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Fast track to the labour market or highway to hell? The effect of activation policies on quantity and quality of labour market integration

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Fast track to the labour market or highway to hell? The effect of activation policies on quantity and quality of labour market integration

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Fast track to the labour market or highway to hell? The effect of activation policies on quantity and quality of labour market integration.

Abstract

Activation policies such as sanctions, workfare employment and counselling and monitoring schemes have been found to speed up labour market reintegration. At the same time, it has been suspected that this quicker reintegration is paid for with worse job quality, e.g. in terms of lower wages. I contribute to this discussion by analysing the effects of a workfare (counselling and monitoring) scheme from Germany on employment probability and post-unemployment wages via regression-adjusted matching estimations. This scheme tightens behavioural requirements for unemployed workers but also offers support in terms of more intense counselling. The results point to a strongly positive effect on employment probability but no effect on wages. They are robust to changes in the matching algorithm, and placebo tests refute concerns about endogenous selection or substitution effects. These findings contrast the results from previous research on sanctions, which confirmed a negative effect on job quality. This puzzle suggests that the existence of adverse effects on job quality depends on the type of activation programme. While it may indeed be there for very intense kinds of activation, it can be avoided if the right balance between pressuring and supportive components is found.

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1 INTRODUCTION

Most post-industrialized countries have experienced an activating turn in social and labour market policy, which is characterized by a huge diversity of different active labour market and activation policies (Eichhorst, Kaufmann and Konle-Seidl, 2008; Kenworthy, 2010; Vlandas, 2013; Haskins, 2015). They range from long training programmes aimed at human capital accumulation to activation policies such as sanctions, public workfare employment as well as counselling and monitoring schemes which put a strong focus on quick reemployment.[1] Unlike long training programmes, activation policies have the advantage that they are relatively inexpensive and easy to implement. Moreover, they are likely to yield quick returns because they have an immediate influence on targeted unemployed worker. From an economic and fiscal point of view, it is thus tempting to focus on activation policies, especially in times of high unemployment and tight budget constraints (Van der Klaauw and Van Ours, 2013; Andersen and Svarer, 2014). In contrast, scholars of social and public policy have raised the concern that quicker, activation-induced labour market integration is paid for with worse job quality, e.g in terms of lower wages (Barbier and Ludwig-Mayerhofer, 2004; Taylor-Gooby, 2004; Dingeldey, 2007). If unemployed workers are pushed into the labour market by all means, it is possible that they are forced to apply for and accept available job offers which are worse than the best job they could have reached with less pressure but more support and time for job search. This raises the fundamental question whether activation policies face a quantity-quality trade-off regarding their influence on labour market (re-) integration.

Exploring whether such a trade-off is present is of great importance in at least three regards. From the perspective of the individual, quicker but worse labour market integration is likely

[1] There are different distinctions of active labour market and activation policies in the economic and social policy literature. In the terminology used here, activation programme refers to policies that have variously been labelled as workfare (Dingeldey 2007), liberal (Barbier and Ludwig-Meyerhofer 2004) or repressive (Vlandas 2013) activation, and include sanctions, workfare employment and counselling and monitoring schemes.

to result in lower levels of job and life satisfaction. From a more structural, political economy perspective on the labour market, low job quality of reintegrated workers bears the danger of increasing labour market dualization. In presence of a rapidly increasing gap between labour market insiders and outsiders, who experience frequent transitions between unemployment and unstable reemployment, high quality of labour market reintegration is of growing importance to keep outsiders from falling into a low-wage trap (Emmenegger et al., 2012; Schwander, 2012; Fervers and Schwander, 2015). Finally, from an economic point of view, it is questionable whether quicker but worse labour market integration is beneficial in the long-run, because worse job quality may incur human capital losses thus contributing to higher unemployment over the life course (Van den Berg and Vikström, 2014).

Despite the growing body of policy and programme evaluations in this field, evidence on the effect of activation policies on job quality is very limited. I contribute to this discussion by analysing the effects of a large-scale activation (counselling and monitoring) scheme from Germany[2] on quantity and quality of labour market integration, measured by employment probability and post-unemployment wages, respectively. I combine administrative data from various sources to conduct matching estimations with regression adjustment. The credibility of the analysis is enhanced by the high quality of the data, as well as the institutional setting of the programme which allows testing more rigorously for endogenous selection and substitution effects than many previous evaluations could.

The remainder of this paper is organized as follows. I start section 2 with some theoretical considerations on the expected effects of workfare programmes (2.1). Moreover, I review existent evidence on this issue (2.2). Subsequently, I present my empirical analysis in section 3. I start with a short description of the programme under discussion (3.1), which is followed by the explanation of the data sources and variables (3.2) as well as the identification strategy

(3.3). Afterwards, I present and discuss the results of the treatment effect estimation including robustness and specification analyses (3.4). The last section (4) concludes with a short summary of the results and implications for future research and policy-making.

2 ACTIVATION POLICIES – A ROAD TO QUICK BUT DIRTY INTEGRATION?

2.1 Theoretical Considerations

The starting point for the concern about a quality-quantity trade-off is the goal of activation policies. They aim at quick reintegration, whereas job quality is regarded as less important, i.e. "emphasis is placed on the pressure or even compulsion for the unemployed (…) to (re-) enter the labour market, even with low-income-jobs" (Dingeldey, 2007, p. 825). This implies that the unemployed are encouraged or even forced to accept "any job on the market as it is" (Barbier and Ludwig-Meyerhofer, 2004, p. 27). Standard search theory argues that unemployed workers adapt their search behaviour if labour market policies change their utility of ongoing job search or immediate reemployment, respectively (as outlined in the context of unemployment benefits by Katz and Meyer, 1990). The focus on quick reemployment may therefore contribute to a quality-quantity trade-off in three ways. First, they force unemployed workers to be less selective with regard to available job offers. If, for example, threatened with sanctions (which induce a severe drop in the utility of ongoing job search), the unemployed have no other choice but to apply for and accept any available job, even if there may be better job offers to come. Second and relatedly, the pressure of activation programmes shortens the time that is available for job-search. If the time for job-search needed to find the best available job is, say, one year, but the activation programme forces targeted unemployed workers to find and accept jobs within a shorter period of time, this will contribute to inferior job quality (Burdett 1979; Gangl 2006). Finally, and looking at this issue

[2] The results presented here are part of an official evaluation conducted on behalf of the German Federal Ministry of Labour

from a more sociological or social psychological perspective, activation programmes may lead to social stigma for targeted workers (which also reduces the utility of ongoing job search). Suffering from such a stigma may again lead to the acceptance of jobs which are worse than the best job that would have been found without this stigma (Wulfgramm, 2014). All these three mechanisms can be expected to lead to quicker reintegration, but also to worse job quality. A similar argument has been outlined with regard to unemployment benefits. Benefits of short duration and low level influence the unemployed in a similar way activation programmes do, they pressure them to accept available jobs quickly. Even though empirical evidence is somewhat mixed here, it shows that intense pressure on unemployed workers can have non-negligible negative effects on job quality (Gangl, 2006; Tatsiramos, 2009; Caliendo et al., 2013).

It has to be considered that these arguments generally apply for most kinds of activation programmes, but not necessarily in the same way. They differ in their intensity and the mix of pressuring and supportive components. If a programme entails severe sanctions after a very short period of time, the resulting positive (negative) effect on quantity (quality) of reemployment is likely to be very strong. In contrast, if a counselling and monitoring scheme increases the pressure on unemployed workers but also includes counselling services such as profiling or information about available job offers, the trade-off is likely to be much weaker. If the supportive components are strong enough, they may even counterbalance the negative impact of the pressuring components completely. Therefore, a sound knowledge of the institutional setting of an activation programme is of great relevance with regard to considerations concerning external validity. General conclusions should only be made with regard to programmes which are rather similar in their mix of pressure and support (and eventually also other institutional characteristics).

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and Social Affairs. The results presented here do not necessarily represent its own opinions and views.

2.2 Previous Evidence

I briefly summarize existent evidence on the effects of activation policies. Following the aforementioned argument, I distinguish between different kinds of activation programmes, namely sanctions, counselling and monitoring programmes and public workfare employment.

Sanctions are probably the most intense kind of activation. In their most extreme form, they withdraw any income from the unemployed, leaving them with very little choice concerning the compliance to their obligations. Previous research on sanctions has initially put a strong focus on the effect on the quantity of employment (measured by the probability of exit from unemployment or benefit receipt as well as outflow into employment). Overall, the results are quite optimistic. Positive impacts on one or more of these variables have been found in a number of studies for different countries, including Switzerland (Lalive, Ours and Zweimüller, 2005), Netherlands (Van den Berg, Van der Klaauw and Van Ours, 2004; Van der Klaauw and Van Ours, 2013) and Germany (Boockmann, Thomsen and Walter, 2014; Hillmann and Hohenleitner, 2015). The magnitudes of the effects differ, but they are mostly reported to be very strong. For example, Lalive, Ours and Zweimüller (2005, p. 1404) estimate that the exit from unemployment (all else being equal) increases by 25 percent if unemployed workers are threatened with sanctions, which is followed by another increase of 20 percent if the sanction is actually imposed. In sum, it has been concluded that sanctions are an effective means of increasing exit from unemployment and benefit dependency as well as reemployment probability (for a meta-analysis see Kluve, 2010). At the same time, this rather optimistic view has recently been challenged by empirical evidence which revealed a negative impact on job quality. Arni, Lalive and Van Ours (2013) rely on Swiss register data and detect a negative influence on post-unemployment wages and job stability. Similarly, Van den Berg and Vikström (2014) combine Swedish register data and information from a large-scale employer survey and confirm a negative effect on job quality in terms of wages, occupational

level and the probability to move to a part-time job. These findings confirm the concern that quicker integration achieved by the means of sanctions is paid for with worse job quality.

Workfare employment (employment programmes that have to be carried out in exchange for benefits) and counselling and monitoring schemes are less intense kinds of activation. They may be accompanied with sanctions in case of non-compliance, but also consist of supportive components. Counselling and monitoring schemes provide better information about available and suitable job offers, whereas workfare employment may support the unemployed to get used to regular working activities again. Therefore, one would expect a weaker effect on the quantity of employment but also less negative effects on job quality. Indeed, the results concerning the impact of counselling and monitoring schemes on labour market integration are more mixed. Neither Gorter and Kalb (1996), Ashenfelter, Ashmore and Deschênes (2005), Van den Berg and Van der Klaauw (2006), nor Manning (2009) find any effect of counselling and monitoring schemes. In contrast, positive effects are reported by Dolton and O'Neill (2002), Graversen and Van Ours (2008), McVicar (2010), Hägglund (2011) as well as Cockx and Dejemeppe (2012). Once again, the results differ not only between but also within studies. For example, the randomized experiment conducted by Hägglund (2011, p. 92) yields an increase in the outflow from unemployment (even before programme start) of about 50 percent in Jämtland, whereas the effect is insignificant for the three other Swedish counties. The estimates of Graversen and Van Ours (2008, p. 2031) translate into a relative effect on the job finding rate of 30 percent, whereas McVicar (2010, p. 311) reports that the abandonment of counselling and monitoring has led to a 15 percent increase of registered unemployment. All in all, the effect on employment status has been reported to be either positive or insignificant. However, none of the aforementioned considers the impact on job quality.

The picture is similarly mixed for public workfare employment (workfare employment refers to public employment programmes which have to be carried out in exchange for benefit receipt. They primarily aim at testing the compliance of unemployed workers). On the one hand, the studies conducted by Huber et al. (2011) and Hohmeyer and Wolff (2012) both conclude that a large-scale public workfare programme from Germany (the so-called One-Euro-Jobs) does not foster labour market integration. On the other hand, Bennmarker, Nordström, Skans and Vikman (2013) exploit a natural experiment from Sweden and estimate that the threat effect of a workfare programme on outflow from unemployment amounts to 10 percent. Their study is also one of the rare ones which explicitly considers the quality of labour market reintegration. In contrast to the studies on sanctions, they do not find a negative effect on post-unemployment wages. This is consistent with the aforementioned argument that the expected quality-quantity trade-off is likely to be weaker. To sum up, previous research yields either positive or insignificant effects of counselling and monitoring schemes as well as public workfare employment on employment probability.

Thinking about the arguments outlined in the public and social policy literature, it becomes quite clear where the gaps in the literature are. Activation and active labour market policies are usually conceptualized as a continuum, with exclusively pressuring programmes (e.g. sanctions) on the one and programmes with strongly supportive components (e.g. long training programmes) on the other end (see e.g. Barbier and Ludwig-Mayerhofer, 2004; Taylor-Gooby, 2004; Dingeldey, 2007; Vlandas, 2013). As the literature review indicates, there is strong evidence that programmes that lie at the extreme point of the continuum (namely sanctions) indeed hurt job quality. However, considering the theoretical arguments on the causal mechanisms of activation policies, it becomes apparent that we may observe different effects if we move away from the extreme point of the continuum towards programmes which combine pressuring and supportive components, namely counselling and

monitoring schemes or public workfare employment. In this regard, analysing the effect of e.g. counselling and monitoring programmes on job quality and quantity is a major gap in the literature.

3 EMPIRICAL EVIDENCE

To contribute to this discussion, I now present my analysis of the impact of a counselling and monitoring scheme from Germany (called "Activating Citizens") on job quality and labour market reintegration. Before I present the actual empirical analysis, I outline its institutional features, which make it particularly interesting in the given theoretical context.

3.1 Activating Citizens

Activating Citizens is a large scale counselling and monitoring programme from Germany. Programme participation started between July 2010 and June 2011 with altogether 138,010 participants, who were scattered throughout the whole country. This makes it one of the largest active labour market policy (ALMP) programmes in Germany during this time. It is not part of the regular set of ALMP instruments, but a special programme co-funded by the European Social Fund.[3] In addition to these basic facts, there are a couple of features which are of relevance for internal and external validity, namely the content of the programme as well as the mode of implementation.

While there are some differences in the administration of the programme between different regions, it essentially consists of more and intensive counselling services and monitoring of job search behaviour. This includes more frequent contacts between the targeted unemployed worker and its counsellor. Additionally, short courses such as job application training could have been part of the programme. However, these courses are not aimed at systematic human

[3] The results presented here are part of an official evaluation that has been conducted on behalf of the German Federal Ministry of Labour and Social Affairs.

capital accumulation but rather test the compliance of participants. If the unemployed do not comply with their legal obligations defined by the programme, they are threatened with sanctions in terms of benefit withdrawal. The period of increased counselling and monitoring usually lasts for six months, and participation is mandatory in most job centres (the legal employment agencies). The goal pursued with the programme is direct labour market integration with nothing being specified on the type or quality of the job. The target group was rather broadly defined: all participants who rely on social assistance benefits but are physically able to work could have been selected as participants (receipt of social assistance in Germany mostly starts after a period of unemployment of at least one year but is then unlimited). For the identification strategy, it is crucial to note that the implementation mirrors the structure of a (nonrandomized) multi-level experiment (see e.g. Sinclair, McConnell and Green, 2012), i.e. there are participating and non-participating job centres, as well as participants and non-participants within participating job centres. Finally, it hast to be mentioned that participants who cannot be successfully integrated into the labour market during this scheme can apply for a job in a public employment scheme. While this paper focusses on the counselling and monitoring scheme only, this has to be considered for the interpretation of the long-term effects. For time periods of more than six months after programme start, the displayed treatment effects estimate the common effect of the counselling and monitoring and public employment scheme. Moreover, it has to be considered that reductions of the outflow into employment immediately before the end of the counselling and monitoring scheme may result from anticipation effects.

There are at least four institutional features that make this activation scheme particularly interesting in the given theoretical context. First, it sticks to the goal of quick reemployment but also consists of supportive components: Participation is mandatory, and non-compliance is sanctioned, but these sanctions are only the last resort. The initial attempt of the programme

is to reintegrate targeted workers by more intensive counselling. Second and relatedly, it is a rather typical counselling and monitoring scheme without extraordinary features. While it may also be of interest to focus on rather extreme cases, the scheme under discussion allows for more general conclusions with regard to counselling and monitoring schemes as a whole. Thirdly, the target group is rather broadly defined and therefore constitutes a more representative picture of all long-term unemployed workers in Germany, again contributing to higher degree of external validity. Finally, the large number of participants again increases the political relevance but also tends to support the generalizability of the results.

3.2 Data and Variables

I rely on register data to identify the effect of the programme, the Integrated Employment Biographies (IEB). The IEB is an administrative dataset that is commonly used in German ALMP evaluations, and combines information from all social security records. It therefore contains daily information on all spells of all persons who are employed, unemployed, participate in an ALMP programme or receive social assistance. It is not publicly available, but the necessary parts of the dataset are directly delivered to researchers on request and for clearly defined purposes. I have access to four subsamples of this database, one sample of treatment observations and three different samples of control units. The sample of treatment observations is a 50 percent random sample of all participants, which amounts to 69,005 treated observations. They have all started programme participation between July 2010 and June 2011. The three samples of control units each consist of 125,000 observations. The first group of control observations consists of persons from participating job centres who would have been eligible for programme participation (i.e. have been unemployed and received social assistance at some point between July 2010 and June 2011), but did not actually participate. This sample will serve as the basis for the matching analysis. The two other samples will be used to identify substitution effects (see section 3.3). They both consist of

individuals who have been or become unemployed between July 2008 and June 2009. One sample is drawn from job centres which have later participated in the programme, the other one is drawn from non-participating job centres.

Some sample restrictions have been imposed, but mainly for technical reasons. The only substantive restriction is that people had to be older than 17 but younger than 60 years, because these groups of workers might be treated very differently by employment agencies. Moreover, observations have been discarded from the analysis if they have missing, strongly conflicting or unreliable information on very important covariates (e.g. gender) or on treatment information. For example, observations are discarded if their individual information indicates that they have participated in the programme, but they are administered by a job centre that does not participate in the programme at all. Even though the cleaning process leads to a loss of observations, the analysis can still rely on 63,707 treatment and 103,644 control observations.

Following my argument outlined in section 2.1, I use two dependent variables. To measure the quantity of reemployment I use a simple 0/1 indicator that is equal to one if someone is regularly employed and zero otherwise. Moreover, the dataset contains information on absolute daily wages, which are used as proxy for job quality. In the estimations for wages, I follow the approach by Bennmarker, Nordström, Skans, and Vikman (2013) who only rely on the wage information for persons who are actually employed (the implications of this measurement are discussed in section 3.4). Both variables are recorded in monthly intervals beginning from individual participation.[4] Additionally, the dataset consists of a rich set of covariates. Generally speaking, all variables that affect programme participation and the outcome should be included in the analysis. Given that the literature offers no clear-cut

[4] For programme participants, their individual start of the programme is the start of the measurement of the dependent variables. For non-participants, there is no actual start of non-participation. Therefore, a hypothetical programme start has

criteria on which variables (not) to use, the selection of covariates is mainly based on the experience of previous ALMP evaluations as well as theoretical considerations. To begin with, I use information on sociodemographic characteristics and household composition, namely age, sex, education, family status, German/non-German citizenship, health, size of the household, number of own children and total dependent children in the household, as well as the number of adult and unemployed persons. Moreover, I include information on lone parenthood. In addition to this standard information, the dataset contains two additional groups of variables which are particularly valuable for the matching analysis. First, several special characteristics are recorded by the employment agencies. These include the subjective assessment of future employment prospects by the counsellors from the employment agency, the reason for the end of the last spell of social assistance receipt, and whether someone has ever dropped out of an ALMP programme due to inappropriate behaviour or has finished an ALMP programme unsuccessfully. The latter two variables are of particular interest because they can be seen as proxies for usually unobservable variables such as motivation or behavioural characteristics. Second, I can rely on very detailed information on past employment history. This includes some characteristics of the last job, namely whether someone has worked as white-collar or blue-collar worker, the degree of complexity and the industrial sector. Moreover, I have extracted information on all spells of regular employment, subsidized employment, unemployment, and programme participation. I have calculated the number of months in the respective employment status within the first, second to fourth and fifth to seventh year before the official programme start. Additionally, I include a 0/1 indicator which states whether someone has ever been regularly employed during the last seven years. I refrain from the approach of Biewen et al. (2014), who match exactly on employment sequences of binary variables which indicate whether someone has been

been defined. It is equal to the start date of the programme plus a random variable which mirrors the temporal pattern of the inflow into the programme of participants between July 2010 and June 2011.

employed in a certain year, because the variance within these sequences is too low for my sample. Finally, I include some additional regional information from another administrative source on the local labour market situation at the job centre level. Since treatment and control observations both come from participating job centres, differences in regional variables only result from different distributions of participants and non-participants between job centres and are therefore rather small. Hence, I limit myself to the regional employment and unemployment rate as well as GDP per capita. All ordinal variables are split into dummy variables to avoid functional form misspecification in the propensity-score estimation.

Table A.1 gives the number of observations and the mean for both dependent variables and each covariate, separately for treatment and control observations. Checking for ex-ante covariate differences is crucial for the matching analysis, because strong differences may result in thin common support. This implies that (depending on the matching algorithm applied) either many observations will be discarded from the analysis, or few observations receive very high weights and dominate the estimator (Imbens, 2015). Therefore, it contributes to the reliability and robustness of the analysis that covariate differences are very limited (restricting the maximum weight that is given to one observation does not change the results here, see section 3.5). Moreover, it is worth mentioning that existing differences do not point to strong and systematic positive or negative selection, even though there is a certain tendency for positive selection. On the one hand, participants are slightly higher educated and their labour market history is somewhat more favourable. On the other hand, the subjective assessment is worse and the incidence of lone parents is higher. Together with the high number of observations that is available, this creates very favourable conditions for the matching analysis. The raw data for wages and employment probabilities show limited differences, too, whereas participants have slightly lower integration rates, but higher wages.

However, this difference cannot be interpreted as a causal effect. The picture may change if observable characteristics are conditioned on.

3.3 Identification Strategy

Following the potential outcome framework (Rosenbaum and Rubin, 1983), the treatment effect on the treated is equal to the outcome they have realized by participating in the programme, and the one they would have realized without participation. Given that the latter cannot be observed, it has to be estimated using a control group. Therefore, the treatment effect estimation is based on the comparison of participating and control observations. To begin with, I follow the most common approach in programme evaluation and apply a matching analysis. I use all variables described in section 3.2 as covariates. To decide about the details of the matching analysis, I rely on recent insights from the microeconometric treatment effect estimation literature on the finite sample properties and performance of different algorithms and approaches (for recent and sophisticated examples see Iacus, King and Porro, 2011; Abadie and Imbens, 2006, 2011; Hainmueller, 2011; Huber, Lechner and Steinmayr, 2014). Based on Monte-Carlo-Simulations, these and other studies have created some guidance on the performance of the estimators. No estimator dominates all the other ones, but there seem to be a couple of reasonable approaches. I decide to start with radius matching with regression adjustment as suggested by Huber, Lechner and Steinmayr (2014). I follow some of the advice from their companion paper (Huber, Lechner and Wunsch, 2013) when selecting tuning parameters. Therefore, I start with radius matching with linear bias correction, and the radius is defined as three times the maximum distance in propensity scores that would have been reached with one-to-one-matching. In the initial estimation, I do not restrict the maximum weight that is given to one particular observation, because I am confident that the favourable conditions will not lead to high weights for some observations, anyway. As expected due to the limited differences in pre-matched covariates, the

standardized bias after matching is very low, with only one covariate showing a standardized bias of slightly more than five percent (see graph A.2). Nevertheless, these still somewhat arbitrary decisions should be subject to robustness checks. To begin with, I restrict the maximum weight that is given to one observation to four percent of total weights. This trimming procedure performs best in the Monte-Carlo Simulations of Huber , Lechner and Wunsch (2013). Afterwards, I replace radius matching with regression adjusted mahalanobis matching as outlined by Abadie and Imbens (2006; 2011). Finally, I use inverse probability weighting with regression adjustment as alternative approach.

Even though the results will show that the matching analyses are very robust to changes in the algorithms and tuning parameters, there may be concerns about systematic bias due to violations of the identifying assumptions. The estimates can only be interpreted causally if the conditional independence assumption (CIA) and stable unit treatment value assumption (SUTVA) hold (see e.g. Keele, 2015). The conditional independence assumption states that potential outcomes are (conditional on observable variables) independent of treatment status. This means that there must be no unobserved differences between treatment and control group left, which affects both the outcome and treatment assignment probability. In the given context, there is no clear-cut reason which points to endogenous selection. The treatment group is rather broadly defined and observable covariates do not point to strong selectivity. Moreover, the credibility of the CIA is enhanced by the exceptionally good quality of the data. Even though potentially relevant variables such as career preferences or motivation are not observable, it seems reasonable to argue that these have been absorbed by past employment outcomes or the information on behaviour in previous ALMP programmes. Nevertheless, I conduct a placebo-test on endogenous selection (Heckman and Hotz, 1989; Imbens, 2015; Imbens and Rubin, 2015) to further substantiate the credibility of the CIA. This test is based on a matching estimation, in which a variable that is connected to the actual

outcome variable, but unaffected by the treatment, is defined as the (placebo-) outcome. If the matching analysis reveals a significant effect on this pseudo-outcome, endogenous selection is likely to be present. In ALMP evaluations, past employment outcomes are natural candidates for the placebo-outcomes. Since I have used information on employment biographies of the past seven years (which should then obviously not be used as placebos), I define the number of months in employment eight and nine years before the start of the programme as placebo-outcomes. For sake of robustness, the placebo-test is conducted twice, once with radius and once with mahalanobis matching.

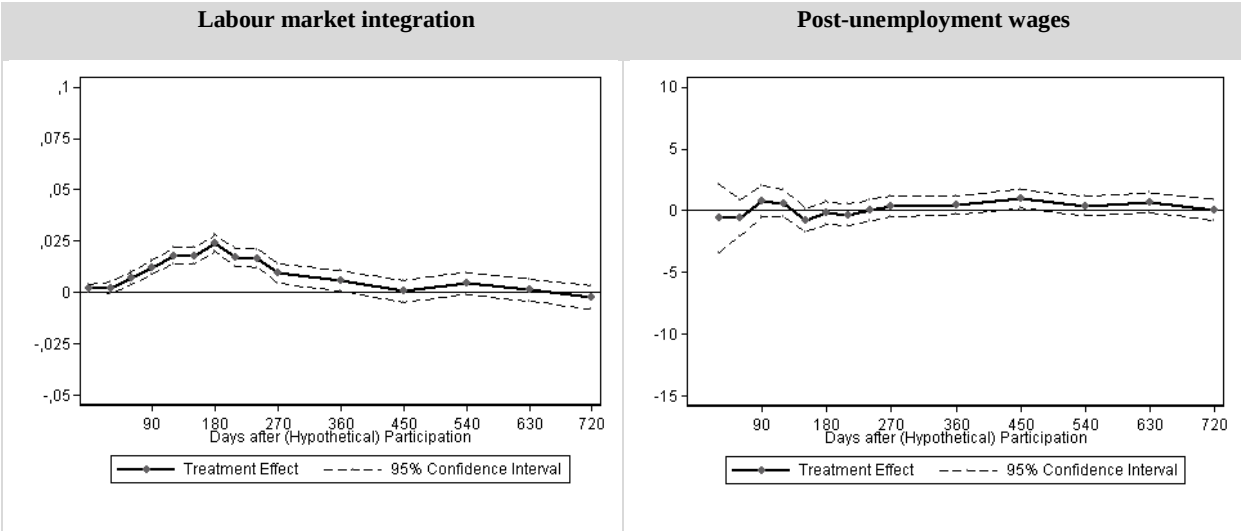
Finally, whether the stable unit treatment value assumption (SUTVA) holds is rather ambiguous from a theoretical point of view. As in any other ALMP programme, it is possible that treated workers simply substitute untreated ones, e.g. because they are better equipped for job interviews (Imbens and Wooldridge, 2009). Moreover, the execution of the programme might lead to a redistribution of resources to the disadvantage of untreated workers in the same jobcentre, because participating job centres do not receive additional funding for the programme. Due to budget constraints, this is likely to result in reduction of time and effort spent for non-participants, which could worsen their employment prospects. Taken together, both factors may lead to negative effects on non-participants, which would bias the matching estimation upwards. Such interferences between units have recently been a very active field of research in almost all disciplines that apply statistical methods. The gold standard for their estimation that has recently occurred is what Sinclair, McConnell and Green (2012) refer to as multilevel experiments. In these multilevel-experiments, there are treated and untreated clusters (e.g. regions), and treated and untreated observations within treated clusters. Interference is then estimated by (regression-adjusted) difference-in-means comparisons between untreated observations from treated cluster, and untreated observations from untreated cluster (for applications from different theoretical contexts see Nickerson (2008),

Ichino and Schündeln (2012), VanderWeele, Tchetgen and Halloran (2012), or Crépon et al. (2013)). A similar idea will be applied here: I observe non-participants from participating as well as non-participating job centres. The test for interference between units is therefore based on the comparison of the outcomes of these two groups. However, unlike in the aforementioned applications, it has to be considered that regional participation is not randomized. Therefore, raw differences in employment outcomes may also stem from regional selection bias, i.e. differences in regional labour market conditions or in the socio-demographic composition of unemployed worker. Due to the comparatively low number of observations at the upper level (the jobcentres), differences in regional characteristics will be difficult to balance in a matching analysis. Therefore, I combine matching with difference-in-differences estimation, i.e. DiD-estimation in a matched sample (semi-parametric DiD, Abadie, 2005). To this end, I rely on the two samples of workers who have become unemployed between one and two years before the start of the programme (i.e. between July 2008 and June 2009). Untreated workers from participating job centres are defined as (pseudo-) treatment units, non-participants from non-participating job centres function as control units. One point in time within the period before the start of the programme (i.e. July 2008 to June 2010) is defined as t_0, whereas one point in time after the start of the programme (i.e. between July 2010 and June 2012) is defined as t_1. The time difference between both points in time is two years (e.g. January 2010 and January 2012). For sake of robustness, I conduct this analysis twice for different points in time and again once with mahalanobis and once with radius matching.

3.4 Results and Discussion

The treatment effect estimations are summarized in graph 1. The left panel shows the results for integration into regular employment, the right panel the ones for wages at different points in time after programme start. The point estimates represent absolute (regression-adjusted)

differences between treatment and matched control group in the incidence of employed workers and daily wages, respectively. The effects on labour market integration refer to net integration rates, i.e. the corresponding outcome variable is coded 0 if someone is not employed at the respective point in time, regardless whether they have been in and out again of employment after programme start.



Graph 1. Treatment effects based on radius matching with regression adjustment. Thin lines represent confidence intervals, thick lines are the point estimates. The left graph shows treatment effects with regard to labour market reintegration in percentage points, the right graph shows treatment effects on absolute daily wages in €.. *Source*: own calculations based on IEB.

The effect on programme participation is positive from the very beginning and starts to accelerate towards the (scheduled) end of the counselling and monitoring scheme. It reaches up to 2.4 percentage points. The corresponding estimated potential outcome means of the treatment and control group are 9.4 and 7.0 percent which translates into a relative effect of 35 percent. The treatment effect goes down after 180 days and reaches zero towards the end of the observation period. As stated in section 3.1, the effect after 180 days is (partly) a combined effect of *Activating Citizen's* and the subsequent public employment scheme (PES). Given that this PES displays remarkably negative employment effects (IAW, ISG, 2015), the effect of the counselling and monitoring scheme can be assumed to be positive in the long-run. In any case, the effect on cumulated time in employment is positive at the end of the observation period even for the combined effect, because the displayed effects are net

integration rates. It can be concluded that the programme has fulfilled its purpose of fostering labour market integration of participants. Comparing these results with the ones reported from previous research, this appears to be a rather strong effect, but it is still close to e.g. the ones reported by Graversen and Van Ours (2008). The fact that the positive effect appears to be somewhat stronger than the one of many other programmes may be because this programme itself is a rather effective one, or because the circumstances have been rather favourable. For example, the labour market conditions in Germany were quite good at that time. It could be argued that the effect of supply-side programmes would have been weaker in times with high structural unemployment, when vacancies are simply not there, which cannot be solved by counselling services. However, the meta-analysis of Kluve (2010) suggests that it is programme effectiveness as such rather than the circumstances that matter.

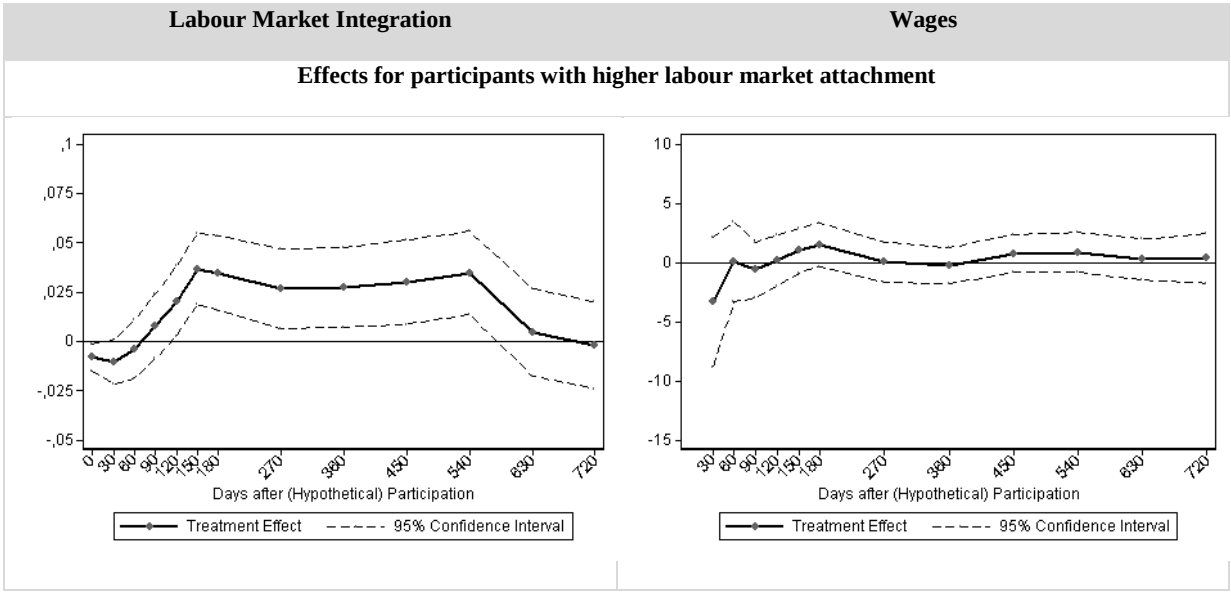
The decisive question now is whether this acceleration of labour market integration is paid for with worse job quality. The results do clearly not support this argument. The estimated effects on wages of those who found employment are almost zero and clearly insignificant for all points in time. Even though the number of observations is lower than the one in the estimations for labour market integration, this does clearly not reflect a lack of statistical power as indicated by the very narrow confidence intervals (e.g. after 180 days, the number of integrated workers for whom reliable wage information is available still amounts to 7,149 observations). Given that the average daily wages amount to about 36€ (once again depending on the point in time; note that not all persons are full-time employed and these are wages per calendar day, not working day), even the upper or lower bound of the confidence interval would translate into negligibly small relative effects. The conclusion that there is no adverse effect on job quality is further substantiated by the argument outlined by Bennmarker, Nordström Skans and Vikman (2013), who point out that in case of a positive effect on labour market integration, the estimates for wages of those who found employment represent a lower

bound. The underlying assumption of this argument is that even if the CIA holds, we would expect that there are (possibly unobservable) differences within treatment and control group with regard to labour market attachment. Moreover, we would expect that persons with more favourable characteristics are integrated first. Therefore, a higher share of integrated workers within the treatment group implies that more persons with less favourable unobservable characteristics are included in the wage effect estimations than in the control group. This may result (if anything) in negatively biased results. Even though this possible bias is likely to be small in presence of high-quality data, this further supports the conclusion that there is no adverse effect on job quality.

3.5 Effect Heterogeneity and Robustness and Specification Analyses

Even though the quality-quantity trade-off was expected to be weaker, it is important to check the robustness and reliability of these somewhat surprising results both in methodological as well as substantive terms. From a substantive point of view, it may be argued that the absence of a quality-quantity trade-off is due to the target group. Even though it has been rather broadly defined, the descriptive statistics show that participants are characterized by rather low labour market attachment. In fact, almost half of the participants (and the control group) have not been regularly employed for the last seven years. It seems reasonable to argue that the outlined arguments that contribute to a quality-quantity trade-off rather apply for workers with higher labour market attachment, because after periods of unemployment of many years, it is questionable whether more time for job-search is still beneficial. Therefore, I repeat the analysis but limit the sample to participants who have been employed in at least 12 months within the last four years. The results of this estimation are summarized in graph 2. They reveal that the effect is not different for this subsample. Compared to the whole sample, the effect on labour market integration is somewhat weaker in the beginning but stronger at later points in time. The only remarkable difference is that absolute integration rates (not shown)

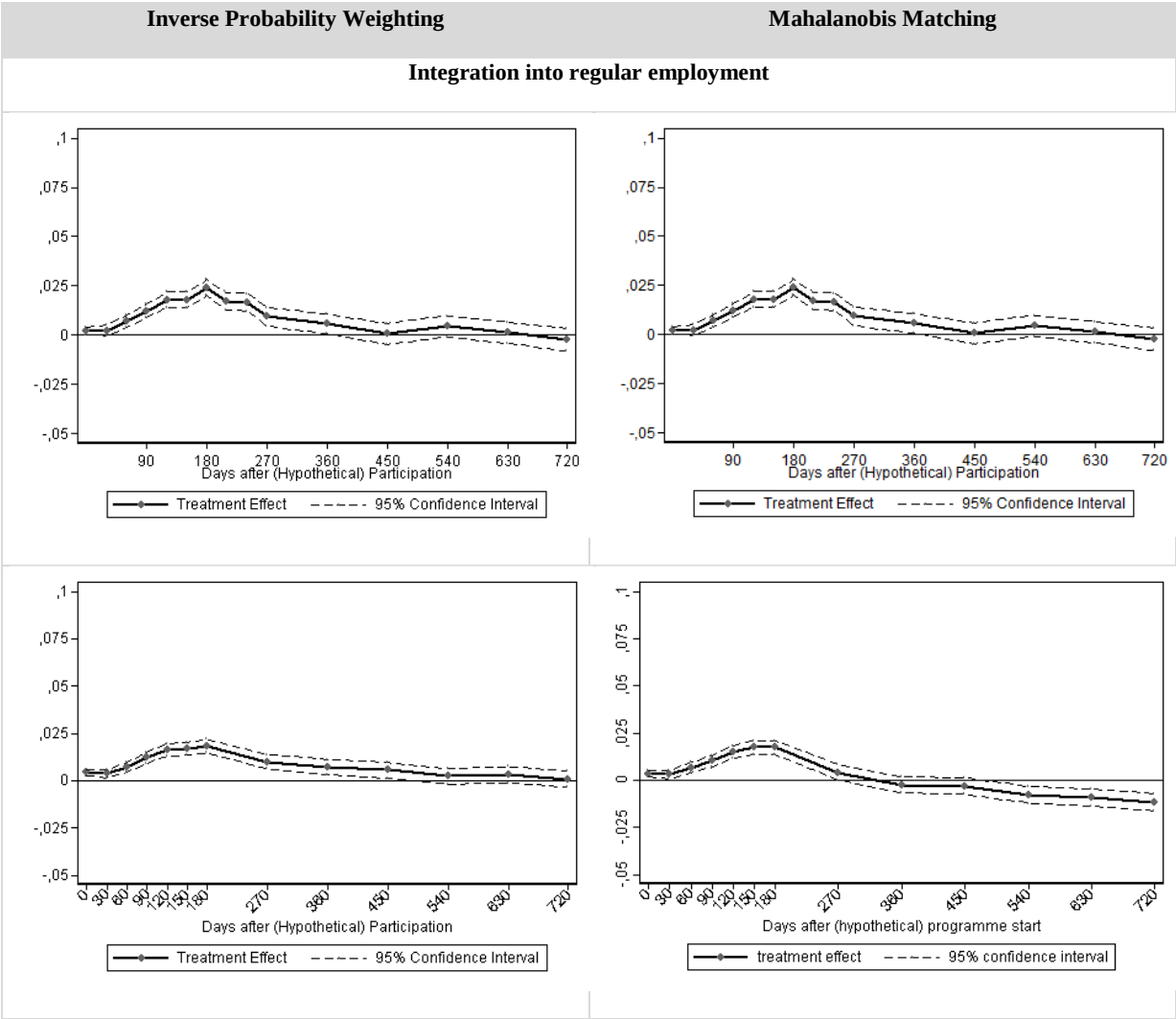
are higher in both groups (21.0 and 17.5 percent after 180 days), which is clearly consistent with the theoretical expectations. The effect on wages is again close to zero and insignificant. Given that the outlined distinction is somewhat arbitrary, I have also tried other sub-sample constructions such as the restriction to persons who have ever been employed within the last seven years (not shown, available upon request), but the results again rarely change. It is worth mentioning that the same holds true for other sociodemographic characteristics which are typical suspects for effect heterogeneity, namely age, gender, or region of residence (East vs. West Germany). Apparently, the programme effect does not vary systematically with indicators of employment history or other sociodemographic characteristics.



Graph 2. Treatment Effects on workers who have been employed in at least 12 months within the last four years. *Source:* Own calculations based on IEB.

From a methodological point of view, it still has to be investigated whether the results are robust to methodological choices by the researcher, or whether all these matching estimations are systematically biased by endogenous selection or substitution effects. To test the robustness of the results, I have conducted the analysis with alternative estimation approaches as outlined in section 3.3. Graph 3 summarizes these results. The upper left panel shows the results from the original analysis again (to allow for comparisons via a quick glance). The upper right corner replicates this analysis with the restriction on the maximum weight given to

one observation. The lower part of the graph shows the results for inverse probability weighting (left) and mahalanobis matching (right) with regression adjustment. Once again, differences in the estimated effects are very limited indicating that the results are not sensitive to methodological choices. Finally, the specification analyses neither point to endogenous selection nor substitution effects. As graph A.2 shows, the estimated effects on both placebo-outcomes (months in employment eight and nine years before programme start) is close to zero and insignificant for both matching algorithms. It is worth mentioning that this is a remarkable finding given the high statistical power (exceptionally high number of observations) of this test. This implies that there is no indication for endogenous selection which confirms the claim that conditional independence is a reasonable assumption in presence of such high quality data. Similarly, the results shown in graph A.3 refute concerns about substitution effects. It displays estimated substitution effects for different points in time after programme start (e.g. the coefficient at 90 days after programme start represents the effect when 90 days after programme start is t_1, and the point in time two years before that is t_0). For both estimations, the results are almost exactly zero for all points in time during the counselling and monitoring scheme. They get marginally significant towards the end of the observation period for mahalanobis matching but are very small in magnitude. In any case, this (if any) very small degree of interference does in no way affect the results from the matching analysis in substantive terms.



Graph 3. Treatment Effects based on different matching/weighting estimations with regression adjustment. The upper left panel show the results from the original analysis again (as reference category). The upper right corner replicates this analysis with the matching procedure outlined in section 3.3. The lower part of the graph shows the results for inverse probability weighting (left) and mahalanobis matching (right) with regression adjustment. The Source: own calculations based on IEB.

4 Summary and Conclusion

This paper was motivated by the question of whether activation policies face a quality-quantity trade-off in their effect on employment outcomes of targeted workers. It has been suspected that quicker, activation-induced labour market integration is paid for with worse job quality. Despite a huge and growing body of policy and programme evaluations in this field, the effect of activation policies on job quality has been considered only recently and remains an important gap in the literature. The few existing studies have concentrated on sanctions, and – in line with the concerns outlined in the public and social policy literature – revealed

remarkably negative effects on job quality. However, it still is an open question whether the quality-quantity trade-off is also present for counselling and monitoring schemes (or other activation policies) which combine pressuring with supportive components. I have contributed to this discussion by analysing the effects of a counselling and monitoring scheme from Germany on labour market integration and post-unemployment wages. My results do not confirm the concern about a quality-quantity trade-off. The programme exerts a strongly positive effect on employment probability which reaches 35 percent towards the end of the (scheduled) programme duration. At the same time, there is no effect on wages of those who have been successfully integrated into the labour market. These findings are robust to methodological changes (namely different matching algorithms or trimming procedures) and do not vary systematically with sociodemographic characteristics such as age, gender, region of residence or employment history. Moreover, specification analyses refute concerns about biases in the matching estimations due to endogenous selection and/or substitution effects.

What are the implications of these results in the broader theoretical debate on activation policies? Taken together, the results outlined here and the ones concerning sanctions and public workfare employment reveal an interesting puzzle. On the one hand, the negative impact of sanctions on job quality confirms the concern that quicker, activation-induced labour market reintegration is paid for with worse job quality. On the other hand, the results outlined here and the ones presented by Bennmarker, Nordström Skans and Vikman (2013) on the effects of public workfare employment suggest that the negative impact on job quality can be avoided if the right balance between pressuring and supportive components can be found. This has two implications for future research and policy-making: First, it reveals that previous categorizations of activation policies have been too broad. Distinguishing between “emancipating” activation which focusses on supporting unemployed workers (e.g. via long training programmes) and “repressive” (Vlandas, 2013, p. 5) activation that forces them into

the labour market by all means ignores the diversity of activation programmes within these two categories. Therefore, it should be an ongoing task for future research to develop more fine-grained typologies of activation policies. Bonoli (2010) has made a first promising step into this direction. Second and relatedly, the question of which components or combinations of activation policies exactly may contribute to quicker labour market integration without hurting job quality needs further exploration. By analysing the effect of different activation policies on quantity and quality of labour market integration, empirical research can constitute the basis for well-informed public policy-making that succeeds to reduce unemployment at the same time circumventing the danger of pushing unemployed worker into a low-wage trap. Only by relying on such an empirical basis, policy-makers cannot only decide about which policies (not) to implement, but continuously improve these policies (Besharov, 2009). In this regard, a lot of work in this area remains to be done.

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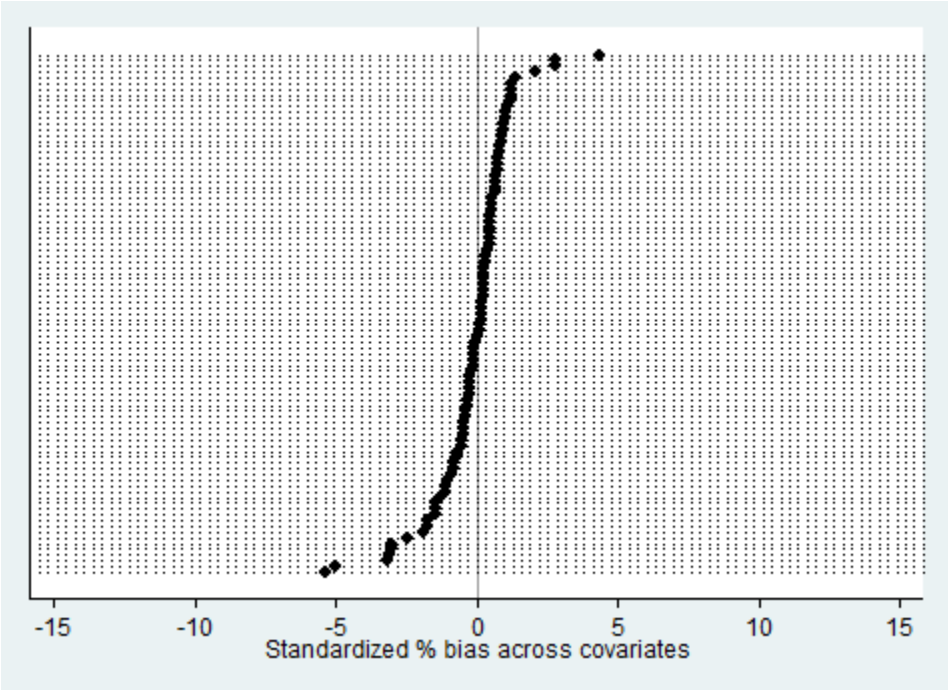
Appendix

Variable	Participants		Non-Participants	
	N	Mean	N	Mean
Sociodemographics				
Employment probability 180 days after programme start	63878	0.10	103641	0.11
Daily wage 180 days after programme start (if employed)	2332	38.23	4817	36.94
Gender: female	63878	0.45	103641	0.46
No graduate	63877	0.10	103617	0.14
Intermediate school certificate, no professional qualification	63877	0.24	103617	0.29
High school degree or vocational training	63877	0.58	103617	0.47
High school degree and vocational training	63877	0.04	103617	0.05
Subject-linked university degree	63877	0.02	103617	0.02
University degree	63877	0.02	103617	0.03
Age group < 25 years	63878	0.04	103641	0.14
Age group 25 until 30 years	63878	0.13	103641	0.14
Age group 30 until 35 years	63878	0.13	103641	0.13
Age group 35 until 40 years	63878	0.14	103641	0.11
Age group 40 until 45 years	63878	0.18	103641	0.12
Age group 45 until 50 years	63878	0.20	103641	0.13
Age group 50 until 58 years	63878	0.15	103641	0.18
Age group > 58 years	63878	0.03	103641	0.05
Family Status: living alone	63878	0.45	103641	0.45
Family Status: married/living with a partner	63878	0.33	103641	0.34
Family Status: divorced/widowed/living separately	63878	0.23	103641	0.21
Family Status: missing	63878	0.00	103641	0.00
Citizenship: German	63878	0.90	103641	0.81
Citizenship: other than German	63878	0.10	103641	0.19
Citizenship: missing	63878	0.00	103641	0.00
Health problems: yes	63870	0.25	103520	0.22
Household information				
Child < 3 years: no	63878	0.58	103641	0.50
Child < 3 years: yes	63878	0.03	103641	0.04
Child < 3 years: missing	63878	0.39	103641	0.46
Child between 3 and 6 years: no	63878	0.58	103641	0.50
Child between 3 and 6 years: yes	63878	0.04	103641	0.04
Child between 3 and 6 years: missing	63878	0.39	103641	0.46
Child between 6 and 10 years: no	63878	0.54	103641	0.47
Child between 6 and 10 years: yes	63878	0.08	103641	0.07
Child between 6 and 10 years: missing	63878	0.39	103641	0.46
Child between 10 and 15 years: no	63878	0.52	103641	0.46
Child between 10 and 15 years: yes	63878	0.09	103641	0.08
Child between 10 and 15 years: missing	63878	0.39	103641	0.46
Role within household: Main person	63878	0.86	103641	0.79
Role of within household: partner	63878	0.13	103641	0.15

Role of within household: minor, unmarried child / unmarried adult	63878	0.01	103641	0.06
Role of within household: missing	63878	0.00	103641	0.00
Number of persons in BG: 1	63878	0.48	103641	0.46
Number of persons in BG: 2	63878	0.23	103641	0.23
Number of persons in BG: 3	63878	0.14	103641	0.15
Number of persons in BG: 4	63878	0.09	103641	0.10
Number of persons in BG: 5 or more	63878	0.06	103641	0.07
Number of persons in BG: missing	63878	0.00	103641	0.00
Number of persons of age able to work: 1	63878	0.62	103641	0.58
Number of persons of age able to work: 2	63878	0.31	103641	0.34
Number of persons of age able to work: > 2	63878	0.06	103641	0.08
Number of persons of age able to work: missing	63878	0.00	103641	0.00
Number of persons under age able to work: 0	63878	0.91	103641	0.91
Number of persons under age able to work: 1	63878	0.08	103641	0.08
Number of persons under age able to work: more than 1	63878	0.01	103641	0.01
Number of persons under age able to work: missing	63878	0.00	103641	0.00
Number of unemployed persons: 0	63878	0.98	103641	0.97
Number of unemployed persons: 1	63878	0.02	103641	0.03
Number of unemployed persons: > 1	63878	0.00	103641	0.00
Number of persons above age limit: 0	63878	1.00	103641	0.99
Number of persons above age limit: 1 or more	63878	0.00	103641	0.01
Number of persons above age limit: missing	63878	0.00	103641	0.00
Lone parent: no	63878	0.83	103641	0.86
Lone parent: yes	63878	0.17	103641	0.14
Additional administrative information				
Profile: Integrated	63878	0.07	103641	0.07
Profile: Market, activation, promotion	63878	0.16	103641	0.22
Profile: About to develop	63878	0.35	103641	0.26
Profile: About to be stable	63878	0.18	103641	0.14
Profile: Support necessary	63878	0.15	103641	0.15
Profile: missing	63878	0.09	103641	0.14
Job returner: no	63878	0.95	103641	0.96
Job returner: yes	63878	0.05	103641	0.03
Job returner: missing	63878	0.00	103641	0.01
Responsible administrative body: ARGE/gE [s.o., green area]	63574	0.93	103423	0.93
Responsible administrative body: gT/gAw	63574	0.07	103423	0.07
Responsible administrative body: zkT	63574	0.00	103423	0.00
Reason for end of receiving social assistance benefits: start of work	63878	0.07	103641	0.07
Reason for end of receiving social assistance benefits: relocation	63878	0.11	103641	0.13
Reason for end of receiving social assistance benefits: omission of employment	63878	0.13	103641	0.12
Reason for end of receiving social assistance benefits: other reasons	63878	0.16	103641	0.18
Reason for end of receiving social assistance benefits: missing	63878	0.53	103641	0.50
Special status	63878	0.15	103641	0.25
Relieved receiving of benefit: children	63878	0.02	103641	0.02

Relieved receiving of benefit: job returner	63878	0.05	103641	0.02
Relieved receiving of benefit: none	63878	0.85	103641	0.75
Relieved receiving of benefit: missing	63878	0.09	103641	0.21
Dropout of measure due to inappropriate behaviour	63878	0.03	103641	0.02
Dropout of measure due to other reasons	63878	0.04	103641	0.03
Measure not completed successfully	63878	0.05	103641	0.03
Employment History				
Information on last job				
Blue-collar worker	63878	0.11	103641	0.12
White-collar Worker	63878	0.03	103641	0.03
profession: missing	63878	0.86	103641	0.85
Semi-skilled worker	63878	0.14	103641	0.16
Professionally oriented activities	63878	0.69	103641	0.64
Complex specialized activities	63878	0.07	103641	0.07
Highly complex activities	63878	0.08	103641	0.08
Complexity: missing	63878	0.02	103641	0.05
Manufacturing/ processing trade / agriculture	63878	0.41	103641	0.36
Service sector or others	63878	0.59	103641	0.64
Indicators of past employment history				
Number of months employed: 1 years before 2010	63878	0.36	103641	0.62
Number of months employed: 2-4 years before 2010	63878	2.53	103641	3.55
Number of months employed: 5-7 years before 2010	63878	4.13	103641	4.70
Number of months unemployed: 1 years before 2010	63878	9.47	103641	8.65
Number of months unemployed: 2-4 years before 2010	63878	25.34	103641	21.37
Number of months unemployed: 5-7 years before 2010	63878	14.93	103641	11.88
Number of months seeking work: 1 years before 2010	63878	0.83	103641	0.49
Number of months seeking work: 2-4 years before 2010	63878	1.86	103641	1.20
Number of months seeking work: 5-7 years before 2010	63878	1.28	103641	0.92
Number of months program: 1 years before 2010	63878	0.53	103641	0.66
Number of months program: 2-4 years before 2010	63878	1.87	103641	2.28
Number of months program: 5-7 years before 2010	63878	6.80	103641	4.76
Employed at all in the last 7 years before 2010	63878	0.44	103641	0.45
Regional information				
Regional unemployment rate (level of job centres)	62722	10.05	102493	9.86
Regional employment rate (level of job centres)	62722	50.32	102493	49.53
GDP per capita of employed person (level of job centres)	63149	54149.53	101594	59327.68

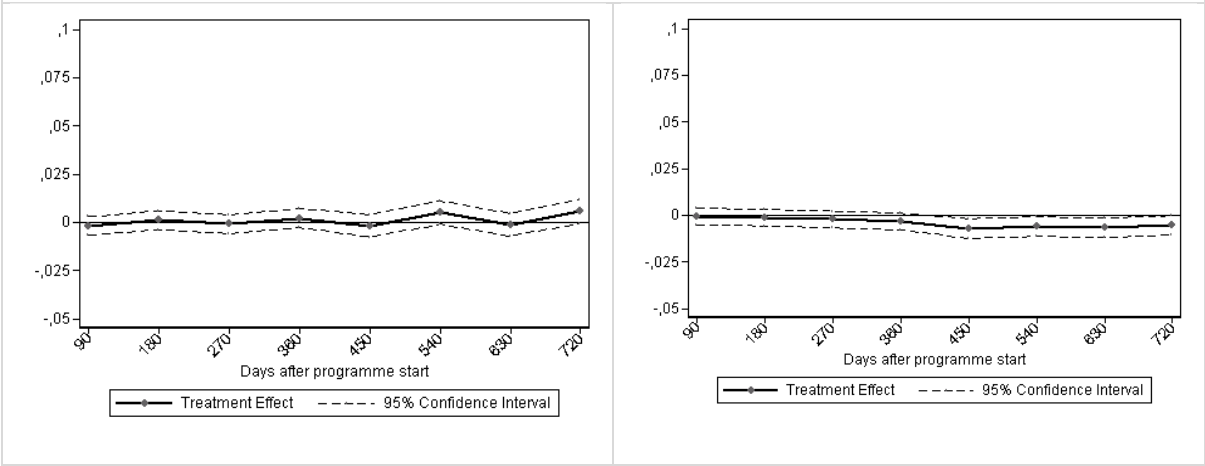
Table A.1. Summary of descriptive statistics for participants and non-participants. *Source:* own calculations



Graph A.1. Standardized bias of the radius-matching estimation, 180 days after programme start. *Source:* own calculations, based on IEB.

Graph A.2. Estimated (pseudo-) treatment effects based radius-matching (left) and mahalanobis-matching (right) with regression adjustment. *Source:* own calculations based on IEB.

Radius Matching	Mahalanobis Matching
Test for Substitution Effects	



Graph A.3. Estimated substitution effects based on semi-parametric difference-in-differences estimation with radius-matching (left) and mahalanobis-matching (right). *Source:* own calculations based on IEB. The points in time refer to the definition of the point t_1, t_0 is the point in time two years ago.

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