zukünftigen Arbeitsmarktstatus vorherzusehen, die individuelle Motivation und den Zeitpunkt des Programmstarts. Eine tiefgreifende Analyse des Suchverhaltens zeigt außerdem, dass die Erwartungen bezüglich der Teilnahmewahrscheinlichkeit mit diversen Unterschieden im Arbeitssuchverhalten verbunden sind. Zwar lässt sich kein Unterschied hinsichtlich des Suchaufwandes feststellen, allerdings wählen Personen mit einer hohen subjektiven Teilnahmewahrscheinlichkeit andere Suchmethoden als Personen mit niedriger Teilnahmewahrscheinlichkeit. Im Besonderen lässt sich feststellen, dass hohe Erwartungen bezüglich der Teilnahmewahrscheinlichkeit mit einer intensiveren Beratung durch die Arbeitsagentur und einer höheren Flexibilität der Suchstrategie verbunden sind. Darüber hinaus zeigt eine strukturelle Analyse des Prozesses der Erwartungsbildung, dass die individuelle Beratung durch die Arbeitsagentur Arbeitssuchende dazu anhält ihre Suchstrategien frühzeitig im Hinblick auf eine zukünftige Programmteilnahme anzupassen. Diese höhere Bereitschaft zur Anpassung des Suchverhaltens ist direkt mit den späteren Beschäftigungsaussichten von Teilnehmern in Trainingsmaßnahmen verbunden. Die Ergebnisse zeigen, dass Arbeitsagenturen die Effektivität von spezifischen Arbeitsmarktprogrammen direkt beeinflussen können, in dem potentielle Teilnehmer umfangreich über bevorstehende Maßnahmen und die allgemeine Arbeitsmarktlage informiert werden.

Des Weiteren beschäftigt sich ein wichtiger Teil der Dissertation mit einer Gruppe verschiedener Maßnahmen welche bisher nur geringe Beachtung in der ökonomischen Literatur fand. Ziel dieser sogenannten Mobilitätshilfen ist die Förderung regionaler Mobilität von Arbeitssuchenden. Im Speziellen, werden Reisekosten zu Jobinterviews, Fahrtkosten zum Arbeitsplatz oder Umzugskosten finanziell gefördert. Ziel dieser Maßnahmen ist es bestehende finanzielle Hindernisse zu beseitigen, welche die regionale Mobilität von Arbeitssuchenden einschränken. Theoretische Überlegungen legen nahe, dass die Verfügbarkeit dieser Maßnahmen Arbeitssuchende dazu anhält ihre Suchaktivitäten von lokalen hin zu regional entfernten Arbeitsmärkten zu verlagern. Zur Analyse des kausalen Effekts dieser Maßnahmen auf das Arbeitssuchverhalten und den nachfolgenden Erfolg am Arbeitsmarkt werden regionale Unterschiede in Bezug auf die Zuweisung dieser Maßnahmen ausgenutzt. Diese Unterschiede generieren exogene Variation hinsichtlich der Wahrscheinlichkeit, dass Arbeitssuchende über die Verfügbarkeit dieser Maßnahmen informiert werden und entsprechende Anträge genehmigt werden. Deswegen ist davon auszugehen, dass Arbeitssuchende, in Regionen in denen Arbeitsagenturen eine starke Präferenz für Mobilitätshilfen haben, öfter nach neuen Stellen suchen die einen Umzug erfordern und tatsächlich am Programm teilnehmen.

Die empirische Analyse in Kapitel 3 verwendet die oben beschriebenen Befragungsdaten für arbeitslose Personen in Deutschland um Einfluss der Mobilitätshilfen auf das

Arbeitssuchverhalten und den Übergang zu Beschäftigung zu untersuchen. Die Ergebnisse zeigen, dass die Verfügbarkeit des Programms Arbeitssuchende in der Tat dazu anhält sich auf Stellen zu bewerben welche einen Umzug erfordern und ihre Suchaktivitäten vom lokalen Arbeitsmarkt hinzu geografisch entfernten Regionen zu verlagern, ohne jedoch die Anzahl der Stellenbewerbungen insgesamt zu beeinflussen. Darüber hinaus führt der erweiterte Suchradius zu einer höheren Wiederbeschäftigungswahrscheinlichkeit, höheren Löhnen und zu einer geringeren Abhängigkeit von anderen Formen staatlicher Zuschüsse, wie zum Beispiel Gründungszuschüssen für Selbstständige. Aufbauend auf diesen Ergebnissen untersucht Kapitel 4 den Einfluss der tatsächlichen Nutzung einer spezifischen Mobilitätshilfe, der sogenannten Umzugskostenbeihilfe, auf den langfristigen Erfolg am Arbeitsmarkt. Dieses spezielle Programm bietet finanzielle Unterstützung für Arbeitssuchende die eine Beschäftigung in einer entfernten Region annehmen und hierfür temporär oder dauerhaft ihren Wohnort wechseln. Anspruchsberechtig sind grundsätzlich alle Arbeitssuchenden die eine Förderung benötigen. Voraussetzung ist das Vorliegen eines Stellenangebotes welches eine tägliche Fahrzeit von mindestens 2,5 Stunden zwischen dem ursprünglichen Wohnort und der neuen Stelle mit sich bringt. Ist dies der Fall werden entweder die vollständigen Umzugskosten durch die Arbeitsagentur übernommen oder Programmteilnehmer können eine monatliche Unterstützung von 300 € erhalten um eine zweite Wohnung am Ort des neuen Arbeitsplatzes anzumieten. Basierend auf detaillierten administrativen Daten wird empirisch gezeigt, dass sich die Programmteilnahme positiv auf die nachfolgenden Beschäftigungsaussichten auswirkt. Arbeitssuchende die zur Aufnahme einer neuen Beschäftigung umziehen und hierfür Unterstützung der Arbeitsagentur erhalten beziehen signifikant höhere Löhne und enden in stabileren Beschäftigungsverhältnissen. Es wird außerdem gezeigt, dass die positiven Effekte auf den nominalen Lohn tatsächlich durch eine höhere Arbeitsplatzqualität erklärt werden und nicht durch regionale Unterschiede im Lohn- und Preisniveau. Insgesamt zeichnen die Ergebnisse von Kapitel 3 und 4 ein sehr positives Bild über die Effektivität von Mobilitätshilfen in Deutschland bei der Eingliederung arbeitsloser Personen in den Arbeitsmarkt. Dies könnte auch von besonderer Bedeutung für andere europäische Länder sein, in welchen es typischerweise große regionale ökonomische Unterschiede gibt, gleichzeitig jedoch nur geringe eine Mobilitätsbereitschaft in der Erwerbsbevölkerung. Die Ergebnisse zeigen, dass der Abbau finanzieller Hindernisse diese Bereitschaft erhöht und gleichzeitig sowohl die kurzfristigen Wiederbeschäftigungsaussichten, als auch den langfristigen individuellen Erfolg am Arbeitsmarkt erhöht. Die Ergebnisse sind umso bemerkenswerter unter Berücksichtigung der relativ geringen Kosten des Programms im Vergleich mit anderen AAMP Maßnahmen. Beispielsweise verursachen Teilnehmer in Programmen zur beruflichen Weiterbildung durchschnittlich sechsmal höhere Kosten verglichen mit einem geförderter Umzug.

Kapitel 5 untersucht außerdem die Relevanz von typischerweise nicht beobachtbaren Variablen bezüglich Persönlichkeitsmerkmalen, subjektiven Erwartungen, sozialen Netzwerken und generationenübergreifenden Informationen, im Kontext der Evaluierung typischer AAMP Programme, wie Trainingsmaßnahmen und Lohnkostenzuschüssen. Ziel der empirischen Analyse ist die Beantwortung der allgegenwärtigen Frage im Bereich der Evaluation aktiver Arbeitsmarktpolitik, ob die vorhanden Daten alle Faktoren erklären können welche zum einen die Programmteilnahme und zum anderen den Erfolg am Arbeitsmarkt beeinflussen. Falls dies nicht der Fall ist, führen empirische Schätzungen basierend auf Annahme bedingter Unabhängigkeit (*conditional independence assumption*), wie zum Beispiel Propensity Score Matching, zu verzerrten Ergebnissen. Obwohl die Qualität administrativer Daten, welche üblicherweise zur Evaluation von Arbeitsmarktprogrammen herangezogen werden, in den letzten Jahren kontinuierlich gestiegen ist, legen neuere Erkenntnisse nahe, dass eine Vielzahl von zuvor nicht berücksichtigten Variablen, wie Persönlichkeitsmerkmalen oder Präferenzen, den individuellen ökonomischen Erfolg beeinflusst. Da zu erwarten ist, dass diese Variable ebenfalls das Arbeitssuchverhalten und die Selektion in Arbeitsmarktprogramme beeinflusst, bestehen erhebliche Zweifel hinsichtlich der Gültigkeit der zugrundeliegenden Annahmen diverser ökonometrischer Methoden.

Basierend auf der Kombination von administrativen Daten und individuellen Befragungsdaten zeigen die empirischen Ergebnisse in Kapitel 5 zwar, dass Variablen bezüglich Persönlichkeitsmerkmalen, subjektiven Erwartungen, sozialen Netzwerken und generationenübergreifenden Informationen, einen signifikanten Einfluss auf die Teilnahmewahrscheinlichkeit haben, ihre Nichtberücksichtigung aber zu keiner Verzerrung der geschätzten Effekte führt. Die Ergebnisse legen nahe, dass umfangreiche Kontrollvariablen bezüglich der individuellen Erwerbsbiographie und detaillierte administrative Daten ausreichen um Rückschlüsse auf die Effektivität von typischen Arbeitsmarktprogrammen zu ziehen. Dies ist insbesondere dann der Fall, wenn davon auszugehen ist, dass die typischerweise unbeobachtbaren Merkmale bereits die zurückliegende Erwerbsbiographie beeinflusst haben.

Abschließend wird in Kapitel 6 die Rolle von Reservationslöhnen bei der Entwicklung von geschlechtsspezifischen Lohnunterschieden untersucht. Der Reservationslohn bezeichnet in der Literatur üblicherweise den Lohn zu dem ein Arbeitssuchender gerade noch bereit ist ein Stellenangebot anzunehmen. Es wird die Fragestellung untersucht ob sich geschlechtsspezifische Lohnunterschiede auf Unterschiede im Reservationslohn zurückführen lassen. Es ist zu erwarten, dass die empirische Analyse der Fragestellung wichtige Einblicke für die Erklärung und Wirkungsmechanismen der realisierten Lohnunterschiede zwischen Männern und Frauen liefert. Dies ist von besonderem Interesse, da die potentiellen Erkenntnisse Aufschluss darüber liefern wie die Ausgestaltung der Arbeitsmarktpolitik effektiv bei der Beseitigung geschlechtsspezifischen Lohnunterschiede mithelfen kann.

Die empirische Analyse zeigt, dass der realisierte Lohnunterschied zwischen Männern und Frauen einhergeht mit einem geschlechtsspezifischen Unterschied in Reservationslöhnen welcher nicht durch Faktoren wie Bildung, Unterschiede in der Erwerbsbiografie, soziodemographische Eigenschaften und Persönlichkeitsmerkmale, die typischerweise zur Erklärung von Lohnunterschieden herangezogen werden, erklärt wird. Darüber hinaus liefert eine umfassende Subgruppenanalyse Erkenntnisse über die Hintergründe zur Entstehung der geschlechtsspezifischen Unterschiede in Reservationslöhnen. Die Ergebnisse deuten darauf hin, dass unterschiedliche Tätigkeitsvorlieben, der Erwartung von Diskriminierung gegenüber Frauen und unbeobachtbaren Produktivitätsunterschiede sowohl Unterschiede im Reservationslohn als auch im realisierten Lohn zwischen Männern und Frauen hervorrufen. Dies legt nahe, dass spezielle arbeitsmarktpolitische Maßnahmen gerechtfertigt sind um diese geschlechtsspezifischen Unterschiede zu beseitigen.

Zusammenfassend lässt sich festhalten, dass die Dissertation neue Einblicke über das Zusammenspiel des Suchverhaltens arbeitsloser Personen und den Wirkungsmechanismen aktiver Arbeitsmarktpolitik liefert. Dies ist von besonderer Bedeutung, da sich traditionelle Evaluationsstudien typischerweise auf die Betrachtung des nachfolgenden Erfolgs am Arbeitsmarkt beschränken und häufig keine zufriedenstellenden Erkenntnisse über die tatsächlichen Wirkungsmechanismen aktiver Arbeitsmarktpolitik liefern. Die Ergebnisse zeigen, dass ein besseres Verständnis dieser Wirkungsmechanismen eine effektivere Ausgestaltung der Arbeitsmarktpolitik zulässt. Wie in Kapitel 2 gezeigt wird, bietet sich Arbeitsagenturen die Möglichkeit durch bessere Informationspolitik die Effektivität von Trainingsmaßnahmen zu erhöhen. Im Zusammenhang damit offenbaren die Ergebnisse in Kapitel 3 und 4, dass sich durch Informationen hinsichtlich innovativer AAMP Programme, wie in diesem Fall Mobilitätshilfen, das Verhalten von Arbeitssuchenden direkt beeinflussen lässt und dies zu positiven Effekten auf den langfristigen Erfolg am Arbeitsmarkt führt. In diesem Zusammenhang deuten jedoch die Ergebnisse von Kapitel 5 daraufhin, dass Informationen bezüglich Persönlichkeitsmerkmalen, Erwartungen, sozialen Netzwerken und generationenübergreifenden Eigenschaften, welche erwartungsgemäß mit der Selektion in bestimmte AAMP Programme verknüpft sind, keinen Einfluss auf die Schätzung der Effekte dieser Programme hat. Abschließend zeigt Kapitel 6, dass geschlechtsspezifische Unterschiede im Suchverhalten existieren welche die Einführung spezieller arbeitsmarktpolitischer Maßnahmen für Frauen rechtfertigen.

Curriculum Vitae - Robert Mahlstedt

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