The Return to Labor Market Mobility: An Evaluation of Relocation Assistance for the Unemployed

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Abstract

Supporting regional mobility among the unemployed might be an effective instrument to reduce unemployment in depressed regions and eliminate the shortage of labor in prosperity areas. Using German administrative data we investigate the impact of mobility assistance for unemployed individuals on labor market prospects of participants. In fact, we focus on relocation assistance, a program which pays a subsidy if the unemployed moves its place of residence in order to find a job. To take into account endogenous selection into treatment we use the treatment intensity for a local employment agency as an instrumental variable to estimate causal treatment effects. We find mixed results for the effect on the unemployment duration, while participants receive higher wages and end up in more stable jobs.

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1 Introduction

High regional disparities in the unemployment rate are common to many European countries (Taylor and Bradley, 1997; Giannetti, 2002). Supporting regional mobility among the unemployed might be an effective instrument to reduce unemployment in depressed regions and eliminate the shortage of labor in prosperity areas. The idea is to bring unemployed individuals who are not able to find employment locally to other regions. One possible way to do this, is to pay unemployed individuals a subsidy if they move regionally in order to take up employment. Considering the classical job search model from Mortensen (1986) extended by Rogers (1997) with respect to job distance such a subsidy is expected to have three effects. First of all, it would reduce the individual costs of moving which in turn should increase job-seeker's search radius and hence increase the job offer arrival rate. Furthermore, the subsidy reduces the commuting costs of the job-seeker, which lowers ceteris paribus the individual reservation wage.[1] The lower reservation wage in turn has a positive effect on the employment probability.

Moreover, the individuals are expected to move predominately to areas characterized by better economic conditions, so that in total the relocation is expected to improve participant's labor market outcomes in terms of employment probability and wages. To evaluate the effectiveness of such a policy we take Germany as a case study. Although Germany is confronted with increasing demand for mobile workers and there is clear evidence for a positive effect of inter-regional mobility on individual income (e.g. Lehmer and Ludsteck, 2011), it is still characterized by relatively low willingness to change the place of residence in order to leave unemployment. It is much more acceptable to change profession, work in jobs below workers' technical skills or have high commuting distances (Brixy and Christensen, 2002). According to the German Socio-Economic Panel even 45% of people between 20 and 59 years have lived in the same region as their parents since birth (Schneider, 2005). To improve the inter-regional labor mobility, as part of active labor market policy (ALMP), the unemployed are offered a subsidy if they are successful to find an employment outside their local labor market. We investigate the impact of relocation assistance for unemployed job-seekers on the labor market prospects of participants. The relocation assistance is a subsidy that reimburses costs associated with temporary or permanent moving.

[1] The commuting can either be supported directly or a relocation reduces the distance to potential job.

Descriptive statistics show that participants in relocation assistance move to regions with lower unemployment rates, are more likely to be employed and receive higher income than non-participants. As participation in the program is likely to be correlated with observed and unobserved characteristics, like motivation and ability, we adopt an instrumental variable approach to identify the causal treatment effect.[2] We use the treatment intensity within the hosting local employment agency district as an instrument for the treatment participation. Thereby, the treatment intensity is defined as the ratio of total entries into mobility assistance and the stock of unemployed. Each local employment agency (LEA) faces a fixed budget (based on the local labor market conditions) and decides at the beginning of each year which share of this budget is spend on the different types of active labor market policies, i.e., their individual policy mix. The idea is that unemployed individuals living in a LEA district with relatively high treatment intensity face a higher probability to receive knowledge about the existence of the subsidy. As the unemployed individual has no influence on the policy mix of its local employment agency, the instrument generates exogenous variation in treatment participation.

Using this instrumental variable approach, we find that participation in the relocation assistance leads to higher wages and more stable jobs, while the results are mixed with respect to the short-term employment prospects. The next section gives an overview about the institutional settings in Germany and summarizes related literature. Section 3 provides information about the data, describes the treatment group and provides descriptive statistics. Section 4 discusses the identification and estimation strategy. Finally, Section 5 presents the results and Section 6 concludes.

2 Institutional Settings and Related Literature

2.1 Mobility Assistance in Germany

Given a fixed budget and a variety of measures each local employment agency (LEA) decides autonomously about their individual policy mix of ALMP programs.[3] The responsibility of a certain LEA for an applicant results from the initial place of residence. One of these ALMP programs is the mobility assistance as introduced in 1998. This can assigned

[2]For example, better motivated individuals are more willing to change their place of residence in order to find better jobs.

[3]The federal employment agency (FEA) allocates a fixed budget to each LEA based on the local labor market conditions.

to unemployed and job-seekers who are threaten of unemployment and supports initiation and affiliation of an employment subject to social insurance contributions. The term mobility assistance contains several instruments, like transition, equipment, traveling and commuting assistance. However, in our analysis we focus on relocation assistance, which reimburses costs associated with temporary or permanent moving.

The relocation assistance supports temporary housekeeping with a monthly payment of €260 for a period of maximal 6 months, while the subsidy for permanent moving covers only the pure transportation costs. The applicant has to provide three cost estimates to find the most cost-efficient offer and the subsidy is paid directly to the removalists. Alternatively, also the costs of a rental car can be taken on. The permanent relocation has to occur at least two years after the beginning of the new employment. There is no subsidy if the employer provides an accommodation. Both types of subsidy are only available if the daily commuting to the employment is not appropriate, in fact the commuting time exceeds 2.5 hours per day.[4]

In order to being eligible to the relocation assistance, the unemployed has to start a new employment. Without a legal employment contract no subsidy will be paid. Furthermore, the application has to take place before the causing event, the relocation, takes place. An ex post permission is not possible. Every decision about the permission of relocation assistance is taken by the case worker. The mobility assistance can also accord if the new employment is abroad, but not to occupational groups typically working in foreign countries, like animators, professional athletes or artists. Employees of domestic firms working abroad will be also supported. As job creation schemes (JCS) are also subject to social security contributions, participants are also eligible to apply for such a subsidy. However, as we are interested in the effect on unsubsidized employment, we exclude all participants in JCS who received a mobility assistance.

2.2 Similar Programs

Considering similar programs there is only few support for the returns of mobility assistance on labor market outcomes. However, Briggs and Kuhn (2008) analyze the Relocation Assistance Program (RAP) in Kentucky introduced in May 1998. A lump sum payment up to $900 is awarded to households of welfare recipients if they accept a job offer within

[4]If the daily commuting time is less than 2.5 hours the daily commuting can be supported with 20 cent per kilometer for a period of 6 months after the beginning of the new employment.

90 days of the date of request. This employment must be at least 10 miles away from their place of residence and contains not less than 30 hours of work per week at the minimum wage (5.85 $ per hour). The participation in the program requires that the welfare clients have a confirmed job offer, so the beginning of a new employment is endogenously given with the receipt of relocation assistance. To avoid this problem they use the treatment intensity in a certain county as an instrumental variable to estimate the treatment effect on the employment status and the offered wages. They find a positive significant effect on the mean quarterly employment and also on the unconditional earnings. However, the results are ambiguous considering the earnings conditional on employment.

In 1994, the U.S. Department of Housing and Urban Development starts the Moving to Opportunity Program (MTO) in the five metropolitan areas Baltimore, Boston, Chicago, Los Angeles and New York City. This is a randomized experiment, where housing vouchers are offered to low-income families. The aim of this program was to give these families the opportunity to move to better neighborhoods and therefore increase their health status, the educational opportunities of the children and the labor market outcomes. A variety of studies, like Katz, Kling, and Liebman (2001), Kling, Liebman, and Katz (2007) or Ludwig and Kling (2007) investigate the effectiveness of this program. The MTO successfully relocates families to less economically distressed regions and improve the overall health status of the adult family members. The MTO also increases the health status of female youths, but has deleterious effects on male youths. However, there is no significant effect with respect to educational outcomes or the labor market outcomes, like wages, employment rate or the welfare usage.

3 Data, Treatment and Descriptive Statistics

3.1 Data

This study uses the IZA Evaluation Dataset A, which was created by IZA with financial support of the Deutsche Post Foundation and consists of Integrated Employment Biographies (IEB), Version 9.01, from the Institute for Employment Research (IAB). The dataset consist of individuals who entered unemployment between January 2001 and December 2008 in Germany (see Caliendo, Falk, Kaiser, Schneider, Uhlendorff, van den Berg, and Zimmermann, 2011). The IEB are based on different data sources, the social security insurance and the unemployment register, including benefit and ALMP participation history.

Therefore, the data contain detailed information about the employment status, wages, benefits and different individual characteristics, like gender, age, educational level, health and family status. For the baseline estimation we draw a random sample of entries into unemployment in 2006, to avoid direct influence by a major reform of the labor market in 2005 in Germany.[5]

We observe the monthly employment status of each individual over a period of 4 years until 2010. All unemployment spells shorter than 2 weeks are excluded and before the entry the individuals have to be employed subject to social security for a period of minimum 3 months. The sample contains individuals from East- and West-Germany, between 25 and 55 years. We consider two labor market states, regular employment and unemployment. The unemployment state refers to being registered unemployed with or without benefit receipt including individuals in ALMP. Regular employment is defined as employment subject to social security contributions with an income more than €600 per month. Low-income workers are excluded, since they often increase their income with unemployment assistance and cannot clearly assign to one of two labor market states. Since receipt of relocation assistance is connected to the beginning of a new employment we restrict our main sample on individuals, who start a new employment within the first 24 months after the beginning of the unemployment spell.[6] We further exclude job-seekers, whose initial unemployment spell is interrupted by missing information lasting more than one month.

3.2 Definition of the Treatment Group

Due to data restrictions the treatment is not directly assignable to the initial transition to regular employment. Therefore, we set the following design presented in Figure 1. First of all, we take a random sample of entries into unemployment in 2006 and restrict this sample to all individuals who find an employment within the first 24 months after the entry. The entry into unemployment is denoted by t_0 and the month of the transition to regular employment by t_{ue}. This restriction is due to the fact that eligibility to the program is connected to the beginning of a new employment.[7] To ensure that a certain

[5]For the verification of our estimation approach we also use entries into unemployment from 2005 to 2008. Descriptive statistics for this sample are given in the appendix.

[6]Since all participants begin a new employment, by the institutional settings of mobility assistance, we only use non-participants who also start a new employment. Taking the first 24 months after the transition as an interval seems to be appropriate, since about 98% start the employment within this interval.

[7]Excluding individuals who do not find an employment reduces the impact of unobserved characteristics that influence the selection into treatment and employment simultaneously.

subsidy is related to the initial transition we define an individual as treated if the payment of the subsidy takes place within in a window of six months before and after the transition (denoted by the shadowed area in Figure 1) and the payment has to occur before the end of the subsequent unemployment spell.[8]

Finally, we exclude all recipients of relocation assistance who do not change their residential location from the entry into unemployment to the beginning of the new employment. The control group contains all individuals entering unemployment in 2006, beginning a new employment within the first 24 months and receive no relocation assistance associated with the first transition from unemployment to regular employment. Table 1 shows the change of the three groups after each of our previous restrictions. We end up with 433 participants in relocation assistance and 35524 non-participants.

[Insert Table 1 and Figure 1 about here]

3.3 Descriptive Statistics

Table 2 shows descriptive statistics with respect to observed outcomes separated for participants and non-participants. Participants in relocation assistance receive higher wages when they start a new employment (€77.92 vs. €60.92) and have less job quits within the first 24 months after the transition (1.26 vs. 1.97). However, there are only small differences with respect to the duration of the initial unemployment spell and the employment probability 48 months after the entry into unemployment. The duration of the initial unemployment spell is even slightly shorter for non-participants. Furthermore, we compare the working location at the entry into unemployment and that directly after the transition to regular employment.[9] Unsurprisingly, participants in relocation assistance are more likely to work in distant regions. 46% of the participants work in a non-bordering federal state, while only 4% of the non-participants do. Only 26% of the participants work in the same federal state as they lived before, while 88% of the non-participants do. Comparing the local macroeconomic conditions before and after the transition, we observe that participants live initially in region which perform slightly worse with respect to the unemployment and the vacancy rate, but they move predominately to regions characterized by better economic conditions.

[8] We explore different settings and this one seems to be appropriate to reduce the bias due to wrong assignment of the treatment and maximize the size of the treatment group.

[9] The working location at the entry into unemployment is given by the employment office in charge.

[Insert Table 2 about here]

Table 3 shows differences with respect to observed characteristics. Participants are more likely to be female (41% vs. 33%) and younger (36.13 years vs. 38.69 years) than non-participants. Moreover, participants are generally better educated. We observe more individuals visiting an upper secondary school (48% vs. 18%) and a higher share with an university degree (32% vs. 18%). Unsurprisingly, the participants have less often children and a lower share of married individuals.[10] Furthermore, participants in relocation assistance are more likely to work in technical occupations before the unemployment and receive a substantially higher previous income (€65.03 vs. €55.16) per day.

[Insert Table 3 about here]

4 Estimation Methods

4.1 Identification in a Latent Variable Framework

We analyze the effect of relocation assistance in context of a Roy-Rubin model (Roy, 1951; Rubin, 1974) for two potential outcomes (Y_{0i}, Y_{1i}) (see Amemiya, 1985; Heckman, 2001). Y_1 denotes the outcome in the treated and Y_0 in the untreated state, while the dummy variable D_i indicates the receipt of the treatment. The observed outcome is given as a linear combination treated and non-treated outcomes: $Y_i = D_i Y_{1i} + (1 - D_i)Y_{0i}$. In general, we are interested in the average treatment effect $E[Y_1 - Y_0]$(ATE) or the average treatment effect on the treated $E[Y_1 - Y_0|D_i = 1]$(ATT). An identification problem arises, since only one of the two potential outcomes is observed for each individual, while the counterfactual outcome is missing. Thus, comparing the average outcomes of treated and non-treated does generally not give the causal treatment effect:

$$\begin{aligned} \text{ATT} &= E[Y_1|D = 1] - E[Y_0|D = 0] \\ &= E[Y_1 - Y_0|D_i = 1] + E[Y_0|D = 1] - E[Y_0|D = 0]. \end{aligned} \tag{1}$$

The true value of the ATT is only identified if $E[Y_0|D = 1] = E[Y_0|D = 0]$, which means the selection into the treatment has to be random. An assumption which is mostly violated

[10] The relocation costs, which are not captured by the assistance, are substantially higher for married job-seekers and job-seekers with children.

in non-experimental studies. We assume that the selection process into the treatment is determine by a latent variable D_i^*, with

$$D_i = 1[D_i^* = \mu_d(Z_i) - U_i \geq 0], \tag{2}$$

where Z_i is a vector of observed and U_i of unobserved random variables. The latent variable D_i^* can be interpreted as the net utility from the treatment and the unemployed chose the treatment if this net utility is positive. The outcome variable Y_i is given as a function of the treatment dummy D_i, the observable characteristics X_i and the unobservable characteristics V_i:

$$Y_i = \mu_y(D_i, X_i, V_i), \tag{3}$$

where X_i denotes the observable characteristics affecting the outcome, D_i the treatment status and V_i the unobservable error term. If the selection into the treatment is only based on observable characteristics, OLS yields consistent estimates of the ATT. However, OLS is biased if the selection takes place based on unobservable characteristics that are correlated with the outcome variable. Therefore, we assume that Z_i contains at least one element that effects the treatment decision but not the outcome. This is called the instrumental variable. There is a variety of studies using instrumental variables to detect causal treatment effects (e.g. Heckman and Robb, 1985; Imbens and Angrist, 1994; Heckman and Vytlacil, 1999). Following Imbens and Angrist (1994) a valid instrument has to fulfill the following conditions:

(i). *Independence:* For all elements W in the support of Z, the triple $(Y_{0i}, Y_{1i}, P(W))$ is jointly independent of Z_i. The instrumental variable is not allow to have any effect on the outcome variable Y_i other than trough the effect on the treatment probability $P_i(W)$.

(ii). *Relevance:* The treatment probability $P_i(W) = Pr(D_i = 1|Z_i = W)$ is a nontrivial function of W. So, the instrumental variable creates exogenous variation of the participation probability.

(iii). *Monotonicity.* For all $Z_i, W_i \in \mathfrak{R}$, either $P_i(Z) \geq P_i(W)$ for all i, or $P_i(Z) \leq P_i(W)$ for all i. This ensures that the no sign reversal property.[11]

[11] When the level of the treatment is a monotonic increasing function of the instrument, than the sign of the treatment is identified.

Assumption (i) guarantees that the error term is uncorrelated with the instrumental variable. Assumption (ii) ensures that the instrument creates exogenous variation and assumption (iii) the no sign reversal property. Furthermore, we adopt parametric assumptions about the distribution of the error terms.[12] We assume that the error terms U_i and V_i are jointly normal distributed (see Heckman, 1978), with the correlation ρ. To identify the causal treatment effect δ we estimate the treatment equation and the outcome equation simultaneously with Maximum Likelihood (see Appendix A.2 for a comparison of the standard Two-Stage Least Square and the Maximum Likelihood estimator). Assuming a linear relation for continuous outcome variables the corresponding log-likelihood function is given as

$$\ell = \sum_{i=1}^{N} D_i \left\{ \log \Phi \left[\frac{-\gamma Z_i + (Y_i - X_i\beta - \delta)\rho/\sigma_v}{\sqrt{1-\rho^2}} \right] - \frac{1}{2} \left(\frac{Y_i - X_i\beta - \delta}{\sigma_v} \right)^2 - \log(\sqrt{2\pi}\sigma_v) \right\}$$
$$+ (1 - D_i) \left\{ \log \Phi \left[\frac{-\gamma Z_i (Y_i - X_i\beta)\rho/\sigma_v}{\sqrt{1-\rho^2}} \right] - \frac{1}{2} \left(\frac{Y_i - X_i\beta - \delta}{\sigma_v} \right)^2 - \log(\sqrt{2\pi}\sigma_v) \right\}. \quad (4)$$

For the case of binary outcome variables we assume also a non-linear form of the outcome equation and the log-likelihood function shifts to that of the bivariate probit model:[13]

$$\ell = \sum_{i=1}^{N} \{ D_i \{ Y_i \log \Phi_2(X_i\beta^* + \delta, \gamma Z_i; \rho) + (1 - Y_i) \log[\Phi(\gamma Z_i) - \Phi_2(X_i\beta^* + \delta, \gamma Z_i; \rho)] \}$$
$$+ (1 - D_i) \{ Y_i \log[\Phi(X_i\beta^*) - \Phi_2(X_i\beta^*, \gamma Z_i; \rho)]$$
$$+ (1 - Y_i) \log[1 - \Phi(X_i\beta^*) - \Phi(\gamma Z_i) - \Phi_2(X_i\beta^*, \gamma Z_i; \rho)] \} \}. \quad (5)$$

4.2 The Treatment Intensity as Instrumental Variable

Our analysis focuses on the treatment effect of the relocation assistance on several labor market outcomes. The short-term employment prospects are measured as the duration

[12] Nowadays, there are several non-parametric IV approaches that relax these distributional assumptions (e.g. Heckman and Vytlacil, 2005; Abadie, 2003). However, Heckman and Vytlacil (1999) show that the identification of the treatment effect depends on the support of the treatment probability $P_i(W) = Pr(D_i = 1|Z_i = W)$. In our approach, the treatment probability has only small support due to the small size of the treatment group, which would result in inconsistent estimates without the distributional assumptions (see also Chiburis, Das, and Lokshin, 2012).

[13] Therefore, the cdf of the joint normal distribution of U and V is given as

$$\Phi_2(U, V, \rho) = \int_U \int_V \frac{1}{2\pi\sigma_v\sigma_u\sqrt{1-\rho}} \exp\left[-\frac{1}{2} \left(\frac{U^2 + V^2 - 2\rho UV}{1-\rho^2} \right) \right] dU\, dV.$$

of the initial unemployment spell, while the number of job quits, the first wage and the employment status 24 months after the initial transition to regular employment are measures for the job match quality. Since all of these outcomes are likely to be correlated with unobservable characteristics that also influence the treatment decision, like motivation or unobservable abilities, we use the treatment intensity of a certain (LEA) as instrumental Variable. We define the treatment intensity as the ratio of entries into mobility assistance and the stock of unemployed in the LEA district j:[14]

$$z_j = \frac{N_j^{ma}}{N_j^{ue}} \times 100, \tag{6}$$

where N_j^{ma} denotes the number of recipients of mobility assistance in 2005 and N_j^{ue} denotes the average stock of unemployed in the LEA district j in 2005, the year before the entry into unemployment. Similar regional variations are used by Briggs and Kuhn (2008), Frölich and Lechner (2010) and Card and Krueger (1993) as instrumental variables. Briggs and Kuhn (2008) use the ratio of recipients and eligible individuals in a certain county to evaluate the effect of the Relocation Assistance Program in Kentucky. Frölich and Lechner (2010) defines the treatment intensity as the ratio of the minimum quota of treated individuals and the number of unemployed in Swiss cantons to evaluate the impact of ALMP.

To ensure that the treatment intensity is independent conditioned on the control variables, we use the fact that each LEA has to decide at the beginning of a year their individual strategies and on which share of their fixed budget to spend on the different types of active labor market policy (see Blien, 1998; Yankova, 2010). The budget of each LEA depends on the local labor market conditions but the allocation of the different types of active labor market policy is due to the preferences of the LEA. Assuming the LEA complies with their preassigned strategy the treatment intensity in a certain LEA district is independent of the demand for mobility assistance and thus also with the unobserved individual characteristics. If the LEA adjusts the awarding of mobility assistance with respect to the demand the treatment intensity is still independent as long as the regional variation of the demand for the subsidy is not caused by differences in the distribution of the unobservable characteristics. To induce further exogeneity use the treatment intensity

[14]Since the German Social Security Code combines several instruments in the term mobility assistance, which are subject to similar restrictions, we use the entries into all types of mobility assistance, not only relocation assistance.

in 2005, the year before the entry into unemployment. Therefore, our estimation sample is not a sub-sample of the stock of unemployed which creates the instrumental variable.

To satisfy the relevance and monotonicity assumption, we argue that the treatment probability is positive function of the treatment intensity, since one of the driving factors for the permission of an assistance is the caseworker in charge. Since the caseworkers are highly influenced by the preassigned strategies of the LEA, individuals who live in districts with higher treatment intensities are more likely to be informed about the existence of mobility assistance and also to make use of it. Figure 2 shows the geographical distribution of the overall treatment intensity in Germany and the realized number of participants in commuting and relocation assistance in our estimation sample. There is a high geographical heterogeneity in the distribution of the treatment intensity, which translates into participation in our estimation sample. Considering for example the Berlin-Brandenburg area we find very large difference with respect to the local treatment intensities. Job-seekers in Berlin and the bordering areas of Brandenburg has entrance to the same labor market but are managed by different employment agencies with treatment intensities between 1.55% for Berlin North and 6.95% for Potsdam. Therefore, job-seekers in Potsdam has a higher probability to be informed about the subsidy and also to participate, which is independent of the unobservable characteristics. Furthermore, we include variables for the local unemployment rate and for individuals living in East-Germany to control for heterogeneity of the treatment intensity which is caused by differences in the local labor market conditions (see also Appendix A.1 for the results of the first stage estimation of the participation probability).

[Insert Figure 2 about here]

5 Results

5.1 Baseline Results

Table 4 shows the baseline estimates for our main sample of individuals entering unemployment in 2006 and beginning a new employment within the first 24 months. For each outcome we provide the OLS estimates controlling for socio-demographic characteristics, the short- and long-term labor market history, individual benefit conditions and the local

macroeconomic conditions.[15] Additionally, we adopt our instrumental variable strategy from Section 4 to control for unobserved heterogeneity.

The unemployment duration measures the interval between t_0 and t_{ue} (see Figure 1). Using the standard OLS estimator there is positive significant effect of relocation assistance, which disappears by controlling for unobserved heterogeneity. The other outcomes describe the quality of the job match. The first wage denotes the first daily income from regular employment after the transition in period t_{ue}. Due to the restriction on individuals beginning a new employment within 24 months after the transition we observe this outcome for all individuals in our estimation sample and do not have to concern about sample selection issues. For OLS, as well as for the instrumental variable estimation we observe a strong positive and significant effect of relocation assistance on individual earnings, which is even stronger after controlling for unobserved heterogeneity (17% vs 25%). The number of job quits is measured between the transition t_{ue} and the end of our observation period 24 months later. Using OLS, we find a negative and significant effect of -.4, which increases up to -.9 by using IV. Regarding the employment status 48 months after the entry into unemployment, there is no effect using OLS but a significant effect of about 21% after controlling for unobserved heterogeneity.

[Insert Table 4 about here]

We interpret the results the following way. Conditioned on the observable characteristics, there is a selection into the program of individuals with lower unobserved abilities. This explains why controlling for unobserved heterogeneity increases the positive effects of the subsidy with respect to all outcomes.[16] However, there is no positive effect on the initial unemployment duration, but after the transition participants receive higher wages and end up in more stable jobs.

5.2 Unobserved Regional Heterogeneity

So far, we use the treatment intensity in 2005 as an instrument for participation in relocation assistance for individuals entering unemployment in 2006. We argue that each LEA

[15] For the post transition outcomes (first wage, number of job quits and employment status after 48 months) we include the duration of the initial unemployment spell, while we do not for the unemployment duration itself.

[16] Considering for example participants and non-participants with the same unobserved characteristics there is no difference with respect to the unemployment duration while we observe longer unemployment durations for participants conditioned only on observable characteristics. Therefore, the actual participants must have lower unobserved abilities.

decides independently, about their individual policy mix. However, the treatment intensity might be determined by the interaction of supply and demand for mobility assistance. There might exist unobservable regional differences which influence the local demand for mobility assistance and the labor market outcomes simultaneously. Assuming the LEA's adjust their policy mix with respect to the demand for the subsidy and the unobservable differences are correlated over time, the treatment intensity in 2005 is no longer independent of the labor market outcome of the individuals entering unemployment in 2006. However, if there is no time-dependence or the LEA defines the policy mix independent of the demand for relocation assistance the treatment intensity in 2005 is independent of the labor market performance and the previous approach is consistent.

To overcome this potential endogeneity issue we adjust our estimation approach the following way. We draw a random sample of entries into unemployment between 2005 and 2007. Conditioned on the responsible LEA at the entry into unemployment, we assign each job-seeker the local treatment intensity in the year before the entry as instrumental variable. For example, a job-seeker entering unemployment in the LEA district A in 2006 is assigned the treatment intensity in district A in 2005 as instrument. To control for potential regional unobserved heterogeneity which is constant over time, we include regional fixed effects in both estimation equations. The first stage estimation of the treatment participation changes to

$$D_i = 1[D_i^* = \mu_d(Z_i, \gamma_k, \eta_t) - U_i \geq 0] \tag{7}$$

and the outcome equation to

$$Y_i = \mu_y(D_i, X_i, \gamma_k, \eta_t, V_i). \tag{8}$$

Therefore, γ_k denotes the regional fixed effect and η_t the fixed effect of the year of entry.[17] The regional fixed effects capture that part of the treatment intensity which is driven by time-constant unobserved regional heterogeneity and is potentially correlated with the labor market outcomes.

Table 5 shows the corresponding estimation results. Unfortunately, we observe the individuals only until the of 2010, which reduces the observation period for individuals entering unemployment in 2007 to 36 months. Therefore, we observe for these individuals

[17] Again, we assume a linear relation for continuous outcome variables and a non-linear relation for binary outcome variables. The log-likelihood functions from equation 4 and 5 changes by including the fixed effects.

only the unemployment duration and the first log wages as outcome variables, and not the post-transition outcomes. For this reason we use the full sample of individuals entering unemployment from 2005 to 2007 for the estimation treatment effect on the unemployment duration and the first log wage, while we use only individuals entering unemployment in 2005 and 2006 to estimate the effect on the post-transition outcome.[18] For each outcome variable, we provide estimation results with and without fixed effects for the responsible LEA district at the entry into unemployment. Comparing the estimation results with and without regional fixed effects, we observe significant differences for all outcome variables (see Hausman test). However, the fixed effect estimators of the treatment effect on the unemployment duration, first log wages and the number of job quits are very close to our baseline IV estimates from Table 4. There is still no significant effect on the unemployment duration, a positive and significant effect of about 25% on the first wages and a negative and significant effect on the number of job quits. Nevertheless, there is no longer a significant effect on the long-term employment probability 48 months after the entry into unemployment.

[Insert Table 5 about here]

5.3 Heterogeneous Treatment Effects and Wage Premiums

Regarding the previous results, the relocation assistance seems to be a quite effective instrument to bring unemployed individuals into a job, which is already better paid and more stable than jobs started without the subsidy. However, in contrast to other active labor market policies, the subsidy is assigned only to a small number of unemployed. A reason for this relatively low take up rates, is the fact that the act relocation causes monetary and non-monetary costs beyond the scope of the subsidy. Such costs arise with the settling in a new social environment. The participants need to establish a new social environment, life partners need to find a new employment and children have to change school. Therefore, job-seekers must receive some sort of premium, which compensates them for this additional costs, for having incentives to participate in the treatment. The size of these additional costs, and hence the size of the premium, is likely to differ with respect to socio-demographic characteristics, like family status, educational level or gender.

[18] Since including regional fixed effects weaken the explanatory power of the local treatment intensity using several years of entries increases the efficiency of the model. However, our results suggest that using only entries in 2005 and 2006 still achieve satisfying results (F-statistic larger than 10).

Table 6 shows the size of the wage premium for different subgroups.[19] It is conceivable that some part of the wage premium is purely determine by the improvement of the better labor market conditions, while another part depends on the individual claim for a wage premium. Therefore, we include in column (2) the local unemployment and vacancy rate at the new workplace as control variables, to distinguish the two effects. We expect that the presence of a partner, and even more that of children, increases the relocation costs.[20] Therefore, we run the instrumental variable estimation for singles, individuals with children and individuals with a partner and children. Having children increases the wage premium about 5%, while having a partner and children actually increases the wage premium about 10% compared to singles.

Furthermore, we expect some heterogeneity with respect to the educational level. Assuming that the relocation costs do not increase with the wage level, high qualified workers need a lower share of their income as compensation for the non-pecuniary relocation costs. There are only small differences between, individuals holding a (spec.) upper secondary school degree and those who do not. Considering only individuals with a university degree we find a negative and significant treatment effect of 58%. Therefore, we observe a very strong positive selection into treatment.[21] We expect that this selection is driven by the field of study, which is important regarding only individuals with a university degree but unobserved in our data. We conclude that individuals holding a degree in a technical field receive higher wages and are more likely to receive relocation assistance. However, comparing them to non-participants with the same unobserved characteristics they obtain substantially lower wages.

Moreover, Schneider, Limmer, and Ruckdeschel (2002a) show that women are overall less often occupational mobile than men. These differences can be explained mostly by socio-demographic factors like family condition, partnerships and children, which restricts women's mobility more than men's (Schneider, Limmer, and Ruckdeschel, 2002b; Jürges, 2005). Hence, we run the estimations separated by gender. Men receive a 4% higher premium for relocation than women (23% vs. 19%). We expect that this effect by the dif-

[19] It also conceivable that the premium shows up in a higher job stability, but in general the job stability is not an object of the employment contract, while the wage is indeed.

[20] A partner may have own career plans and own social networks, which are affected by the decision to relocate. Additionally, having children makes the relocation more costly, since they need to settle in a new social environment and a new school class. The latter is even more problematic given the heterogeneity of the German school system.

[21] Using OLS, we observe a positive treatment effect of 12%.

ferent gender roles. After becoming mobile women are more likely to being still involved in housework and care-giving. Therefore, they are expected to being less productive, which explains why female participants in relocation assistance receive a lower wage premium than male. Bonnet, Collet, and Maurines (2007) argue that these differences in the distribution of work are likely to be caused by the tendency that men hold the stronger position for negotiations within the couple. The couple decision about the relocation illustrates these inequalities. Indeed, these differences in the labor mobility can also caused by the maximization of the couple's utility rather than individual negotiation (Badoe, 2002).

[Insert Table 6 about here]

6 Conclusion

In this paper, based on a random sample of German entries into unemployment, we are interested in the causal effect of the relocation assistance on the unemployment duration and the job match quality. Since the participation in the treatment is likely to be correlated with unobserved factors, like motivation and abilities, we use the local treatment intensity as instrumental variable. Due to the relatively small size of the treatment group parametric assumptions about the distribution of the error terms are necessary to identify the causal treatment effect. In contrast to our expectations, there is no effect of the relocation assistance on the unemployment duration. However, participants in the relocation assistance receive higher wages and end up in more stable jobs. Moreover, our estimation approach seems to be quit robust against unobserved regional heterogeneity, which can potentially bias our estimation results.

Although, we find evidence that recipients of relocation assistance move predominately to areas with better overall labor market conditions. This circumstance explains only about 3% of the wage increase. We conclude that major part of the wage rise is a premium, which compensates the job-seeker for the non-pecuniary costs of the relocation, which arise in addition to the pure transportation costs, while job-seekers who not receive this premium do not participate. The size of the premium depends on a job-seeker's gender and the family status. Men receive more than women and individuals with children and life partners earn more than singles. To increase the participation rate the employment agency can increase the subsidy above the pure transportation costs. It is thinkable that the participants receive the transportation costs plus a wage subsidy for the first 6 months

of the new employment, which compensates them for non-pecuniary costs of the relocation.

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Figures and Tables

Figure 1: The Transition Process and Definition of The Treatment Group

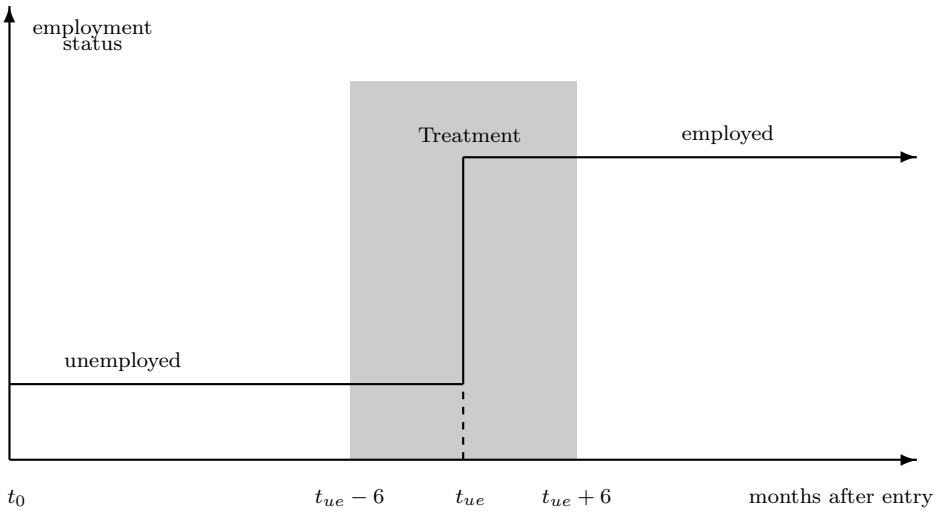
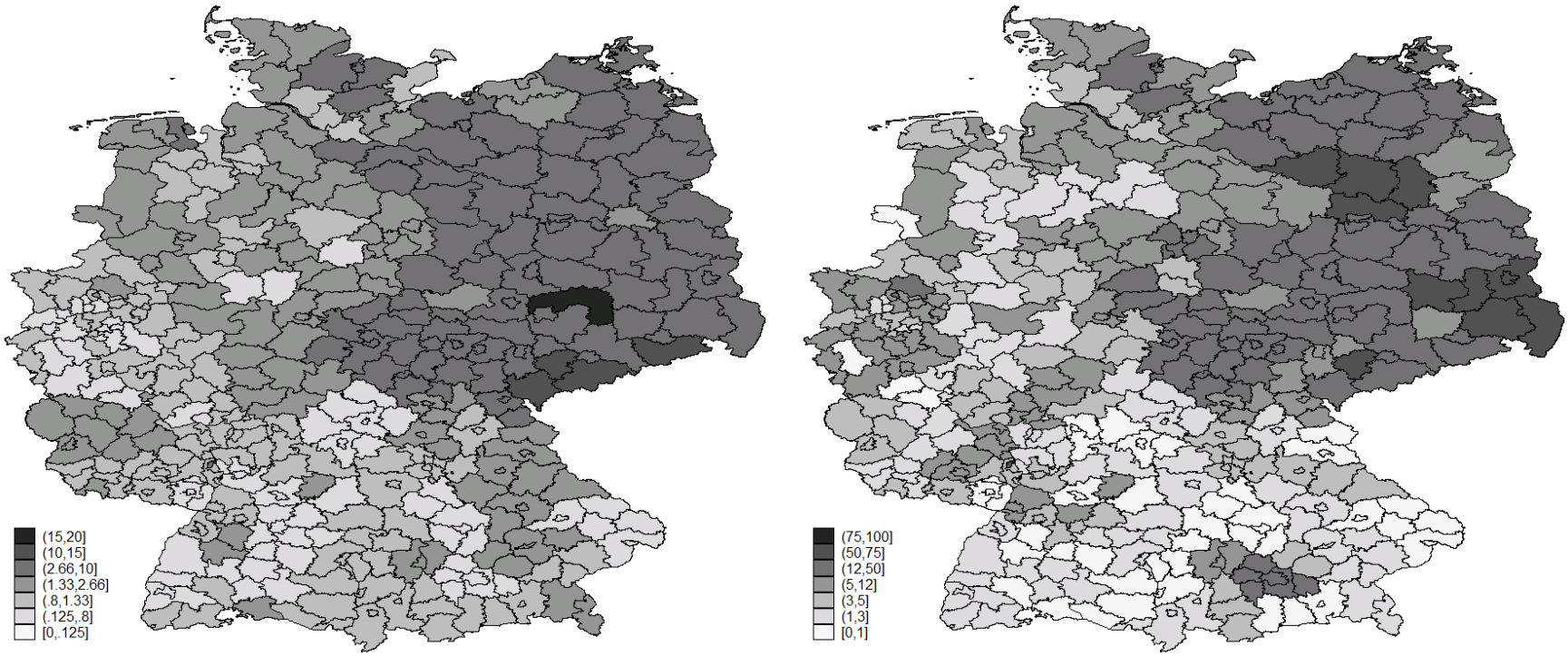


Figure 2: Geographical Distribution of the Treatment Intensity in Germany



(a) Distribution of the treatment intensity among LEA's. Source: statistic of the Federal Employment Agency.

(b) Distribution of participants in commuting and relocation assistance in our estimation sample among LEA's. Source: own calculations.

Table 1: Number of Observations by Treatment Participation

	Non-participants	Participants
Entries into Unemployment	53958	560
Transition to Employment within 24 months	35524	493
Subsidy related to first transition (+/-6 months)	$\star$	453
Change of the working location	$\star$	433

Note: The table shows the size of each group conditioned on several restrictions. $\star$ denotes that the restriction was not conducted for this group.

Table 2: Labor Market Outcomes

	Non-participants	Participants
Obs.	35524	433
Individual labor market performance		
Duration of initial unemployment spell in months	5.55	6.23
First daily income after transition in €	60.92	77.92
No. of job quits within 24 months after transition	1.97	1.26
Employed 48 months after entry	0.73	0.74
Working location after transition		
in the same state as before	0.88	0.26
in a bordering state	0.08	0.27
in a non-bordering state	0.04	0.46
Local macroeconomic conditions		
Local unemployment rate		
before the transition	0.12	0.13
after the transition	0.12	0.11
Local vacancy rate		
before the transition	0.08	0.07
after the transition	0.08	0.10

Note: The table contains only those non-participants who begin a new employment within 24 months after the entry into unemployment. Unless otherwise indicated all values denote shares.

Table 3: Selected Descriptive Statistics at the Entry into Unemployment

	Non-participants	Participants
Obs.	35524	433
Socio-demographic characteristics		
Female	0.33	0.41
Age in years	38.69	36.13
School leaving degree		
None	0.06	0.02
Lower secondary school	0.40	0.16
Middle secondary school	0.36	0.34
(Spec.) Upper secondary school	0.18	0.48
Vocational training		
None	0.16	0.07
Technical college education	0.03	0.03
University degree	0.09	0.32
Children $\leq$ 10 years	0.18	0.13
Married	0.55	0.37
Migration background	0.49	0.55
Short-term labor market history		
Months in employment in year		
t-1	9.94	10.40
t-2	8.50	8.46
t-3	8.10	7.53
Months in program in year		
t-1	0.24	0.24
t-2	0.54	0.48
t-3	0.63	0.68
Last job was full-time employment	0.94	0.96
Last daily income in€	55.16	65.03
Occupational group of previous job		
Agriculture	0.42	0.22
Manufacturing	0.03	0.07
Technical occupation	0.49	0.68
Long-term labor market history		
No. of employers in last 10 years	3.78	3.86
Time in unemployment in last 10 years (in days)	459	358
Any professional experience	0.80	0.68
Benefit conditions		
Remaining benefit entitlement		
less than 3 months	0.31	0.29
4 - 6 months	0.07	0.08
7 - 9 months	0.10	0.10
10 - 12 months	0.44	0.49
more than 12 months	0.08	0.04
Any form of non-compliance with benefit conditions	0.27	0.24
Month of entry into unemployment		
Jan. - Mar.	0.39	0.31
Apr. - Jun.	0.15	0.18
Jul. - Sep.	0.17	0.25
Oct. - Dec.	0.29	0.26

Note: Unless otherwise indicated all values denote shares.

Table 4: Baseline Results - Entries in 2006

	Unemployment duration		First log wage		No. of job quits		Employed in t+48	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Relocation assistance	0.658***	-.129	0.171***	0.252***	-.398***	-.947***	0.017	0.207***
	(0.226)	(0.229)	(0.015)	(0.036)	(0.08)	(0.214)	(0.021)	(0.039)
F-statistic for weak identification	22.7	22.7	23.4	23.4	23.4	23.4	23.4	23.4
Obs.	35957	35957	35957	35957	35957	35957	35957	35957
Mean value non-participants	5.55	5.55	60.92	60.92	1.97	1.97	0.73	0.73
Rho		0.07		-.123		0.143		-.355

Note: */**/*** indicates significance at the 10%/5%/1%-level. The table shows the causal treatment effects on several labor market outcomes for all individuals beginning a new employment within the first 24 months after the entry into unemployment. All estimates report marginal effects and contain control variables for socio-demographic characteristics, short- and long-term labor market history, individual benefit conditions, local macroeconomic conditions and the duration of the initial unemployment spell. Full estimation results are available on request.

Table 5: Fixed Effect Estimation - Entries 2005-2007

	Unemployment duration			First log wage			No. of job quits			Employed in t+48		
	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Relocation assistance	0.619***	-.191	0.079	0.158***	0.238***	0.246***	-.385***	2.906***	-.876***	0.018	0.126**	0.03
	(0.13)	(0.332)	(0.371)	(0.008)	(0.021)	(0.021)	(0.054)	(0.035)	(0.145)	(0.014)	(0.064)	(0.095)
LEA fixed effects			✓			✓			✓			✓
Obs.	113192	113192	113192	113192	113192	113192	80750	80750	80750	80750	80750	80750
F-statistic for weak identification		48.966	16.41		46.908	16.134		46.908	16.134		46.908	16.134
Mean value non-participants	5.656	5.656	5.656	60.593	60.593	60.593	1.918	1.918	1.918	0.733	0.733	0.733
Rho		0.069	0.049		-.106	-.114		-.907	0.118		-.169	-.018

Note: */**/*** indicates significance at the 10%/5%/1%-level. The table shows the causal treatment effects on several labor market outcomes for all individuals beginning a new employment within the first 24 months after the entry into unemployment. All estimates report marginal effects and contain control variables for socio-demographic characteristics, short- and long-term labor market history, individual benefit conditions, local macroeconomic conditions and the duration of the initial unemployment spell. Full estimation results are available on request.

Table 6: Heterogeneity of the Treatment Effect

	All Individuals		Singles	Children	Children and Partner
First log wage	(1)	(2)	(3)	(4)	(5)
Relocation assistance	0.252***	0.23***	0.202***	0.255***	0.308***
	(0.036)	(0.033)	(0.055)	(0.061)	(0.068)
Labor market conditions after transition		✓	✓	✓	✓
Obs.	35957	35957	13670	15051	12554
F-statistic for weak identification	23.4	31.4	13.6	15.5	12.0
Mean value non-participants	60.92	60.92	62.07	59.70	61.16
No. of participants	433	433	244	114	87
	No upper sec. school degree	Upper sec. school degree	University degree	Men	Women
First log wage	(6)	(7)	(8)	(9)	(10)
Relocation assistance	0.272***	0.262***	-.579***	0.231***	0.191***
	(0.045)	(0.099)	(0.036)	(0.039)	(0.059)
Labor market conditions after transition	✓	✓	✓	✓	✓
Obs.	29376	6581	3324	23948	12009
F-statistic for weak identification	27.1	11.4	11.2	24.5	10.3
Mean value non-participants	57.68	75.76	86.02	65.83	51.10
No. of participants	226	207	137	256	177

Note: */**/*** indicates significance at the 10%/5%/1%-level. The table shows the causal treatment effects on the first daily wages for different subgroups using the instrumental variable approach. All estimates report marginal effects and contain control variables for socio-demographic characteristics, short- and long-term labor market history, individual benefit conditions, local macroeconomic conditions and the duration of the initial unemployment spell. In column (2)-(10) we include also control variables for the local labor market conditions for the workplace after the transition to regular employment. Full estimation results are available on request.

A Appendix

A.1 First-Stage Estimation of the Treatment Probability

The relevance condition (ii) states that the treatment probability $P_i(W)$ needs to be a nontrivial function of the local treatment intensity Z_j. To verify this condition Table A.1 shows the estimates of the first stage regression of the treatment intensity on the participation probability for relocation assistance. Column (1) and (2) assume a linear relation between the treatment intensity and the participation probability, while column (3) and (4) assume a non-linear probit model. The linear specification of the first stage is related to the standard two-stage least square estimation, while the probit model refers to our non-linear Maximum Likelihood estimator. Column (2) and (4) include also the duration of the initial unemployment spell as control variable, while column (1) and (3) do not.[22] The coefficients are positive and highly significant irrespective of the specification. To test for weakness of the instrument we provide the standard F-statistic on excluded instruments for the linear specification (see Staiger and Stock, 1997). For both types of treatment the F-statistic exceeds 10, the critical value for weak instruments. As a counterpart for the non-linear model we also provide a Likelihood-Ratio test comparing the model including the instrumental variable and one without the instrument. Again, the instrument passes the test for weak identification. Finally, we compute the hitrate, the share of correctly predicted treatment values. We predict an individual to be treated if the estimated treatment probability is above the mean value of the observed participation rate, and to be not treated if it is below. For the linear specification we observe a hitrate between 59%, while that of the non-linear specification are substantially higher (71%). All these results suggest that the treatment intensity is a quite good predictor of the participation in relocation assistance, and the instrumental variable estimation introduced in Section 4.1 leads to consistent estimates of the treatment effect.

[Insert Table A.2 about here]

[22] Including the unemployment duration is relevant for the post transition outcomes like the first wage, the number of job quits and the employment status 48 months after entry.

A.2 Two-Stage Least Square vs. Maximum Likelihood

The standard approach for instrumental variables ist the two-stage least square (2SLS) estimator. In the first stage, the participation probability is estimated conditioned on the instrument and the covariates. In the second stage, the predicted participation probability, instead of the actual treatment status, is used to estimate the treatment effect. The 2SLS estimator of the treatment effect is given as $\delta_{2SLS} = (Z'X)^{-1}(Z'Y)$. Compared to the Maximum Likelihood estimator of Section 4.1 the 2SLS estimator is based on less restrictive assumptions about the distribution of the error terms. The 2SLS estimator require only the basic IV assumptions (i)-(iii).[23] Following Imbens and Angrist (1994) these assumptions identify only the local average treatment effect (LATE), the causal effect on those individuals induced to change participation status by a change in the instrumental variable. In contrast the Maximum Likelihood estimator identifies, under correctly specified distributional assumptions, the causal treatment effect on all treated individuals (ATT). There are several studies, e.g. Angrist (1991), Angrist (2004) or Chiburis, Das, and Lokshin (2012), showing that the LATE can differ substantially from the ATT especially for small samples and asymmetric first-stage estimations.[24] Especially, the latter is a problem in our estimation sample. Although, the group of non-participants is very large, we observe only 433 individuals receiving the relocation assistance. This results in a participation probability of 1.2%. Table A.2 shows the predicted participation probabilities after a linear and non-linear first-stage estimation. The linear first-stage predicts participation probabilities between -3.4% and 10.7%. Considering this small support the first-stages estimates creates only few variation, to explain the outcome differences. Table A.3 compares the results of 2SLS estimator and baseline results of the Maximum-Likelihood estimator from Section 5.1. We concentrate the analysis on the first wage and the employment status 48 months after entry. For the latter the estimated causal treatment effects are very similar for the 2SLS and the ML estimator, however the 2SLS estimator shows no significance. Estimating the effect on the first wage both estimators differ substantially. While we get a positive treatment effect of relocation assistance about 25% with ML, the effect of 2SLS estimator is about -273%. Therefore, we conclude that the 2SLS estimator is heavily bi-

[23] As mentioned before the Maximum Likelihood estimator requires the error terms of the first stage and the outcome equation to be jointly normal distributed. 2SLS requires only relevance, independence and monotonicity of the instrumental variable.

[24] Angrist (1991) stated that the LATE is close to the ATT when the first-stage changes the treatment probability at values centered on one-half.

ased due to the small size of the treatment group, while the ML estimator is in line with our expectations.

[Insert Table A.3 about here]

A.3 Does Relocation Assistance Increase the Job Finding Rate?

One of the main objectives of the relocation assistance is to bring unemployed individuals into a job, who do not without. Considering a job search model the job finding rate, also called the hazard rate from unemployment, depends mainly on two factors. The net wage distribution among the job offers and the job offer arrival rate. Assuming that a job-seeker receive a variety of job offers, he has to decide for each of this offer whether to accept it or not. If he accepts the offer or not depends on the respective net wage.[25] Paying a subsidy on accepting distant jobs increases the net utility of these offers and increases a job-seekers likeliness to accept distant jobs, which has a positive effect on the overall job finding probability. Furthermore, the job-seekers anticipate that the subsidy increase their net utility of accepting a distant job offer, which changes also the search behavior. They increase their search radius, so they are confronted with a higher number of job offers. This growth of the potential job offers clearly raises ceteris paribus the job finding probability (see Rogers, 1997).

To investigate the effect of the subsidy we estimate the probability to leave unemployment into regular employment for each period of the initial unemployment spell, including, beside the socio-demographic characteristics, the labor market history, the benefit and macroeconomic conditions, also control variables for the duration dependence of the initial unemployment spell. Table A.4 shows the estimates of a univariate and a bivariate probit model, controlling for unobserved heterogeneity. Column (1) and (2) contain the conditioned estimation sample of Section 5.1, while column (3) and (4) include all individuals, regardless of whether they find an employment within the observation period or not. Column (1) and (2) are comparable to the estimates of the unemployment duration in Table 4. From our estimates of the hazard rate λ we predict the corresponding unemployment duration t_{ue} following by a Poisson process:

$$t_{ue} = \int_0^\infty t\lambda e^{-\lambda t} dt = \frac{1}{\lambda}. \tag{9}$$

[25] The job-seeker accepts a job if the corresponding net wage exceeds his individual reservation wage, the wage that equalizes the expected utility of being employment and unemployment.

Obviously, there is a negative relation between the hazard rate and the unemployment duration. An increase of the hazard rate shortens the unemployment duration. Using this strategy, our prediction of the effects on the unemployment duration are quite similar to directly estimated effects in Table 4. Applying this estimation procedure on the unconditioned sample of all individuals, regardless of whether they find a new employment or not, the subsidy increases the job finding probability substantially. Without controlling for unobserved heterogeneity the relocation assistance increases the job finding rate about 3.7%. Using the treatment intensity as instrumental variable the effect increases up to 4.6%. Therefore, we expect the availability of the relocation assistance to shorten the unemployment duration about 6 months, comparing participants to all non-participants.

[Insert Table A.4 about here]

Table A.1: Estimates of the First Stage Participation Probability

Relocation assistance	OLS (1)	OLS (2)	Probit (3)	Probit (4)
Treatment intensity	0.001***	0.002***	0.001***	0.001***
	(0.0003)	(0.0003)	(0.0002)	(0.0002)
Controlling for unemployment duration		✓		✓
Obs.	35957	35957	35957	35957
F-statistic	22.665	23.417		
LR-test			22.285	23.232
			(0.00)	(0.00)
Hitrate	0.595	0.591	0.705	0.706

Note: */**/*** indicates significance at the 10%/5%/1%-level. The table shows the effect of the local treatment intensity on the participation probability. All estimates report marginal effects and contain control variables for socio-demographic characteristics, short- and long-term labor market history, individual benefit conditions and local macroeconomic conditions. Column (2) and (4) also contain control variables for the duration of the initial unemployment spell. Full estimation results are available on request.

Table A.2: Summary of the First-Stage Participation Probabilities

Relocation assistance	Obs.	Mean	Std. Dev.	Min.	Max.
OLS	35957	0.012	0.014	-0.034	0.107
Probit	35957	0.012	0.016	0.000	0.491

Note: The table summarizes the predicted participation probabilities after a linear (OLS) and a non-linear (Probit) first-stage estimation. The estimations contain control variables for socio-demographic characteristics, short- and long-term labor market history, individual benefit conditions, local macroeconomic conditions and the duration of the initial unemployment spell.

Table A.3: Comparison of the ML and the 2SLS estimator

	First log wage			Employed in t+48		
	OLS (1)	ML (2)	2SLS (3)	OLS (4)	ML (5)	2SLS (6)
Relocation assistance	0.171***	0.252***	-2.730***	0.017	0.207***	0.232
	(0.015)	(0.036)	(0.836)	(0.021)	(0.039)	(0.829)
F-statistic for weak identification			23.4			23.4
Obs.	35957	35957	35957	35957	35957	35957
Mean value non-participants	60.92	60.92	60.92	0.73	0.73	0.73

Note: */**/*** indicates significance at the 10%/5%/1%-level. The table shows the causal treatment effects on several labor market outcomes for all individuals beginning a new employment within the first 24 months after the entry into unemployment for the OLS, the Maximum Likelihood and the 2SLS estimator. All estimates report marginal effects and contain control variables for socio-demographic characteristics, short- and long-term labor market history, individual benefit conditions, local macroeconomic conditions and the duration of the initial unemployment spell. Full estimation results are available on request.

Table A.4: Estimates of the Treatment Effects on the Transition Rate

	Transition within 24 months		All individuals	
	Probit	IV	Probit	IV
	(1)	(2)	(3)	(4)
Treatment effect on				
Transition Rate	-.018***	0.012	0.037***	0.046**
	(0.006)	(0.032)	(0.004)	(0.023)
Unemployment duration (predicted)	0.87	-0.50	-5.23	-6.05
Obs.	35957	35957	54398	54398
Average transition rate non-participants	0.152	0.152	0.067	0.067
Average unemployment duration non-participants	6.59	6.59	14.90	14.90

Note: */**/*** indicates significance at the 10%/5%/1%-level. The table shows the causal treatment effects on the transition probability. The estimations in column (3) and (4) contain all individuals, while those in column (1) and (2) only individuals beginning a new employment within the first 24 months after the entry into unemployment. All estimates report marginal effects and contain control variables for socio-demographic characteristics, short- and long-term labor market history, individual benefit conditions, local macroeconomic conditions and duration dependence. Full estimation results are available on request.

B Supplementary Appendix

Table B.5: Labor Market Outcomes - Entries 2005 - 2007

	Non-participants	Participants
Obs.	111797	1395
Individual labor market performance		
Duration of initial unemployment spell	5.64	6.13
First daily income after transition in €	60.65	79.72
No. of job quits within 24 months after transition[1]	1.92	1.31
Employed 48 months after entry[1]	0.73	0.75
Working location after transition		
in the same state as before	0.88	0.24
in a bordering state	0.08	0.32
in a non-bordering state	0.04	0.45
Local macroeconomic conditions		
Local unemployment rate		
before the transition	0.11	0.13
after the transition	0.11	0.10
Local vacancy rate		
before the transition	0.08	0.07
after the transition	0.08	0.10

Note: The table contains only those non-participants who begin a new employment within 24 months after the entry into unemployment. Unless otherwise indicated all values denote shares. [1]The number of job quits after the transition and the employment status 48 months after the entry is only observed for individuals entering unemployment in 2005 and 2006 (Non-participants: n=79818; Participants: n=932).

Table B.6: Treatment Intensity 2004 - 2007

Year	Mean	Median	Std. Dev.	Min.	Max.
2004	0.060	0.031	0.072	0.005	0.442
2005	0.025	0.015	0.028	0.001	0.190
2006	0.029	0.019	0.027	0.004	0.158

Note: The table shows the local treatment intensity for the 180 LEA districts from 2004 to 2007.

Table B.7: Selected Descriptive Statistics - Entries 2005 - 2007

	Non-participants	Participants
Obs.	111797	1395
Socio-demographic characteristics		
Female	0.35	0.36
Age in years	38.64	36.48
School leaving degree		
None	0.07	0.02
Lower secondary school	0.39	0.16
Middle secondary school	0.36	0.37
(Spec.) Upper secondary school	0.18	0.45
Vocational training		
None	0.18	0.07
Technical college education	0.03	0.03
University degree	0.09	0.32
Children $\leq$ 10 years	0.18	0.13
Married	0.55	0.39
Migration background	0.45	0.51
Short-term labor market history		
Months in employment in year		
t-1	10.06	10.48
t-2	8.45	8.62
t-3	7.99	7.71
Months in program in year		
t-1	0.26	0.26
t-2	0.56	0.57
t-3	0.63	0.65
Last job was full-time employment	0.94	0.96
Last daily income in €	55.39	66.31
Occupational group of previous job		
Agriculture	0.41	0.25
Manufacturing	0.03	0.07
Technical occupation	0.50	0.65
Long-term labor market history		
No. of employers in last 10 years	3.79	3.89
Time in unemployment in last 10 years (in days)	457	330
Any professional experience	0.79	0.70
Benefit conditions		
Remaining benefit entitlement		
less than 3 months	0.33	0.26
4 - 6 months	0.07	0.08
7 - 9 months	0.09	0.09
10 - 12 months	0.42	0.50
more than 12 months	0.09	0.07
Any form of non-compliance with benefit conditions	0.25	0.23
Amount of daily unemployment benefits (in €)	17.62	20.87
Month of entry into unemployment		
Jan. - Mar.	0.22	0.19
Apr. - Jun.	0.11	0.14
Jul. - Sep.	0.13	0.16
Oct. - Dec.	0.54	0.51

Note: Unless otherwise indicated all values denote shares.