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Randomizing information on a targeted wage support program for older workers: A field experiment*

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JEL: J64, J65, J68, J33

Abstract: To address the problem of high reservation wages among older unemployed individuals, a German targeted wage support program aimed at providing incentives to accept lower paid wage offers. We sent out information brochures on this program to randomly selected eligible men. The treatment significantly increased awareness of the program by 20 percentage points. Combining survey and administrative data, we conduct reduced form estimates of the effects of brochure receipt on recipients and estimate local average treatment effects of additional program knowledge. The information treatment significantly increased take-up rates of the program. For unemployed men aged 50–54, we find no positive effects on employment outcomes, thus the additional take-up seems to have been pure windfall. For unemployed men aged 55–59, however, we find some positive effects of additional information on labor market results. The labor market status of unemployed men above age 60 is not affected by brochure receipt.

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1 Introduction

Employment prospects of older workers are bleak in many advanced countries (OECD 2006): People above age 50 are often the first to be fired and the last to be hired. There are some obvious reasons why the reintegration of older unemployed persons is particularly difficult: If older workers are displaced their job-specific human capital becomes obsolete; and they lose existent claims on seniority wages. Thus they face difficulties in finding a job with a similar wage as before. This process is reinforced if unemployment benefits are relatively generous for older workers as it is the case in Germany (Dlugosz et al. 2014). Early retirement regulations might provide an attractive alternative for the older among the older. On the demand side, there are often general concerns of firms regarding the productivity of older workers (Posthuma and Campion, 2009). Furthermore, if firms rely on seniority wages (Becker 1962) or efficiency wages as an incentive device (Lazear 1981), potential years of remaining tenure might seem too short for hiring older workers.

To what extent can active labor market policies help? This paper investigates a rather unique targeted wage support program for older workers that had been in place in Germany during the period 2003-2011, the so-called Entgeltsicherung (EGS). EGS was part of the now famous Hartz reforms and consisted of a wage subsidy paid to workers of age 50 or older: If the wage in a new job was smaller than the pre-unemployment wage, the program temporarily compensated for part of the wage loss. EGS was unique in the sense that it aimed at decreasing reservation wages of older workers who had lost a rather well-paid job. Furthermore, EGS differed from most other in-work benefits as the amount to be reimbursed did only depend on the pre-unemployment wage. That is why the program provided only little incentives for previous low-wage earners. While older individuals were legally entitled to receive this subsidy if they applied for it, take-up rates had been rather low, which might had been due to a lack of information on the program (Brussig et al. 2006).

We propose an innovative approach to evaluate this wage support program: We conducted a randomized controlled field trial in which the amount of information that individuals got about EGS had been manipulated. We sent out an information brochure to a randomly selected treatment group among the eligible, informing this group about access possibilities to a support program. The other eligible in the population, who did not receive extra information, serve as the control group. The empirical analysis combines

survey and register data. The survey data enable us to infer the reasons behind the effectiveness or ineffectiveness of the program. Moreover, they allow us to observe outcomes that are not recorded in the registers.

In a first stage, we examine the effects of the information treatment on the awareness of the program as well as on search and reservation wages using survey data. In particular, results indicate that the information treatment raised the share of workers that know the program by about 20 percentage points. In a second stage we conduct reduced form estimates of the information provision on several labor market outcomes of interest using register data. While the information treatment increased the share of program participants and increased days in employment and earnings for unemployed of age 55–59, it actually worsened the labor market performance of workers aged 50–54. For the latter group, we also conclude that the additional take-up of the program was absorbed by deadweight loss. In a third stage, we use the information provision as an instrumental variable to assess local average treatment effects (LATE) of knowing about the program on the outcomes of interest (or local average awareness efforts) combining survey and register data and applying two-sample-estimators. While estimated coefficients retain their sign and increase in size, effects become insignificant. This is probably due to the fact that the LATE estimates refer to the group of compliers only, which is comparatively small of size.

2 The wage support program

Our project focuses on EGS, a wage support program for older employees, which was introduced as part of the German Hartz reforms in 2003. Until the end of the year 2011, unemployed persons of age 50 and older were entitled to targeted wage support if they accepted a monthly net wage at least 50 Euro below their pre-unemployment wage and met further conditions: (i) Workers had to have a residual claim on not less than 120 days of unemployment benefits. (ii) They had to apply for the support program before taking up the new job. (iii) The new subsidized job was not to be part of a job creation scheme nor of an organization unit at the old employer, which was set-up with the goal to avoid unemployment. (iv) Workers could not at the same time receive EGS and old age pensions. (v) If the firm was covered by a collective contract, wages had to be paid according to this contract; otherwise wages had to reach at least the regional customary

level.

During the first year of employment the support covered half of the net wage difference between actual and previous earnings (up to the social security threshold in the unemployment insurance). For example, if a single earner had a gross wage of 4,000 Euro before getting unemployed and took up a new job with a gross wage of 3,500 Euro, the corresponding net wage rates were 2,313 and 2,090 Euro, and EGS amounted to 111 Euro per month during the first year in the new job.¹ In the second year of employment the support decreased to 30 percent of the net wage difference. While the EGS was a net wage support that was not subject to income taxation, it was nevertheless taken into account when the individual tax rate (which is progressive in Germany) was determined. Furthermore, the additional income for the worker generated pension and future unemployment benefit claims.

Importantly, taking up a lower paid job did not affect the amount of unemployment benefits a worker was entitled to during a follow-up period of two years. Furthermore, all EGS payments were hours-adjusted: If for instance working hours were 40 weekly hours in the last job, but 30 weekly hours in the new job, the wage difference was computed using $3/4$ of the previous earnings. Changes in wages, working hours, employment or household composition induced a recalculation if the net wage change exceeded 100 Euro.

The main goal of EGS was to provide work incentives for unemployed individuals who received a rather comfortable wage income in their last job. For previously low-wage earners, EGS was mostly not attractive, as – due to lower bounds on wages through collective bargaining, sector-specific minimum wage regulations, and fairness norms in wage setting – any new wage offer could possibly not (or at least not very much) undercut the wage rate in their latest employment relationship. An instrument targeted at the group of previously low-wage workers are hiring subsidies to the employer, which strive to counterbalance an initially low productivity of previously unemployed workers at their new workplace.

Even though EGS seems to be a sensible program to bring older unemployed back to work, the take-up of EGS turned out to be low: Until the year 2006, less than 10.000 individuals participated in the program (Statistics of the Federal Employment Agency).

¹Social security contributions account for 21 percent of the wage rate; individual income taxes are applied.

While numbers grew to almost 20,000 participants during the year 2011, EGS remained small compared to other programs. A few aspects of the program have been evaluated within the context of the so-called Hartz evaluation in 2005. The wage support had predominantly been made use of by high-income earners who experienced wage losses due to taking up a job in a smaller firm or a different industry. Using the introduction of EGS as a natural experiment, a difference-in-difference analysis did not find any significant effects on the reemployment prospects of the eligible unemployed which is not surprising regarding the low inflows into the program (ZEW et al. 2006). Qualitative research suggests that the low take-up rates were due to a lack of information on the wage support program (Brussig et al. 2006).

Another potential reason behind low program entries is that the probability of older unemployed being re-integrated into the labor market is relatively low anyway. For instance, during April 2015, the monthly exit rate out of unemployment in Germany amounted to 9 percent for those of age 25–54, but only to 4 percent for those of age 55 and older (Statistik der Bundesagentur für Arbeit 2015, own computations). Of course, there are also other candidates to explain low take-up rates of EGS: Workers might not meet the conditions outlined above or workers might have perceived the program as not attractive. These points will be elaborated below.

3 Review on similar policies

EGS differs from similar policies granting wage support to workers to some extent: Different to most other wage support programs (including negative income taxes), the amount of EGS does depend on pre-unemployment wages, it does not depend on other sources of income, and it is paid temporarily. Different to partial unemployment benefits it is not paid during a period of unemployment. Different to reemployment bonuses, payments depend on the new wage rate and consist of monthly payments over a period of two years.

Well-known examples for in-work benefit programs are the Earned Income Tax Credit (EITC) in the US and the Working Tax Credit (WTC) in Great Britain. These programs provide a tax credit depending on yearly gross income and further circumstances, like having children or being disabled. Contrary to EGS, they are not focused on persons who experienced a job loss. Research on these programs exploited program modifications

using difference-in-differences approach or simulated labor supply responses to potential reforms (see for instance Meyer/Rosenbaum 2000, 2001, Blundell 2000, Francesconi/Vander Klaauw 2004). For single mothers, studies partly find positive participation effects, but adverse effects on working hours. For married couples, the labor supply of the secondary wage earner was affected. Canada implemented a field experiment on in-work benefits within the context of the Self-Sufficiency Project (SSP) (see for instance Brouillette/Lacroix 2008, Card/Hyslop 2009). Needy lone parents were randomly chosen to be informed about the possibility to receive generous wage support, provided that they had been unemployed for at least one year and would find a full-time job within one year after entering the group of eligible. Evaluation results show a great impact of the program on job-finding rates of the treatment group. However, the incentive of staying on the benefited employment decreased at the end of the funding period of three years.

Other in-work benefit programs are related to work during unemployment and are paid in the form of partial benefits. For instance, in Switzerland unemployed persons taking up a job where they earn less than their unemployment benefits can receive in-between wage support ("Zwischenverdienst"): They obtain unemployment benefits for the remaining gap between their old and their new gap. Gerfin/Lechner (2002) used propensity score matching and non-experimental data to evaluate this program. They find for Switzerland that it is one of the most effective programs in reintegrating unemployed persons into the labor market. Kyrrä (2010) investigated a similar program for Finland, where unemployed workers looking for a full-time job but take up part-time or short-term work might qualify for partial unemployment benefits. The results of a timing-of-events model indicate that this might in fact be helpful in smoothing the way into full-time work.

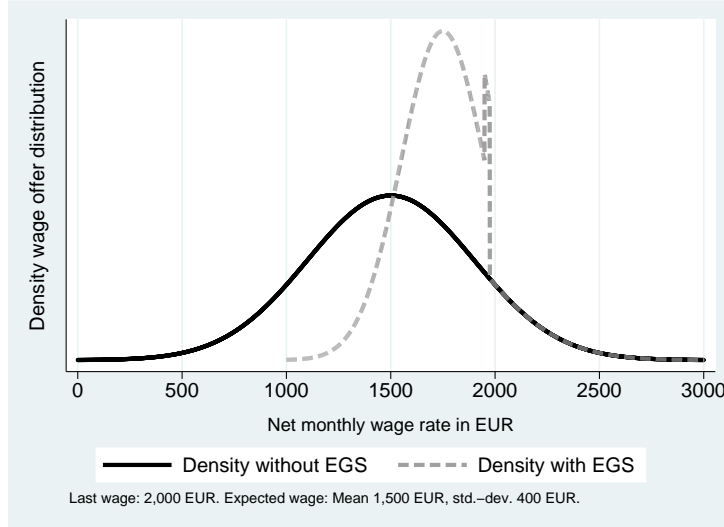
While in-work benefits are paid out regularly during a particular time interval, reemployment bonuses are mostly implemented as lump sum benefits, but serve a similar function. In the 1980s the US experimented with such bonuses that were paid to (displaced) unemployed workers, who found a new job within a short period of time and remained employed for a minimum period. There were various types of bonuses, differing in design and requirements (see Meyer 1995 for more details). Only one bonus variant, however, was found to be cost-effective.

4 Theory

As has been outlined in the introduction, there are several reasons why the reservation wages of older unemployed may be high compared to the market value of their human capital. This is the channel EGS was supposed to tackle: If the wage in a new job was smaller than the pre-unemployment wage, the EGS-program temporarily compensated the wage loss partially. The subsidy amounted to $0.5(w_{pre} - w)$ during the first year and to $0.3(w_{pre} - w)$ during the second year in the new job, with w_{pre} as the net wage in the pre-unemployment job and w as the net wage in the new job. Wages are subsidized up to an upper threshold \bar{w} .

From a theoretical point of view, a natural approach to investigate the effects of wage support programs is the job search theory. Let λ be the job offer arrival rate, while $F(w)$ is the distribution of wage offers. The optimal strategy of an unemployed person is to search until a wage offer arrives that matches at least his or her reservation rate. Assume that the reservation rate in a world with EGS is given by ϕ . The transition rate into work is then given by $\theta = \lambda(1 - F(\phi))$.

Figure 1: Effects of EGS on the wage offer distribution



As there is an upper threshold to subsidized wages, the main effect of the program of is to push the left-hand tail of the wage offer distribution to the right. Remember that the minimum net wage difference to receive the wage support amounted to 50 Euro. Thus, as long as $w < w_{pre} - 50$, the perceived wage offer w_p is $w_p = 0.5(w + w_{pre})$. Thus EGS affects the perceived wage offer distribution at values below the pre-unemployment wage. For a numerical example, the effect is displayed in Figure 1 .

Generally, $F_{EGS}(w)$, the perceived wage distribution in a situation with EGS, dominates $F(w)$ without EGS. If the reservation wage ϕ exceeds $w_{pre} - 25$, EGS does not have any effect on job search behavior. Otherwise, EGS will make at least some wage offers more attractive. So workers can become more selective, with $\phi_{EGS} > \phi$, and the distribution of accepted wages $w_p \mid w_p > \phi$ dominates the distribution in absence of EGS.

Two opposing effects on the transition rate into work θ occur: On the one hand, workers receive higher wage offers, on the other hand, reservations wages ϕ increase. For a location shift of the wage offer curve, Burdett (1981) showed a positive effect on the transition rate into work. The EGS, however, does not lead to a location shift.

Nonetheless, some results can be derived if we consider a general type of EGS where a fraction $\gamma \geq 0$ of w_{pre} is compensated, so that $w_p = \gamma w_{pre} + (1 - \gamma)w$ if $w < w_{pre}$. The shape of $F_{EGS}(w)$ is then determined by the parameter γ . In a stationary setting, the net effect of γ and thus EGS on the transition rate from unemployment to work θ is always positive. Thus, EGS reduces unemployment duration.

Let us now consider the take-up rate η of EGS. It is defined as $\eta = \lambda \max\{(F(w_{pre}) - F_{EGS}(\phi_{EGS})), 0\}$. An important implication is that EGS induces windfall gains for workers (or deadweight losses for society). These are given by the hazard rate of taking-up jobs with EGS that would also have been acceptable in a world without EGS, $\theta + \eta - \theta_{EGS}$.

Of course, effects of EGS may vary by heterogeneous subgroups of workers. First, EGS should have no impact on the behavior of unemployed persons who were previously working in a low-wage job: There might simply be no jobs with even lower wages for which EGS steps in, or such jobs could pay less than the amount of basic care they might be entitled to. Second, effects might be smaller for older workers as they have the additional option of early retirement. Third, EGS might in principle become more attractive shortly before the threshold of a residual claim of 120 days of benefit receipt was reached.

Finally, the effects of EGS might be short-lived as in practice the wage support was paid for no more than two years. If workers build up firm specific human capital, the withdrawal of wage support might be (at least partly) offset by wage increases. If this is not the case, workers might consider to voluntarily quit their new job after two years, probably even earlier (as the size of unemployment benefits was related to wages in the job before EGS take-up during two subsequent years). However, voluntary quits usually result in a cut-off periods of benefits. Furthermore, workers might realize that on-the-job search

would be more beneficial for their labor market prospects than search during additional periods of unemployment.

5 The information treatment

Many targeted individuals fail to take up benefits that they are eligible for (Currie 2006). A lack of information, stigma effects of program participation, transaction costs, and complexity of the application procedure are candidates to explain incomplete take-up. In fact, a growing body of evidence suggests that individuals are often not fully informed about transfer policies relevant for economic choices (e.g., Bhargava and Manoli 2015, Chetty and Saez 2013, Liebman and Luttmer 2015, Osborne Daponte et al. 1999). The provision of information about available support programs can thus enable individuals to draw on additional resources when making their economic choices, altering these choices. To thoroughly investigate this topic, a number of researchers have used the amount of information on the program as a treatment. Typically, such information treatments have been applied within randomized trials. The treatment is then the receipt of an information brochure or letter, an information event, or a personal or telephone consultation. The main purpose of such studies is to explore whether and what kind of information has an impact on human decisions. Information can be used to balance information deficits, to simplify procedures or processes, or to push information to the attention of the beholder, for instance by overcoming procurement costs.

The empirical evidence indicates that information often has significant effects on decisions. This includes participation in various (North American) aid or social security programs. Osborne Daponte et al. (1999) examine whether a lack of information can explain the low participation rates of needy households in the US-American Food Stamp program. They find that information on the eligibility to participate in the program increases the take-up of the program. Bhargava and Manoli (2015) informed eligible individuals about the EITC. Results indicate that information about the EITC in-work benefit eliminate information deficits and increase the take-up rate. Aizer (2003; 2007) finds similar results for the American Medicaid program, highlighting the role of imperfect information for the decision on participation. In another experiment, Duflo and Saez (2003) sent out letters informing about monetary rewards for attending an information fair about retirement

plans. Compared to the control group, the attendance at the fair was more than five times higher for those treated. After the fair, enrollment into the Tax Deferred Account (TDA) was higher for the treatment group. Treatment also increased enrollment of other unemployed from the same departments, a finding that points to peer group effects. In another study, Liebman and Luttmer (2015) show that an information treatment on key Social Security provision increased the share of older working US Americans by four percentage points one year after treatment. Contradicting this result, Chetty and Saez (2013) do not find any effect of additional information on EITC on average labor supply and earnings for EITC recipients. For Belgium, Huysentryt and Lefevere (2010) study the effects of information (and information presenting formats) on peoples payment method for family benefits. They find that information procurement costs affect individual decisions, and that the costs are much smaller if information is easy to access and little complex. Altmann et al. (2015) sent out information brochures to increase the job search activities of randomly selected job seekers in Germany. Informing about job search strategies and consequences of unemployment has mainly no effect on the job seekers employment prospects and labor market outcomes. However, they find positive effects on employment and earnings for unemployed job seekers who are supposed to have a high risk of becoming long-term unemployed.

Although information treatments – characterized also as encouragement designs by Duflo et al. (2007) – constitute cheap and low-threshold interventions (that should not evoke moral concerns), they have not yet been used in the evaluation of European active labor market programs. We will build on the international literature and use the potential of information treatments within the context of a randomized field trial, the gold standard of evaluation methods. Our treatment consisted of sending out an information brochure about EGS to the home address of 2,600 nationwide randomly selected eligible older men around October 1, 2011. The treatment group is comparatively small as we did not want to cause major disturbances in the work of local labor market agencies and to avoid general equilibrium effects. The control group consists of eligible unemployed men who did not receive our brochure.

We selected the target population from process generated data of the German Federal Employment Agency (FEA), focusing on individuals who would be entitled for EGS if they received a suitable wage offer. We first selected men who were registered as unemployed

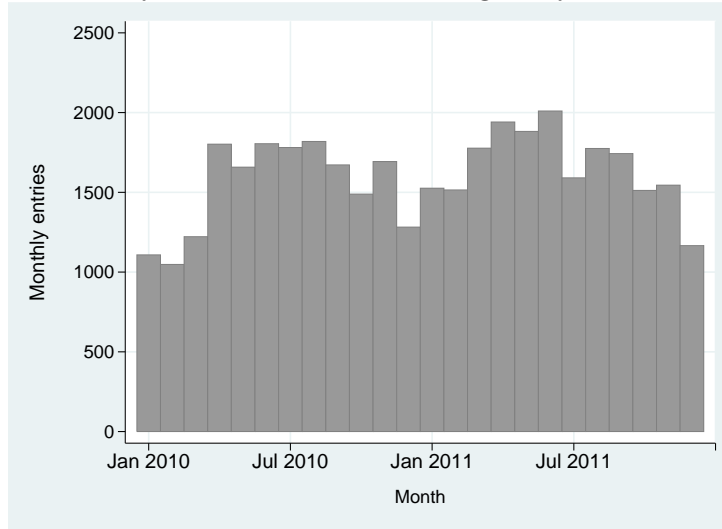
during June 2011. Second, we merged information from the Integrated Employment Biographies (IEB) to further select persons who would have been entitled to EGS. These data are drawn from several linked administrative sources and contain daily spell information about employment periods subject to social security contributions, job seeking periods, participation in active labor market programs, and unemployment benefit and unemployment assistance claim periods. The version V09.00.03-110325 of IEB we used for this step contained employment information until the end of 2009.

To identify persons with stable employment biographies who should be entitled to the full length of unemployment benefit duration, we restricted the sample to persons, who had been in employed due to social security contributions for a period of at least five years (2005–2009). As EGS was targeted especially at previously well-paid persons, we selected only older unemployed, whose previous gross earnings were at least one third of the social security threshold. Thus individuals in the sample earned at least 53 (61) Euro per day in their last job, depending on having worked in East (West) Germany beforehand. The remaining population from which our 2,600 brochure recipients were chosen consisted of around 23,000 workers; thus our potential control group is composed of around 20,000 individuals.

The selected sample of brochure recipients has been stratified by three age groups (50-54, 55-59, 60-64) and five labor market types (based on unemployment rate, seasonal unemployment etc., see Dauth et al. 2008). Furthermore, half of the sample consisted of potentially displaced workers. We define a potential displacement as a situation, where at least 10 percent of the 2009 workforce was registered as unemployed during the year 2011.

Note that we can observe potential inflows in EGS only during the last quarter of the year 2011 (as the program was abolished afterwards). Thus the question of anticipation arises - did inflow rates into the program peak during this period because individuals wanted to take the last chance to participate in the support program? Aggregate figures on program entries, however, provide no evidence for such anticipation effects: Figure 2 shows how monthly program entries involved during the years 2010 and 2011. Furthermore, the abolishment of the program was ignored by the media as it was only a small element of a larger restructuring of active labor market policies in Germany that took place during the years 2011 and 2012.

Figure 2: Monthly entries into EGS during the years 2010 and 2011



Source: Statistics of the Federal Employment Agency (DWH), own calculations

6 Causal effects

The main problem arising in policy evaluation is selection: program participants are hardly ever a random selection of the population. This complicates the estimation of treatment effects, which requires an adequate comparison group. Estimators that fail to adjust for non-ignorable selection are biased and inconsistent. Numerous studies utilize natural experiments to analyze the effect of changes in legislation or eligibility thresholds on the group of eligible individuals, applying difference-in-differences or regression discontinuity approaches (Imbens and Angrist 1994). Non-experimental evaluations partly apply duration models (Abbring and van den Berg 2003) while many others rely on statistical matching methods to choose an adequate comparison group (Rubin 1974). The latter approach relies critically on the conditional independence assumption. This non-testable assumption requires that all variables that determine both selection into the program and the outcome are observed in the case of non-participation.

Our project solves the selection problem by implementing a randomized trial. The sequence considered in the EGS experiment is as follows: (i) Potential participants do (not) get an information brochure on the program. (ii) Potential participants do (not) gain knowledge about the program. (iii) Potential participants decide on job search behavior. (iv) Labor market outcomes of potential participants are realized. The experiment provides random variation in (i); this can be used to estimate average effects of receiving information on (ii). Moreover, (i) can be used as an instrument for effects of (ii) on

(iii) and (iv). Basically, we can thus estimate local average treatment effects (LATE) of awareness of the program (thus local average awareness effects) on several labor market related outcome variables. Estimated effects refer to eligible individuals, who would not have been informed about the program if the planned intervention would not have taken place.

Let us for now ignore stratification of our sample; we will return to this issue when we outline our empirical strategy. In econometric terms, the receipt of a brochure Z is a binary instrument, while the treatment of interest (or potential exposure) D is having knowledge about the support program. Several moments in time after Z und D have been realized, outcome variables of interest Y can be examined. The core conditions for local estimations are (Imbens and Angrist 1994, Clarke and Windmeijer 2012): (i) independence of potential outcomes and the information treatment: $D(z), Y(z, d) \mid Z$, (ii) an exclusion restriction: $Y(z, d) = Y(d)$, (iii) a causal effect of the instrumental variable (IV) on exposure: $E(D(z))$ is a nontrivial function of z .

Within this framework actual program participation cannot be interpreted as a treatment as the receipt of information potentially affects the behavior also of those who decide not to participate in the program. Informed individuals might in general become more motivated to search for a job and search harder, which might again have an impact on their labor market outcomes. This implies a violation of the exclusion restriction, which is a general dynamic instrumental variable (IV) evaluation problem (van den Berg 2007).

In a simple case without covariates, realizations can be distinguished into four groups: (i) compliers: $D(0) = 0$ and $D(1) = 1$, (ii) always-takers: $D(0) = 1$ and $D(1) = 1$. (iii) never-takers: $D(0) = 0$ and $D(1) = 0$, (iv) defiers: $D(0) = 1$ and $D(1) = 0$. Under monotone selection, $D(z)$ is a non-decreasing function of z , and the set of defiers is zero with probability one.

If monotone selection holds, a Wald estimator could be used to estimate LATE using a two sample instrumental variable (2SIV) estimator, computing $cov(Y; Z)/cov(D; Z)$, where $cov(Y; Z)$ comes from register data and $cov(D; Z)$ from survey data. This LATE is also known as the complier average causal effect. In our case, we can safely assume that the monotonicity condition holds as it is very unlikely that persons who received the information treatment are less aware of the program than persons in the control group. However, while we used an experimental design, we still have to take into account

stratification variables as covariates.

Van den Berg et al. (2015) suggest a version of the Wald estimator that controls for covariates, which is the sample equivalent of

$$LATE = \frac{\int E[Y|X = x, Z = 1] - E[Y|X = x, Z = 0]f(x)d(x)}{\int E[D|X = x, Z = 1] - E[D|X = x, Z = 0]f(x)d(x)} \quad (1)$$

This allows estimating LATE without having to assume a functional form while nonetheless controlling for covariates. In our analysis, the nominator is computed based on register data, while the denominator is computed based on survey data; integration occurs about the marginal distribution of covariates in the register data. As van den Berg et al. (2015) write, the estimator starts with the reduced-form effect of the instrument Z on the outcome Y , and then corrects for imperfect compliance in the sense that $Z=1$ does not always imply $D=1$. In our case imperfect compliance means that not every informed person is aware of EGS.

As a parametric alternative, the two-stage least squares (2SLS) is often used to estimate LATE for continuous outcome variables. Under the restrictions outlined by Imbens and Angrist (1994) the 2SLS estimator is consistent for LATE even with discrete endogenous variables (Chesher and Rosen 2013). We apply a two-sample version (2S2SLS), as as been suggested by Inoue/Solon (2010); see also van den Berg et al. (2015).

7 Data and empirical strategy

Our analysis draws on individual survey data as well as on individual register data at the deepest and most detailed level. While process generated data provide lots of reliable information already, they are also missing some important information. To obtain more insights in the search process, we conducted additional computer assisted telephone interviews (CATI) during August 2012. In particular, we asked around 950 randomly selected persons from the treatment and from the control group each about their knowledge of EGS, their opinion of EGS, and their search behavior. Interviewed control group members were stratified according to the stratification of the treatment group described above. To analyze labor market outcomes of the treatment over time, we merged data from the version V11.00.00-131009 of the IEB (from which an earlier version was used to draw the sample), which covers the period until the end of 2012. This was possible for around 1,500

surveyed persons who gave their consent to merging. All following analyses based on the survey data are restricted to this group.

Empirically, we investigate in a first step, whether the information treatment did indeed change the share of informed individuals in the treatment group. To obtain this information, we need the survey data. In a second step, we conduct reduced form OLS-estimates of the effect of the treatment on our outcome variables of interest, which are available from the register data.² The parameters we estimate in the first and second step are average age-specific effects of the brochure receipt on brochure recipients. In a third step, we use the receipt of an information brochure as an instrument for knowledge of the program to estimate LATE of program knowledge (survey data) on the outcome variables (register data), using the Wald- and the 2S2SLS-estimator outlined above. Standard errors are bootstrapped using 1,000 iterations. The parameters we estimate in this third step are average age-specific effects of program knowledge (instrumented through brochure receipt) within the group of compliers.

Due to random selection of the treatment group, we do not have to be concerned on differences in unobservable characteristics between the treatment and the control group within our defined strata (age, potential displacement, and region). However, as we randomized on observables, we have to take into account stratification. If the treatment and the control group are stratified in the same fashion, a simple, while adequate strategy for estimating treatment effects is to control for stratification variables in estimated regression models (Duflo et al. 2007, Section 6.1). Accordingly, we also have to account for stratification when estimating LATE of program awareness on labor market outcomes.

In addition to awareness of EGS, the survey data provide us with information on behavioral impacts of EGS, in particular search intensity and willingness to take-up less paid jobs. We also asked about the reservation wage, specified as the minimum monthly net wage rate a worker had to be offered to take up a job with 40 weekly working hours.

²While the means of several outcome variables are low (in particular, the mean take-up rate of EGS in our sample is 1 percent), this does not automatically imply that we deal with a rare event in the statistical sense. As the number of events observed even for EGS take-up is around 300, and moreover, predictions do not fall outside the $[0,1]$ interval, we are confident that OLS is an adequate tool for our analysis.

The register data allow us to investigate a broad range of further outcome variables:

- Program participation and short-term employment outcomes are measured by dummy variables indicating take-up of the EGS and take-up of a job until the end of 2011 (up to 3 months since random assignment).
- Longer run employment outcomes are computed as a dummy variable indicating the employment status until the end of 2012 (up to 15 months since random assignment) and as accumulated days of employment until the end of 2012.
- Accumulated earnings (computed from times in employment and daily wages) are measured until the end of the year 2012. Note that wage information is subject to the social security threshold, which amounted to 230 (187) Euro per day in west (east) Germany during the year 2012.
- Retirement is inferred from the reason given for leaving unemployment (if unemployment ended before the end of 2012).

A very interesting aspect of our work is that we can determine to what extent non-subsidized employment has been replaced by subsidized employment (EGS) as a result of additional information available: If we find higher take-up rates of EGS in the treatment group while employment rates are not higher than in the control group, this will indicate that the scheme induced windfall gains only.

Table 1 provides some basic descriptive statistics on the workers in the treatment and in the control group, for the register as well as for the survey data. Our treatment group consists to a quarter each of older workers in the age groups 50–54 and 55–59. Half of the group is composed of workers age 60 and older. Compared to the control group from the register data, the comparatively younger workers among the elderly were oversampled in the treatment group. Furthermore workers that were potentially displaced are oversampled in the treatment group. This clearly shows that these stratifications have to be taken into account in the empirical estimates. Further information confirms that our sampling strategy focused on workers with rather stable employment biographies: The share of workers without a vocational degree is 14 percent in the register data and even lower in the survey data. Related to the sampling strategy that focused on workers with stable employment biographies, only around 20 percent of workers were unemployed for

more than a year at the treatment date; and most of them were employed for more than four of the five years preceding the unemployment spell.

8 Average information effects on awareness, search and reservation wages

The main goal of the first step is to investigate the effects of the information treatment – receiving an information brochure on EGS – on the awareness of EGS. This step of the analysis draws entirely on the survey data. Table 2 presents means of a range of survey data variables, for the entire sample as well as by age group (our most important stratification variable). Table 3 provides OLS-regressions of outcome variables on interactions of the information treatment indicator with age groups, while controlling for stratification. Note that the results do not change substantially if we additionally control for interactions of the treatment dummy with further stratification variables or include interactions of all stratification variables.

On average, three quarter of all individuals participating in the survey are aware of EGS. Generally, the share of informed persons is highest in the age group 55–59. Both tables show that brochure receipt increased the share of individuals informed about EGS by around 20 percentage points, and this difference is highly significant. Thus the information treatment was successful in the sense that the mere receipt of an information brochure on EGS did indeed increase awareness of the program substantially.

Furthermore, the survey data enable us to look for behavioral impacts of EGS and to further investigate one potential reason for low take-up rates of EGS. One third of survey participants said that the possibility to receive EGS induced them to search more actively, and 45 percent answered that EGS increased their willingness to accept less paid jobs. On average, brochure receipt seemed to have had partly negative effects on the search activity (for up to 6 percent of recipients), but positive effects on the willingness to accept a less paid job (for up to 11 percent of recipients). Effects are, however, significant only for the oldest age group investigated.

We also asked for the reservation wage. While differences between the treatment and the control groups are not significant, they convey the impression that the information treatment on average increased reservation wages of men aged 50–54 (by nearly

200 Euro/months), while it decreased reservation wages of older persons (by around 100 Euro/months).

Do workers perceive EGS as an unattractive program? As can be seen from Table 2 this is not the case. More than 70 percent of the workers think that EGS is a suited program to bring older unemployed individuals back into jobs. Only around 20 percent answered that the receipt of EGS stigmatizes workers. Furthermore, asked to compare wage subsidies to employers and the EGS, around two third of respondents preferred EGS.

9 Average information effects on labor market outcomes

In a second step, we focus on differences between the treatment and the control group in outcome variables merged from the register data. Similarly to the survey data, Table 4 displays means of outcome variables for the entire sample and by age group, while Table 5 presents results from reduced form OLS-estimates. While results do not change much if we include interactions of the treatment dummy with further stratification variables, effects become smaller if we additionally control for interactions of all stratification variables.

The first outcome variable of interest is take-up of EGS. On average, only one percent of the workers in our sample participated in the program. Take-up rates decreased with age, increased for potentially displaced workers, and also varied with the labor market region individuals were located in. We find a positive significant effect of brochure receipt in the size of around 2 to 3 percentage points for recipients in the age range 50–59. At the first glance, this effect seems to be small – it is, however, economically important as (compared to the reference group in the estimates) receiving the brochure increased the take-up rate by around 40 percent for those aged 50–54, and by around 150 percent for the age group 55–59. On average the subsidy amounted to 300 Euro per months for all age groups under consideration.

Over the entire sample, four percent of the workers took up a job during the year 2011. This share increased to 14 percent until the end of the year 2012. On average, during the 15 months period from October 2011 to December 2012, workers spend around one month in employment. As would have been expected, shares in employment and days employed

strongly decrease with age, but increase for potentially displaced workers.

Note that entry into EGS was only possible until the end of the year 2011, due to the abolition of the program. Consequently, the workers only had three months to find and accept a new job after brochure receipt if they wanted to participate in the program. In fact, we find that the information treatment increased the share of workers aged 55–59 to find (and accept) a job until the end of the year 2011 by 2 percentage points – while it even decreased the take-up rate of slightly younger workers by 2 percentage points. In the longer run the effect vanishes for the younger among the older individuals, but increases to 4 percentage points for those aged 55–59. Again, while apparently small at a first glance, compared to the reference group the latter corresponds to nearly 20 percent higher entry rates into jobs.

Accordingly, during the entire observation period, treated workers in the age group 50–54 (55–59) spent on average one week less (more) in employment. Earnings are largely driven by days spent into employment. We find significant positive effects of the treatment on earnings for workers in the age group 55–59, who on average earned about 700 Euro more than similar workers in the control group. In contrast, treated workers in the age group 50–54 experienced an on average wage loss of nearly 1,600 Euro. Compared to the reference groups, this implies gains of around 7 and losses of around 5 percent. Beyond employment, retirement is another option that is open to many older workers. We find that in fact around 60 percent of workers in the oldest age group (60 and older) retired from the labor market. The information treatment had, however, no significant impact on the probability to retire.

To summarize, the reduced form estimates provide some evidence that the information treatment had positive effects on the labor market prospects of workers in the age group 55–59. In contrast, it seems that the information treatment slightly induced more program take-up in the age group 50–54, while it slightly decreased employment rates. Taken together, this indicates deadweight and might also be related to lower search efforts (see Table 3). Finally, we did not find any influence of the information treatment on the outcomes of workers in the age group of 60–64.

10 Local average awareness effects on the labor market outcomes of compliers

As had been outlined above, brochure receipt can be used as an instrument to estimate local average treatment effects of (additional) awareness of the program on labor market prospects of individuals, who would not have known about the program without the treatment. As we saw above, the information treatment increase the share of individuals knowing about the program by around 20 percentage points. Intuitively, one would expect these LATE to equal reduced form effects multiplied with factor five, as differences in outcomes between the treatment and the control group will now be contributed to this 20-percent-share of compliers – persons, who would not have known about the program without receiving the information brochure, but are aware of it as a result of the information treatment.

Table 6 presents results from non-parametric Wald estimates. These estimates basically relate differences in outcome variables in the register data to differences in shares in program awareness in the survey data. The first panel presents estimates which take into account age stratification, the second panel additionally stratification by potential displacement, and the third panel additionally stratification by local labor market regions.

The first panel confirms the positive effects found in the reduced form OLS-estimates for the age group 55–59; with considerably larger coefficients (as was expected). However, effects of awareness on labor market outcomes become mostly insignificant if we take into account also stratification by potential displacement and region. In the second and third panel, the only significant effect is found for the take-up rate of EGS. Across all age groups, it is 5 percentage points higher for compliers, mainly due to an increase by 14 percentage points in the age group 55–59.

Why do results become insignificant? First, potentially displaced workers – who seem to have better chances of taking up a new job – are over-represented in the group of brochure recipients, so effects diminish once we control for displacement. Second, insignificance might be partly a consequence of the comparatively small number of compliers (those who knew about EGS only because of the brochure), in particular in the survey data. While reduced in size, the sign of estimated effects does not change once we control for further covariates.

Estimated LATE of awareness on labor market outcomes from the 2S2SLS-estimator – for the entire sample as well as by age group – are displayed in Table 7. In all estimates, we control for the entire range of stratification variables. For the group of compliers, we find again significant effects on the take-up rate of EGS, which are related to higher take-up rates in the group of men aged 55–59 (around 12 percentage points). Again we find no significant effects on further labor market outcomes of compliers, but signs of estimated effects are the same as in the reduced form estimates.

We conclude for the group of compliers that the LATE estimates basically confirm our reduced form result: While mostly insignificant when taking into account stratification of the sample, estimated coefficients are larger than in the reduced form estimates and retain their sign.

11 Conclusions

In this paper, we suggest an innovative approach to analyze the effectiveness of a German wage support program: For worker of age 50 and older who took up a job where they earned less than in their last employment spell, the program covered half of the net wage difference during the first year in the new job, and 30 percent of the difference in the second year. A field experiment involving an information treatment allows us to infer to what extent additional information about the program increased knowledge about the wage support. Furthermore, the treatment can be used as an instrument for knowledge on the program, to estimate local average treatment effects on different labor market outcomes.

On October 1, 2011, we sent out an information brochure about the program to randomly selected eligible men we identified from the register data, while the remaining group of eligible persons constituted the control group. Unemployed workers had the opportunity to claim access to the program until the end of the year 2011. In a follow-up survey we asked both groups about the awareness of the program as well as about potential effects on their search behavior. Merged register data – that cover the time period until the end of the year 2012 – also allow us to investigate a broad range of outcome variables, as take-up of the program, take-up of a job, days in employment and unemployment, earnings, and retirement.

Reduced form OLS estimates show: The treatment significantly increased awareness of the program among eligible men by 20 percentage points, but had only modest behavioral consequences on their (stated) search behavior. Take-up rates of EGS were higher by around 2 to 3 percentage points for treated persons in the age groups 50–54 and 55–59. Only for the latter group, however, we find significant positive effects on the take-up rate of jobs of roughly the same size. This size is similar to the information treatment effect found by e.g. Altmann et al. (2015), Huysentryt and Lefevere (2010) as well as Liebman and Luttmer (2015). Furthermore, for the age group 55–59 the average effect on accumulated days of employment amounted to around a week of additional employment and resulted in average in 700 Euro of additional earnings over the course of 15 months.

For the group of compliers – individuals who were informed about the program only because of our treatment – we find that awareness on the program significantly increased take-up rates of EGS by more than 10 percentage points in the age group 55–59. While LATE of program knowledge on further labor market outcomes become insignificant once we control for stratification, the estimated effects increase compared to the reduced form results and maintain their sign.

Thus, for the younger among the older workers, additional participation in the program resulting from brochure receipt was pure windfall. The older among the older workers might have such low re-employment probabilities anyway that we fail to detect any effect of our treatment. Only for workers of age 55–59, additional program take-up manifested itself at least partly in positive effects on employment outcomes. All in all, we conclude that additional information about the meanwhile abolished program had the potential to help a particular age group of unemployed in taking-up a job.

From a fiscal perspective, what matters for efficiency of a treatment are mainly the effects on a) costs (in our case additional spending on EGS) and b) benefits (savings in unemployment benefits, additional tax revenues and social security contributions from additional earnings), taking into account potential windfall gains. With our experiment, we estimate the effects of additional information on the program for particular age groups, but cannot identify the effects of participation in the program as such. However, we can present some back-of-the-envelope considerations. Among compliers, the program can at best have been efficient for those aged 55–59 (as the additional information resulted in windfall or was not effective for the other age groups investigated). In this age group,

we have 650 treated persons, from which around 3 percent (or 20 in total) took up EGS because of brochure receipt. As we had outlined, subsidized persons receive on average a monthly income support of 300 Euro during the first year of subsidization. If each of the compliers would have received income support for two years (300 per months in the first year, 180 per months in the second year), costs per person would have been 5,760 Euro, or around 115,200 Euro in total. Additional gross earnings of in average 740 Euro for around 650 brochure recipient in the age group 55–59 add up to around 480,000 Euro. Even if no savings in benefits occurred, this should have been sufficient to finance additional EGS payments through additional tax revenues and social security contributions. On the other hand, around 560 brochure recipients aged 50–54 suffered a loss of in average nearly 1,600 Euro, which sum up to overall losses of around 890,000 Euro (so additional program cost could not have been covered even in the best of circumstances). Thus, the policy lesson derived from our analysis is that providing additional information on such a program could be economically sound, provided that adequate groups of potential participants – in our case the age group 55–59 – are targeted. For the younger among the older workers, however, the information treatment turned out to be harmful instead of helpful.

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Tables

Table 1: Descriptive statistics

Data Group	Register		Survey	
	Treated	Controls	Treated	Controls
<i>Stratification variables</i>				
Age 50 to 54	0.24	0.09	0.21	0.21
Age 55 to 59	0.28	0.24	0.30	0.28
Age 60 to 64	0.48	0.67	0.49	0.52
Potential displacement	0.51	0.18	0.49	0.52
No potential displacement	0.49	0.82	0.51	0.48
Urban, strong labor market	0.21	0.22	0.18	0.21
Urban, high unemployment	0.24	0.24	0.24	0.24
Rural, above average unemployment	0.23	0.23	0.23	0.23
Rural, low unemployment	0.15	0.16	0.15	0.14
East Germany, weak labor market	0.18	0.16	0.21	0.18
<i>Individual characteristics</i>				
No degree	0.14	0.14	0.10	0.08
Vocational training or Abitur	0.71	0.71	0.71	0.72
University degree	0.14	0.15	0.19	0.21
German nationality	0.94	0.93	0.96	0.96
Non-German nationality	0.06	0.07	0.04	0.04
<i>Before treatment date</i>				
Up to 6 months unemployed	0.44	0.36	0.45	0.44
6 to 12 months unemployed	0.36	0.40	0.36	0.37
More than 12 months unemployed	0.20	0.24	0.19	0.19
<i>5 years before treatment date</i>				
Up to 4 years employed	0.29	0.32	0.26	0.26
4 to 4.5 years employed	0.36	0.40	0.37	0.38
More than 4.5 years employed	0.35	0.28	0.37	0.36
<i>Last gross wage before treatment date</i>				
Up to 75 Euro/day	0.24	0.21	0.22	0.18
75 to 100 Euro/day	0.30	0.28	0.27	0.24
100 to 150 Euro/day	0.26	0.28	0.29	0.29
More than 150 Euro/day	0.20	0.24	0.22	0.29
Observations	2328	19642	746	790

Source: IEB and survey data.

Table 2: Means of survey data outcome variables and assessment of EGS

Age Group	All	50-54		55-59		60 and older	
		Treated	Controls	Treated	Controls	Treated	Controls
<i>Outcomes</i>							
Aware of EGS (1=yes)	0.76	0.82	0.61	0.90	0.71	0.86	0.68
Induced more active search (1=yes)	0.33	0.27	0.34	0.34	0.35	0.30	0.35
Would accept less paid work (1=yes)	0.45	0.52	0.45	0.58	0.51	0.43	0.32
Reservation wage ¹	2149	2238	2067	2050	2144	2114	2234
<i>Assessment</i>							
Suited measure (1=yes)	0.72	0.71	0.73	0.75	0.75	0.72	0.67
Stigmatizes worker (1=yes)	0.20	0.14	0.13	0.17	0.25	0.20	0.25
Wage subsidy better than EGS	0.21	0.22	0.17	0.17	0.22	0.24	0.23
EGS better than wage subsidy	0.63	0.67	0.69	0.64	0.55	0.65	0.60
Observations	1536	160	163	224	220	362	407

Source: Survey data.

¹ Lower number of observations, see Table 3.

Table 3: Reduced form OLS-estimates for survey data outcomes

Outcome	Aware of EGS	More active search	Would accept less paid work	Reservation wage
<i>Interactions age and treatment</i>				
Age 50 to 54 x treated	0.21*** (0.05)	-0.06 (0.05)	0.07 (0.05)	174 (152)
Age 55 to 59 x treated	0.19*** (0.04)	-0.01 (0.04)	0.07 (0.05)	-99 (130)
Age 60 to 64 x treated	0.18*** (0.03)	-0.06* (0.03)	0.11*** (0.04)	-103 (98)
<i>Stratification variables and constant</i>				
Age 55 to 59	0.09** (0.04)	0.01 (0.05)	0.04 (0.05)	123 (143)
Age 60 to 64	0.05 (0.04)	0.00 (0.04)	-0.16*** (0.05)	295** (131)
Potential displacement	0.03 (0.02)	0.06** (0.03)	0.08*** (0.03)	-361*** (74)
Urban, strong labor market	-0.05 (0.03)	-0.01 (0.04)	-0.02 (0.04)	814*** (111)
Urban, high unemployment	-0.05* (0.03)	-0.08** (0.04)	0.01 (0.04)	633*** (106)
Rural, above average unemployment	-0.04 (0.03)	-0.04 (0.04)	0.00 (0.04)	587*** (107)
Rural, low unemployment	-0.01 (0.04)	0.04 (0.04)	0.02 (0.04)	506*** (121)
Constant	0.64*** (0.04)	0.34*** (0.04)	0.43*** (0.05)	1,651*** (129)
Observations	1536	1536	1536	1452
Adj. R-squared	0.05	0.01	0.03	0.05

Source: Survey data.

Standard errors in parentheses.

*) Significant at 0.10.

**) Significant at 0.05.

***) Significant at 0.01.

Table 4: Means of register data outcome variables

Age Group	All	50-54		55-59		60 and older	
		Treated	Controls	Treated	Controls	Treated	Controls
<i>Until end of 2011</i>							
EGS (1=yes)	0.01	0.07	0.05	0.05	0.02	0.01	0.003
New job (1=yes)	0.04	0.14	0.16	0.08	0.06	0.02	0.01
<i>Until end of 2012</i>							
New job (1=yes)	0.14	0.54	0.52	0.27	0.22	0.06	0.04
Days employed	31	119	126	58	47	12	8
Earnings in Euro	2824	10849	12428	5033	4106	1021	743
Retired (1=yes)	0.40	0.00	0.00	0.04	0.04	0.58	0.60
Observations	21970	559	1810	650	4707	1119	13125

Source: IEB.

Table 5: Reduced form OLS-estimates for register data outcomes

	Until end of 2011		Until end of 2012			
Outcome	EGS	New job	New job	Days employed	Earnings	Retired
<i>Interactions age and treatment</i>						
Age 50 to 54 x treated	0.02*** (0.01)	-0.02** (0.01)	0.01 (0.01)	-8** (4)	-1,593*** (429)	0.00 (0.02)
Age 55 to 59 x treated	0.03*** 0.00	0.02** (0.01)	0.04*** (0.01)	7** (3)	739** (373)	0.00 (0.02)
Age 60 to 64 x treated	0.00 0.00	0.00 (0.01)	-0.01 (0.01)	-2 (3)	-109 (286)	-0.02 (0.01)
<i>Stratification variables and constant</i>						
Age 55 to 59	-0.02*** 0.00	-0.10*** (0.01)	-0.30*** (0.01)	-79*** (2)	-8,284*** (245)	0.04*** (0.01)
Age 60 to 64	-0.04*** 0.00	-0.15*** 0.00	-0.48*** (0.01)	-118*** (2)	-11,624*** (223)	0.60*** (0.01)
Potential displacement	0.01*** 0.00	0.02*** 0.00	0.06*** (0.01)	14*** (1)	803*** (151)	-0.01 (0.01)
Urban, strong labor market	-0.01*** 0.00	-0.01** 0.00	-0.02*** (0.01)	-3 (2)	549*** (197)	-0.03*** (0.01)
Urban, high unemployment	-0.01*** 0.00	-0.01*** 0.00	-0.05*** (0.01)	-9*** (2)	-27 (194)	-0.04*** (0.01)
Rural, above average unemployment	-0.01*** 0.00	-0.01*** 0.00	-0.03*** (0.01)	-4** (2)	286 (196)	-0.02** (0.01)
Rural, low unemployment	-0.01** 0.00	-0.01 0.00	-0.03*** (0.01)	-3 (2)	191 (213)	0.00 (0.01)
Constant	0.05*** 0.00	0.16*** (0.01)	0.54*** (0.01)	127*** (2)	12,021*** (256)	0.03** (0.01)
Observations	21970	21970	21970	21970	21970	21970
Adj. R-squared	0.02	0.06	0.21	0.17	0.13	0.31

Source: IEB.

Standard errors in parentheses.

*) Significant at 0.10.

**) Significant at 0.05.

***) Significant at 0.01.

Table 6: Local average treatment effects of program awareness, Wald-estimates

Outcome	Until end of 2011		Until end of 2012			
	EGS	New job	New job	Days employed	Earnings	Retired
<i>LATE with age covariates</i>						
All	0.07*** (0.02)	0.05* (0.02)	0.15*** (0.04)	25** (10)	1251 (1007)	-0.08 (0.05)
<i>LATE by age groups</i>						
Age 50 to 54	0.10 (0.07)	-0.10 (0.10)	0.08 (0.13)	-34 (40)	-7694 (5231)	-0.01 (0.01)
Age 55 to 59	0.16*** (0.06)	0.13* (0.07)	0.30** (0.12)	58* (30)	4,924* (2910)	0.01 (0.05)
Age 60 to 64	0.03* (0.02)	0.04* (0.02)	0.11** (0.05)	23** (11)	1494 (1013)	-0.13 (0.09)
<i>LATE with age and displacement covariates</i>						
All	0.05*** (0.02)	0.02 (0.02)	0.04 (0.04)	2 (9)	-220 (954)	-0.04 (0.06)
<i>LATE by age groups, with displacement covariate</i>						
Age 50 to 54	0.10 (0.07)	-0.10 (0.11)	0.06 (0.14)	-37 (41)	-7552 (5668)	-0.01 (0.01)
Age 55 to 59	0.14** (0.06)	0.07 (0.06)	0.17 (0.11)	30 (28)	3522 (3034)	0.04 (0.06)
Age 60 to 64	0.01 (0.02)	0.02 (0.03)	0.00 (0.04)	0 (9)	-257 (854)	-0.07 (0.10)
<i>LATE with age, displacement, and regional covariates</i>						
All	0.05*** (0.02)	0.02 (0.02)	0.04 (0.04)	2 (9)	-58 (1006)	-0.05 (0.07)
<i>LATE by age groups, with displacement and regional covariates</i>						
Age 50 to 54	0.08 (0.09)	-0.11 (0.17)	0.03 (0.22)	-45 (81)	-7156 (9471)	-0.01 (0.01)
Age 55 to 59	0.14** (0.06)	0.07 (0.07)	0.14 (0.12)	26 (29)	3692 (3213)	0.04 (0.06)
Age 60 to 64	0.01 (0.02)	0.03 (0.03)	0.01 (0.04)	0 (9)	-164 (902)	-0.09 (0.10)
<i>Observations</i>						
All	21970	21970	21970	21970	21970	21970
Age 50 to 54	2369	2369	2369	2369	2369	2369
Age 55 to 59	5357	5357	5357	5357	5357	5357
Age 60 to 64	14244	14244	14244	14244	14244	14244

Source: IEB and survey data.
Standard errors in parentheses.
*) Significant at 0.10.
**) Significant at 0.05.
***) Significant at 0.01.

Table 7: Local average treatment effects of program awareness, 2S2SLS-estimates

	Until end of 2011		Until end of 2012			
	EGS	New job	New job	Days employed	Earnings	Retired
<i>LATE with age, displacement, and regional covariates</i>						
All	0.06*** (0.02)	-0.01 (0.03)	0.06 (0.05)	-4 (12)	-1,033 (1319)	-0.05 (0.04)
<i>LATE by age group and with displacement and regional covariates</i>						
Age 50 to 54	0.08 (0.10)	-0.12 (0.14)	0.05 (0.20)	-46 (58)	-7,551 (7805)	-0.01 (0.01)
Age 55 to 59	0.12** (0.06)	0.06 (0.07)	0.10 (0.11)	16 (28)	2246 (2749)	0.02 (0.05)
Age 60 to 64	0.03* (0.02)	0.02 (0.03)	0.05 (0.04)	9 (10)	591 (946)	-0.09 (0.09)
<i>Observations</i>						
All	21970	21970	21970	21970	21970	21970
Age 50 to 54	2369	2369	2369	2369	2369	2369
Age 55 to 59	5357	5357	5357	5357	5357	5357
Age 60 to 64	14244	14244	14244	14244	14244	14244

Source: IEB and survey data.

Standard errors in parentheses.

Controls: Stratification variables.

*) Significant at 0.10.

**) Significant at 0.05.

***) Significant at 0.01.