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Working Paper

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DIW Discussion Papers, No. 744

Provided in Cooperation with:
German Institute for Economic Research (DIW Berlin)

Suggested Citation: Freier, Ronny; Steiner, Viktor (2007) : Marginal employment: Stepping stone or dead end? Evaluating the German experience, DIW Discussion Papers, No. 744, Deutsches Institut für Wirtschaftsforschung (DIW), Berlin

This Version is available at:
https://hdl.handle.net/10419/27268

DIW Berlin

German Institute
for Economic Research

Discussion Papers

744

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Berlin, November 2007

Opinions expressed in this paper are those of the author and do not necessarily reflect views of the institute.

IMPRESSUM

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http://www.diw.de

ISSN print edition 1433-0210
ISSN electronic edition 1619-4535

Available for free downloading from the DIW Berlin website.

'Marginal Employment': Stepping Stone or Dead End?

Evaluating the German Experience

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This version: November 9, 2007

Abstract: 'Marginal employment', i.e. employment at low working hours and earnings not covered by social security, has been gaining importance in the German economy over the past decade. Using a large newly available panel data set and statistical matching techniques, we analyse the effects of marginal employment on future individual outcome variables such as unemployment, regular employment and earnings. In addition to average treatment effects, we calculate dynamic and cumulative treatment effects accounting for total time spent in various labor market states and related earnings over a period of three years. We find that marginal employment (i) does not affect time spent in regular employment within a three-years' observation period, (ii) reduces future unemployment, (iii) slightly increases cumulated future earnings, on average, and (iv) is associated with a small negative cumulative earnings effect for older workers in west Germany.

Keywords: Marginal employment, social security contributions, wage subsidies, labour market policy, evaluation of treatment effects
JEL classification: J23, J64, H43, C35

Acknowlegdement: We thank Florian Zinsmeister, Johannes Ziemendorff, Marco Caliendo, Kai-Uwe Müller and seminar participants at Stockholm School of Economics, the IAB Nuremberg, and the Free University Berlin for helpful comments on an earlier version of this paper. We would also like to thank the German Science Foundation (DFG) for funding under the project 'Work Incentives, Earnings-Related Subsidies, and Employment in Low-Wage Labour Markets' (Ste 681/5-1). The usual disclaimer applies.

1 Introduction

'Marginal employment' (*'Geringfügige Beschäftigung'*), i.e. employment at low working hours and earnings not or only partially subject to social security contributions, has been gaining importance in the German economy over the past decade. There are two opposing views on this development among economists and policy makers. On the one hand, the existence of marginal employment (ME) has been seen as a means to improve labour market flexibility, to shore up financial incentives to take up low-paying work and to reduce labour costs for firms, thereby increasing the demand for low-productivity workers. This view seems to underlie recent development in 'active' labour market policy in Germany, and elsewhere (see, e.g., Fertig and Kluve, 2006; Steiner, 2006). On the other hand, critics are skeptical about the potential of marginal employment to enhance job creation and stress the danger of substitution of regular full-time jobs by subsidized ME (see, e.g., Schöb and Weimann, 2004: 115-122; Bofinger et al. 2006).

Studying the labour market impact of ME may therefore shed light on the issue of labor market flexibilty and is also of substantial policy interest. For various reasons, the German case is particularly interesting: Firstly, while social security contributions weigh relatively heavy on low-productivity jobs, ME is partly exempted from this burden in Germany. Secondly, ME has substantially grown in Germany over the last couple of years, while overall employment stagnated in this period. Thirdly, marginal employment has gained considerable importance in German labour market policy. While a reform in 1999 tried to curb the expansion of ME, the so-called 'Mini Jobs' reform in 2003 was implemented with the aim of increasing work incentives in the low-wage sector of the economy (see, e.g., Steiner and Wrohlich, 2005). Recently, the employers contribution rate on these jobs was increased from 25% to 30.1% to again curb the alleged substitution of full-time jobs by publicly subsidised ME.

Despite its increasing quantitative importance and policy relevance, there has been relatively little empirical research on the labour market effects of ME for Germany. There are some studies describing the recent evolution and structure of ME in the German labour market (see, e.g., Schupp and Birkner, 2004; Fertig and Kluve, 2006). Also, the labour supply effects of the mentioned 'Mini Jobs' reform have been analysed on the basis of ex-ante simulation studies (Arntz et al, 2003; Steiner and Wrohlich, 2005) as well as ex-post evaluations (Caliendo and Wrohlich, 2006). These studies found only very small labour supply effects of the reform, especially among the target group of long-term unemployed people. However, to the best of our knowledge there has hitherto been no empirical investigation on whether ME acts

as a 'stepping stone' to regular employment or rather leads to 'dead end' jobs. In a recent study for Austria, Böheim and Weber (2006) find that unemployed people who take up ME end up with less regular employment, more unemployment and lower wages after three years than the control group of unemployed who do not enter ME. These results seem to support the 'dead end' view and the critics of publicly subsidised ME.[1]

This paper provides an empirical analysis of the employment and earnings effects of ME for Germany. The empirical analysis is based on a statistical matching approach making use of register data from the Employment Statistics of the Federal Labor Agency. We restrict the analysis to men because including women would require to analyze the interrelations between ME and (conventional) part-time employment. As shown by Freier and Steiner (2007), these two employment types are substitutes in production, especially for women whose part-time share amounts to more than a third in total employment in Germany. Furthermore, labour supply of married women would require to account for household characteristics, such as number and age of children, earnings of the spouse, and other household income, on which we do not have information in our data. The analysis distinguishes between east and west Germany because of prevailing pronounced differences in unemployment between the two regions. We would expect ME to be more effective as a stepping stone into regular employment in labour markets with lower unemployment as it is the case, on average, in west Germany. Furthermore, we also present results for older men, because, for institutional reasons, we expect that effects of ME on future employment and earnings may differ by age and because older unemployed people have recently become a special target group for labour market policy (see Haan and Steiner, 2006).

In the next section, we provide some institutional background on ME in Germany. Our data and evaluation methodology are described in sections 3 and 4. Evaluation results, summarised in section 5 show that, although ME does not increase time spent in regular employment within a three-years' observation period, it reduces future unemployment and, on average, slightly increases cumulated future earnings. We also identify important age differences in these effects. Section 6 summarises the main results of the study and concludes.

[1] Whereas the 'stepping stone' hypothesis in relation to temporary jobs has been analysed frequently in the literature (for a recent survey, see Ichino et al., 2006), there are apparently no studies for other countries focusing on ME .

2 Institutional Background

Since the mid 1990's, ME has been increasing substantially in Germany. Depending on how ME is measured, estimates for recent years vary between 3 and 6 million people in ME, with a significant increase over time (see, e.g., Schupp et al., 1998; Rudolph, 1998; Schupp and Birkner, 2004, Ziemendorff, 2006). In particular, there is the important distinction between ME held as the only job or as a secondary job. On the basis of data from the German Socioeconomic Panel (SOEP), Schupp and Birkner (2004, Table 4) estimate the total number of jobs in ME ('minijobs') at about 5 million in 2003, of which 3.6 million were held as the only job, with an increase of about half a million since 2000, according to their estimates. Also using SOEP data, Rudolph (1998) documents a substantial increase in ME since the beginning of the 1990's.

This development was accompanied by several policy reforms aimed at either restricting or strengthening financial incentives for ME. Before 1999, jobs with an upper earnings threshold of 325 Euro per month and a maximum of 15 weekly working hours were exempted from social security contributions (SSC) on the side of the employee. The employer had to pay a 20 percent tax on gross wages. ME in a secondary job was treated equally with respect to SSC. Earnings from several ME jobs by a single person were added-up and the resulting sum was subject to SSC.

The political aim of the 1999 reform was to restrict the expansion of ME. Since then, the employer had to pay 22% SSC. Thus, little changed under this reform for the employers of individuals working in ME. The reform also did not improve financial incentives for those workers to expand hours of work and take up regular employment. Since SSC, and possibly also income taxes, had to be paid in full above the relatively low SSC threshold, the marginal tax rate on such jobs remained rather high. On the other hand, ME could be financially attractive for recipients of unemployment compensation up to an earnings threshold of 165 Euro, beyond which the withdrawal rate became 100%.

In contrast to the previous reform, the 'Mini-Jobs Reform' of 2003 was intended to improve incentives to take up ME. The restriction on maximum hours was abolished and the upper threshold of exempted earnings was raised to 400 Euro per month. Moreover, earnings between 401 and 800 Euro are now subject to a modified SSC scheme. Although this reform improved financial incentives to take up low-paying jobs for 'secondary workers' (housewives, students, pensioners), it hardly changed incentives for persons receiving unemployment or social assistance benefits because of the high withdrawal rates (for details see, e.g., Steiner and Wrohlich, 2005; Steiner,

2006). Motivated by the strong expansion of ME following the 2003 reform, the employers' SSC rate was increased from 25% of gross monthly earnings of 400 Euro to 30.1% in July 2006. For part-time jobs with earnings between 401 and 800 Euro, employers still pay the normal employers' SSC rate of currently about 20%.

3 Data and Evaluation Design

Given the focus of our paper, the following analysis is restricted to ME held as the only job and refers to men entering registered unemployment after the introduction of the 1999 reform and before the 2003 reform became effective. Thus, our sample excludes all employed persons with ME held as a secondary job as well as people out-of-the-labour force, such as pensioners and students. For the reasons mentioned in the Introduction, we restrict the analysis to men.

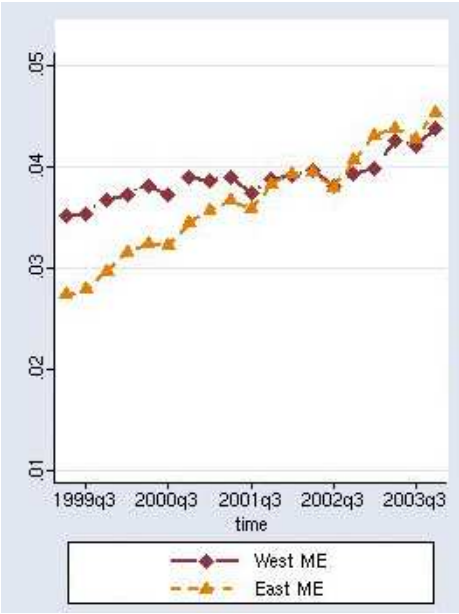
Data for our empirical analysis are derived from the Employment Panel of the Federal Employment Agency (EP-FEA), see Meinken and Koch (2005). The EP-FEA contains detailed quarterly information on employment and wages for a 2% random sub-sample of all employees subject to social insurance for the period 1998-2005, amounting to about 600,000 observations per quarter. Due to the fact that ME became subject to registration in 1999, the data include information on ME starting with the second quarter of that year. Spell information refer to a person's main labour force status. The evolution of marginal employment in east and west Germany within the observation period as derived from our EP-FEA data is shown in Figure (1).

The main strength of the EP-FEA data set is its large size and the correspondence of what is coded as ME in the data to the legal definition. We can, therefore, explicitly distinguish ME from other forms of employment, most importantly from conventional part-time employment.[2] Another main advantage is the high quality of employment spell (measured in days) and wage information in the EP-FEA due to the fact that this information is used for the calculation of individual entitlements under the public pension schemes.

There are also a few shortcomings of the EP-FEA data: Drawing on employment register data, unemployment was initially not registered in the EP-FEA data. In 2000, the FEA started to supplement the data with information from the unemployment statistics. However, unemployment remains incompletely coded in the data, as

[2]For data after the second quarter of 2003, the FEA also provides supplements to the main data which contain information on ME as a secondary job.

Figure 1: Marginal employed men as share of labour force by region, in quarters, 1999 - 2003



both unemployment not registered at the labour agency[3] and unemployment during a spell of ME is not coded. Moreover, there is no information on the amount of unemployment benefits received by a person in the data. Another disadvantage of the EP-FEA data is that the data do not contain information on household variables such as the employment status of the spouse, the presence of children and other household income. Since we restrict the analysis to men whose employment behavior is not expected to significantly depend on these variables, this is of little concern here.

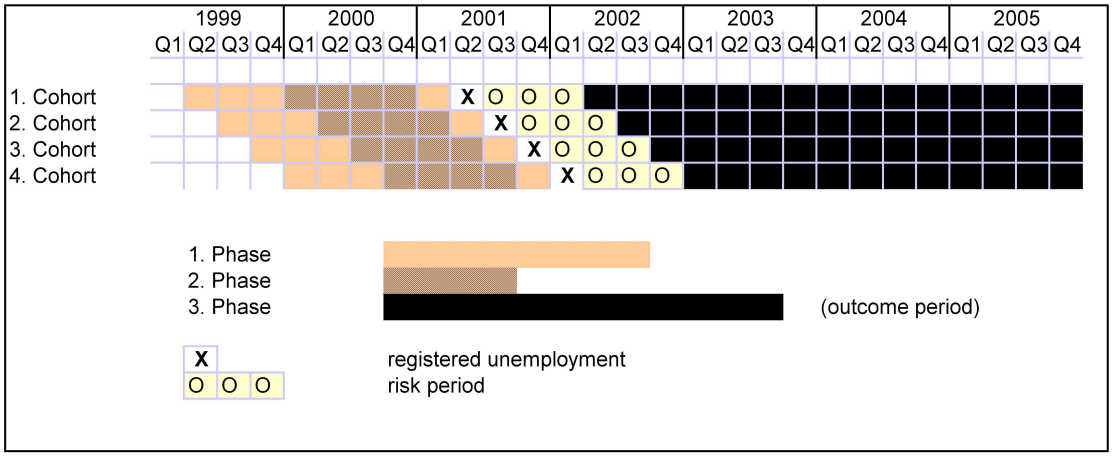
In order to evaluate the effects of ME on subsequent individual labour market outcomes, we have organized the EP-FEA data as illustrated in Figure (2). The analysis is based on a sample of four quarterly inflow cohorts of men who became unemployed for at least 3 months during the period from April 1, 2001 to March 31, 2002 and who were either regularly employed or in ME before. We distinguish between quarterly inflow cohorts to account for potential seasonal and business-cycle effects on individual labour market outcomes.[4] There are two reasons to include only those who have been unemployed for at least 3 months in the analysis: First, shorter spells of unemployment are not identified in our data. Second, even if they

[3]Note that, in Germany, a person is registered as unemployed although he is not entitled to unemployment benefits if he proves to the labour agency to be actively looking for a job.

[4]The German business cycle turned from a modest downturn into a mild recession during the inflow period.

were, we would prefer to restrict the analysis to longer unemployment spells because the analysis appears more relevant for longer-term unemployment than for purely frictional unemployment.

Figure 2: Evaluation design



For each person included in the sample, we define a 'risk period' of 9 month and determine whether the individual takes up ME within this period as the first professional position after terminating unemployment. Following terminology in the evaluation literature, we will denote these persons as belonging to the 'treatment group'. Individuals who remained unemployed or found positions in regular employment within the risk period will comprise the potential control group. Note that, contrary to what seems to be usual practice in the evaluation literature, we neither exclude individuals from the control group altogether nor treat them as right-censored at the time when they change treatment status. The reason for this is that we want to assess the future labour market performance of men who take up ME within a specific time period - the risk period defined above - compared to a control group of people who had the same ex-ante chance to take up ME within this risk period. However, to appraise the sensitivity of our results to this specification, we will also estimate treatment effects leaving out all individuals from the control group who changed treatment status during the outcome period.

Table (1) shows that there is a total of 33,005 observations of which 1,275 (908) are treated and 20,763 (10,059) belong to the potential control group in west (east) Germany. The number of observations is not distributed evenly across the four cohorts, which may reflect both seasonal and cyclical effects. In each of the four cohorts, the share of unemployed men taking up ME in east Germany is much larger than in the

west, with an average share across cohorts of about 9% and 6%, respectively. This may be related to the much higher unemployment rate and poor prospects regarding regular employment in east Germany, also reflected by the high share of the total unemployment inflow in east Germany relative to Germany as a whole.[5]

The lower part of Table (1) shows the distribution of observations for men older than 50 years and for those with monthly earnings of less than 166 Euro. Below, we will present separate estimation results for older men to check whether ME may act as a stepping stone into regular employment also for older people whose share in long-term unemployment is disproportionally high in both east and west Germany. We will also estimate separate treatment effects for ME with monthly earnings of less than 166 Euro to check whether they differ from those obtained for the treatment group as a whole. As mentioned in Section 2, in the observation period the maximum amount a recipient of unemplomyent compensation could earn was 165 Euro per month (earnings above this threshold were withdrawn at a rate of 100%). One might therefore expect that people earning less than this threshold are just topping-up unemployment benefits and ME in this case should be evaluated differently than in case ME is associated with earnings above this threshold. In fact, Table (1) shows a large share of ME below this threshold: About 45% in west Germany and more than 70% in east Germany. Since there is no direct information in our data about whether an individual still receives unemployment benefits while being in ME, we can only indirectly distinguish between ME undertaken as a means of topping-up unemployment benefits and 'pure' ME by using information on the corresponding amount of earnings.

Table 1: Sample description

	West Germany		**East Germany**	
	Pot.controls	**Treated**	**Pot. controls**	**Treated**
Number of observations	20763	1275	10059	908
Cohorts				
Cohort 1	4058	261	1976	165
Cohort 2	4486	308	2215	211
Cohort 3	6411	412	2909	273
Cohort 4	5808	294	2959	259
Number of obs. (50+ yrs.)	3285	243	1981	202
Number of obs. (<166 Euro)	20763	577	10059	648

Note: For the definition of the potential control group, see text.
Source: Own calculations based on EP-FEA data.

Our choice of the inflow sample is motivated by the requirement of sufficiently long observation periods before and after the risk period. The choice of the risk period, in

[5]The east-German share of 1/3 of the total unemployment inflow is almost double its population share.

turn, takes into account that, during the first few months of unemployment, people tend to search for regular employment or do not search for a job at all, and may only later lower their aspiration levels and take up ME. Furthermore, the pressure from the labour office to take up ME might also increase with the duration of unemployment. By chosing a 9-months risk period we account for these effects and, at the same time, leave a sufficiently long time period to evaluate the longer-term effects of taking up ME.

To evaluate the labour market effects of ME, we define several outcome variables over a period of a minimum of 3 years (subsequently denoted as 'outcome period') after the risk period ends (Phase 3, see Figure (2)). For each cohort, the end of the risk period is set before the beginning of 2003 to avoid interference of the 'Mini-Job' reform implemented in April 2003 (see Section 2) and anticipatory effects in the first quarter of that year. The chosen length of the outcome period allows us to study longer-term (dynamic) effects of participating in ME on labour market outcomes. The outcome variables of interest are: (i) time spent in regular full-time employment, (ii) in ME, (iii) in registered unemployment, and (iv) wages.

Table 2: Descriptive statistics for outcome variables

	West Germany				East Germany			
	Pot. controls		Treated		Pot. controls		Treated	
	Mean	Std	Mean	Std	Mean	Std	Mean	Std
Days in regular employment	154.68	137.46	119.63	125.81	145.75	130.97	132.26	121.56
Days in ME	19.69	57.12	116.19	126.39	18.07	52.39	95.23	115.69
Days in ME ($<$ 166 Euro)	9.59	33.88	51.93	86.95	11.73	39.07	62.76	93.79
Days in unemployment	190.24	135.59	128.21	116.19	200.58	130.81	135.64	114.08
Wage (during employment)	1657.63	935.25	882.87	757.59	1306.70	698.85	857.30	619.60

Note: For the definition of the potential control group, see text.
Source: Own calculations based on EP-FEA 1998-2005.

Table (2) shows that, both in west and east Germany, previously unemployed men who took up ME within the risk period (treatment group) spent less time in regular employment during the outcome period than those who did not take up ME within the risk period (unmatched control group), but also spent much less time in unemployment. On average, the treatment group was in ME for 116 (95) days in west (east) Germany. Due to our definition given above, we also measure the time spent in ME for the control group. Compared to ME overall, the average number of days in ME with monthly earnings below 166 Euro is much lower for the treatment group both in west and east Germany. This may suggest that topping-up unemployment compensation by ME is used as a temporary rather than a permanent option. Almost by definition, the average monthly wage in employment is much smaller in the treatment group than in the unmatched control group, although this difference is

less pronounced in east Germany. Monthly earnings coded in the EP-FEA data are derived from information on daily earnings and employment days.[6]

Of course, these differences in outcome variables between the potential (unmatched) control and the treatment group do not represent the effect of taking up a ME on future labor market outcomes because the two groups are likely to differ in various characteristics affecting both selection into ME and the respective outcome variable. In the next section, we present our approach to account for potential selection effects in the estimation of treatment effects.

4 Matching Methodology

We apply propensity-score matching to estimate the average effect of taking up ME in the group of previously unemployed people who actually took up ME instead of remaining unemployed and continued searching for a regular job. This effect, which is the focus of much of the recent evaluation literature (see, e.g., Heckman, LaLonde and Smith, 1999), is termed the *'average treatment effect on the treated'*, ATT. It is defined as $ATT(X) = E(Y_1 - Y_0|D = 1, X)$, where Y_1 is the potential outcome if the individual with observable characteristics X takes up ME ($D = 1$), Y_0 is the potential outcome if the individual does not self-select into ME ($D = 0$), and E is the mathematical expectation operator. By simple averaging, ATT for some sub-sample or the whole sample of participants can be derived, e.g. for the latter $ATT = E_X[E(Y_1 - Y_0|D = 1, X)] = E_X[(E(Y|D = 1, X) - E(Y|D = 0, X))|D = 1]$.

Statistical matching is based on variants of the Conditional Independence Assumption (CIA) which states that, conditional on X, the potential outcomes are independent of participation in the programme. Since we estimate ATT, we only need to assume that Y_0 is independent of D, because the moments of the distribution of Y_1 for the treatment group can be directly estimated. In fact, for the purpose of estimating ATT we can even rely on the less restrictive assumption that the conditional mean of the outcome variable is independent of treatment status, i.e. $E(Y_0|D = 0, X) = E(Y_0|D = 1, X) = E(Y_0|X)$. In other words, selection into ME only depends on observables X, but does not depend on unobservable factors.

This is obviously a rather strong assumption whose credibility crucially depends on the quality of the set of matching variables (see, e.g., Caliendo and Kopeinig, 2005). We believe that the quality of our data base and the large number of matching variables which potentially affect both the selection into ME and outcome variables

[6] If a worker was employed for less than 30 days within a month, his wage is adjusted to correspond to a full employment month.

allows us to maintain this assumption. Given this assumption holds, the ATT can consistently be estimated simply by taking the mean over the difference of each participant (or a sub-group of participants defined by the respective partitioning of X) and some weighted control group of non-participants, i.e.:

$$ATT = \frac{1}{N_1} \sum_{i=1}^{N_1} \left(Y_{1i} - \sum_{j=1}^{N_0} \omega(i,j) Y_{oj} \right)$$

where N_1 (N_0) is the number of participants (non-participants) and (i,j) is a weight placed on the j-th individual from the control group of non-participants in constructing the counterfactual for the i-th individual of the treatment group. Matching estimators differ in the choice of the weighting function (see, e.g., Heckman, Ichimura, Smith, and Todd, 1998). Here, we apply a two-step matching estimator:[7]

In the first step, we match on all variables defined at the time when entering unemployment using a combination of nearest neighbour and caliper matching. In particular, for each treated person i we identify six individuals (with replacement) in the control group for whom the estimated propensity score is nearest to the one of person i. Of those individuals we only keep persons whose propensity score lies within a radius of 0.005, which guarantees satisfactory matching quality, especially at the tails of the distribution of propensity scores.

In the second step, we directly match on the elapsed duration in unemployment before ME is taken up within the risk period. It is, of course, crucial to match on this variable to avoid comparing a treated person who takes up ME after an elapsed unemployment duration of, say, 9 month with a person from the potential control group with only three month of elapsed unemployment duration. Since it varies with process time, this variable cannot be used in the first-step of the matching procedure. Given that a minimum of 3 months unemployment is required to be included in our sample and our definition of the risk period, we define the following unemployment duration categories: one quarter of elapsed unemployment duration, two quarters or three quarters. Then, for each person in the treatment group with a given elapsed duration of unemployment at the time of taking up ME we select those individuals from the pool of up to six potential controls with a similar elapsed duration of unemployment. So, for example, a treated person with an elapsed unemployment duration of, say, 2 quarters is only matched to members of the potential control group with at least 2 quarters of unemployment. If no match among the group of up to six potential candidates is found, we match the treated person to controls from the

[7] Alternative ways to match on variables varying over the outcome period are presented in, e.g., Lechner (1999) and Sianesi (2004).

nearest duration category. The chosen number of six nearest neighbours guarantees both a sufficient number of individuals to allow for exact matches in the second step and a high matching quality, as shown below.

For statistical matching to work, it is crucial to condition on those variables expected to affect both an individual's treatment status and labour market outcomes. In German labour market studies, it is generally considered to be especially important to include indicators of an individual's previous (un)employment history in the set of matching variables (see, e.g., Lechner, 1999; Caliendo and Kopeinig, 2005). Given the chosen inflow period, we observe how much time (in days) an individual spent in regular employment, in ME or out-of-the-labour force during a period of 2 full years before entering unemployment (Phase 1, see Figure (2) in the previous section). Since information on registered unemployment is added only after 1999 and we require that an individual in the sample must have been in some form of employment immediately before entering the unemployment spell, we have one full year (Phase 2) during which we can compute individual durations in unemployment.

In addition to indicators of an individual's previous employment history, we match on a large number of other individual characteristics dated at the time of entry into unemployment. These include an individual's age, his previous wage, the level of education[8], nationality, previous professional position, size and industry classification of the last firm, and the quarter of an individual's entry into unemployment. Table (A1) in the Appendix contains descriptive statistics on all matching variables for, respectively, the treatment and the potential control groups in the two regions. The table shows large differences in sample means of most variables between the treatment and the potential control group before matching on observable characteristics. For example, the average wage in west Germany earned by the treatment group before entering unemployment is about 430 Euro less than among the potential controls, whereas the share of time spent in ME by the former is 12.2% compared to only 3.7% in the latter group.

[8]As the EP-FEA has a rather high share of observations with missing information about educational attainment, we have used the panel structure of the data to impute information on education from earlier or later observations of the same person. Observations with missings in the education variable for which we could not impute valid values this way, are given a 'missing' dummy variable as education category in the probit estimation of propensity scores below.

5 Results

5.1 Propensity-Score Matching

Estimation results for the probit models used to calculate propensity scores for matching potential controls to treated individuals are contained in Table (A2) the Appendix. We have estimated these probits for the total east and west German samples and also for the restricted samples including only those observations in the treatment group with monthly earnings of less than 166 Euro. The same set of matching variables is included in all models. To allow for age differences in selection into ME we include interaction terms of an age dummy (50+ years) and some of the matching variables. As expected, age, indicators of an individual's previous (un)employment history and some of the interaction terms between age and some of these indicator variables have significant and strong effects on selection into ME.

To test if our matching procedure balances the distribution of matching variables between the treatment and the control group, we use the standardized bias (SB) measure suggested by Rosenbaum and Rubin (1985). For each matching variable the SB is defined as the difference of sample means in the treated and matched control sub-samples as a percentage of the square root of the average of sample variances in both groups, i.e.

$$SB = 100 \cdot \frac{\overline{X_1} - \overline{X_0}}{\sqrt{0.5(V_1(X) + V_0(X))}}$$

In addition, we have also calculated conventional t-tests for equality of means in two independent samples (assuming equal population variances) for each matching variable. As shown in Table (3), only in one case does the t-test exceed the critical value of about 2 (at the 5% significance level, two-sided test) after matching. This is of little concern, however, since the coefficient of the variable in question ('skilled' in east Germany) is not statistically significant in the probit equation (see Table (A2) in the Appendix). That the matching procedure is fairly succesful in balancing the two groups in terms of observable characteristics is also suggested by the before-after comparison of the SB measure which shows a substantial reduction for almost all matching variables.

Following usual practice (see, e.g., Caliendo and Kopeinig, 2005), we estimate treatment effects only for those for whom we have identified controls with similar propensity scores or, in the language of the matching literature, for observations in the two groups with 'common support' regarding the propensity score. As indicated by the box-plots of the distribution of estimated propensity scores in Figure (A1),

Table 3: Standardised Bias and t-test

	West Germany				East Germany			
	SB		t-test		SB		t-test	
	Before	After	Before	After	Before	After	Before	After
30< Age <50 years	4.8	2.6	4.90	0.82	6.7	3.0	4.70	0.78
Age >49 years	8.2	0.6	8.31	0.2	6.2	-2.4	4.38	-0.65
Wage	-43.8	3.2	-14.41	1.02	-27.9	3.9	-7.81	1.02
Education: (Base: Unskilled)								
Skilled	-16.8	1.3	-16.98	0.41	1.5	8.1	1.03	2.15
High-skilled	-17.6	1.6	-17.75	0.50	-8.4	-2.9	-5.94	-0.76
Education missing	28.0	0.1	28.26	0.03	10.2	-5.0	7.19	-1.31
Share in:								
ME	42.9	-3.7	19.22	-1.15	31.4	-3.5	10.63	-0.92
RE	-39.1	3.6	-15.16	1.15	-22.3	4.2	-6.73	1.10
OLF	2.6	-2.5	0.89	-0.80	-6.0	-4.3	-1.67	-1.12
UE	14.4	-1.2	5.13	-0.37	4.5	-1.8	1.31	-0.47
Interaction with Age > 49 yrs:								
50+ × Wage	-8.6	-2.1	-2.72	-0.65	-2.8	-2.0	-0.76	-0.52
50+ × Share in ME	26.6	2.1	15.73	0.67	17.9	-3.4	7.18	-0.89
50+ × Share in RE	-6.3	-0.6	-2.08	-0.18	2.2	-1.5	0.64	-0.40
50+ × Share in OOLF	8.2	0.7	2.99	0.21	-1.1	-1.6	-0.33	-0.44
50+ × Share in UE	14.6	1.1	5.99	0.36	0.7	0.1	0.20	0.03
Nationality	20.0	-0.8	20.20	-0.27	5.7	-2.2	4.05	-0.57
Firm size: (Base: <5 employees)								
5-9 employees	9.7	0.6	9.77	0.20	7.9	1.1	5.59	0.32
10-19 employees	1.3	2.2	1.26	0.70	5.5	1.1	3.88	0.28
20-49 employees	-9.6	-1.0	-9.66	-0.32	-5.6	3.0	-3.92	0.83
50-199 employees	-6.0	-0.5	-6.05	-0.16	-6.2	0.9	-4.39	0.23
200+ employees	-11.3	-4.6	-11.45	-1.45	-16.8	-5.7	-11.84	-1.51
Cohort dummies: (Base: Cohort 1)								
Cohort 2	6.4	-3.3	6.50	-1.03	3.1	-2.1	2.17	-0.55
Cohort 3	2.5	4.3	2.48	1.37	3.2	0.4	2.25	0.10
Cohort 4	-11.5	2.1	-11.61	0.67	-2.4	2.0	-1.67	0.54
Industries: (Base: Agriculture)								
Manufacturing	-10.7	-2.9	-10.83	-0.92	-2.6	0.8	-1.86	0.21
Construction	-9.1	4.5	-9.17	1.43	0.8	4.8	0.58	1.31
Trade	1.7	-3.5	1.71	-1.08	3.3	-0.2	2.31	-0.07
Transportation	16.6	0.9	16.75	0.27	6.3	-1.3	4.46	-0.33
Business services	-4.1	0.1	-4.13	0.02	2.4	-0.4	1.69	-0.12
Personal services	15.3	-0.9	15.42	-0.29	9.0	-0.3	6.32	-0.06
Public services	-6.1	-0.0	-6.19	-0.01	-13.1	-5.5	-9.23	-1.45
Occupational status: (Base: Apprentice)								
Worker	5.4	1.3	5.467	0.40	0.6	-2.1	0.41	-0.56
Craftsmen	-18.0	4.4	-18.19	1.41	-7.3	6.0	-5.13	1.61
Appointee /Clerk /Employee	-23.1	-3.0	-23.31	-0.93	-13.2	-2.4	-9.29	-0.63
Part-time workers	40.9	-3.1	41.27	-0.97	23.3	-1.7	16.40	-0.44

Note: For definition of the Standardised Bias (SB) and the t-test, see text. Description of variables are in Table (A1) in the Appendix.

Source: Own calculations based on the EP-FEA data.

the overlap between the two groups is quite good in general, with the exception of some treated persons with very high scores. However, only very few observations

had to be dropped because no suitable matched controls could be found, which had no effect on estimation results.

5.2 Employment Effects

Estimated average employment effects of taking up ME in the reference period are summarized in Table (4), where we distinguish between three outcome variables: regular employment, ME, and unemployment. Estimated average treatment effects are reported for our sample of all men living in east and west Germany, respectively, as well as for those aged 50 or older and for those with monthly earnings below the threshold of 166 Euro defined by the unemployment compensation system. Employment effects are measured as differences in days per year.

Table 4: Average employment effects (in days per year)

	West Germany			East Germany		
	All	**(50+)**	**(<166 Euro)**	**All**	**(50+)**	**(<166 Euro)**
Regular employment	-0.77	-9.96	-0.59	8.92	-1.99	11.18
	(4.82)	(9.67)	(6.41)	(5.62)	(11.49)	(6.80)
Marginal employment	89.90***	166.30***	90.16***	70.35***	142.88***	68.25***
	(3.49)	(10.96)	(5.52)	(4.70)	(12.50)	(4.80)
Unemployment	-89.72***	-156.51***	-90.23***	-80.55***	-141.81***	-80.65***
	(4.80)	(11.19)	(6.59)	(6.34)	(12.61)	(7.70)

Notes: Bootstrapped standard errors in parentheses are calculated following Efron and Tibshirani (1986) on the basis of 100 repetitions. *, **, *** indicate statistical significance at the usual levels 10%, 5% and 1% respectively.

As the first line in Table (4) shows, time spent in regular employment within the observation period does not differ significantly between the treatment and the control group. This holds, on average, for the whole treated population in both west and east Germany as well as for older people and for those with monthly earnings below 166 Euro.

As to the other two outcome variables, estimated average treatment effects more or less compensate each other: In west Germany time spent in ME within the observation period is about 90 days per year higher, on average, and unemployment duration is shorter by the same number of days in the treatment group. Given that the outcome period lasts for at least 3 years, this means a total reduction in unemployment duration of roughly 9 months.

Although time spent in ME and in unemployment more or less compensate each other for the older treatment group as well, estimated effects are almost twice as large as those obtained for the treatment group as a whole: Taking up ME reduces unemployment duration in the outcome period for older people by 156 (142) days

per year in west (east) Germany, with corresponding increases in time spent in ME. For the whole outcome period, this amounts to a reduction in the total duration of unemployment of about 1.3 years.

Regarding ME with earnings of less than 166 Euro, estimated treatment effects hardly deviate from average effects. Our results thus do not support the popular view that ME is just a means to supplement unemployment benefits thereby prolonging unemployment and reducing incentives to take up regular employment. Since estimation results differ little between the full and the restricted sample, in the following we only discuss estimation results for the whole sample.

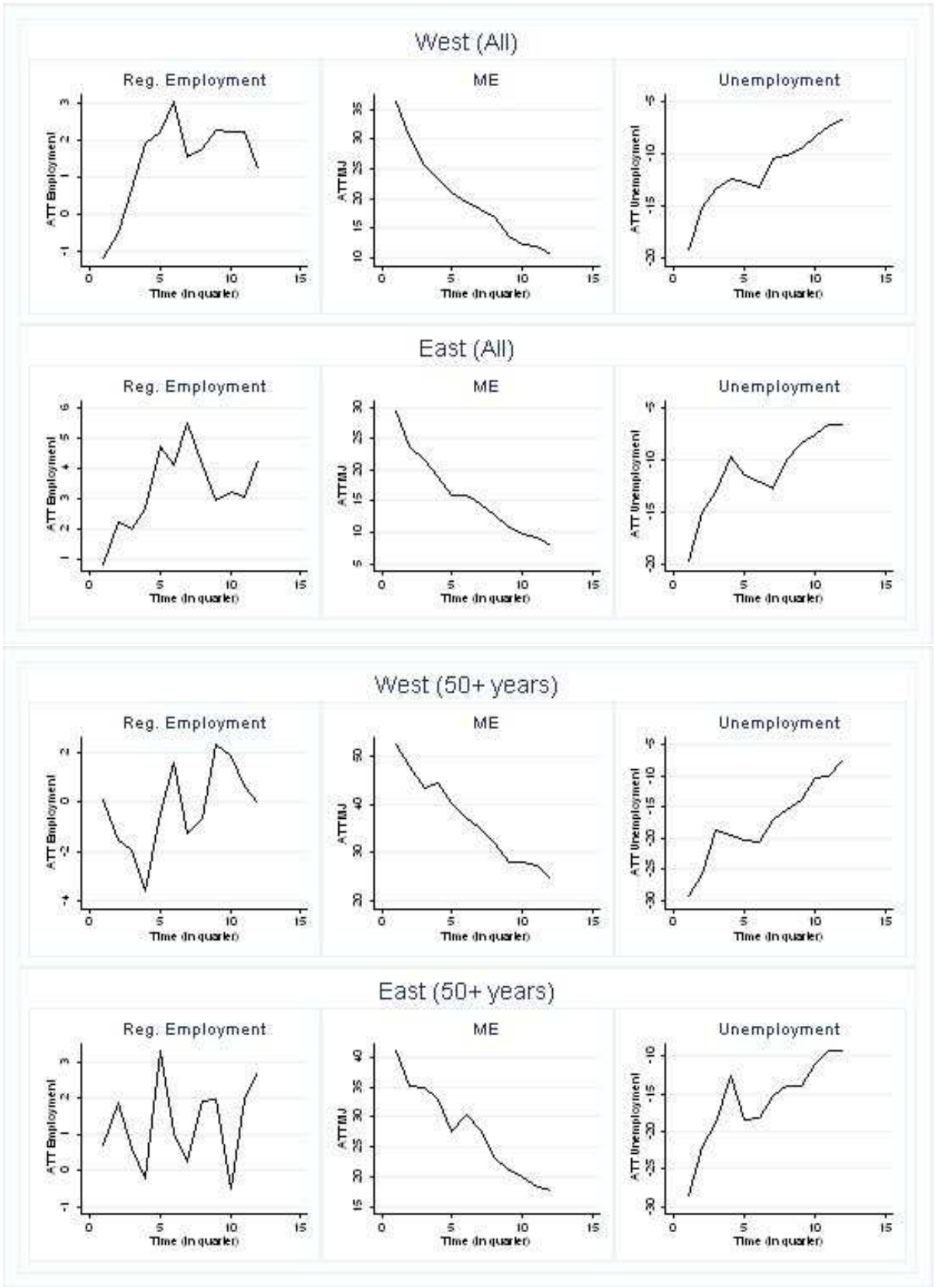
Figure (3) shows dynamic treatment effects for the three (un)employment outcome variables measured as absolute difference in days in each quarter of the outcome period. Regarding regular employment, estimated treatment effects are virtually zero throughout, and this holds for both west and east Germany as well as for older people in both regions. If anything, regular employment in the treatment group seems to be increasing in time relative to the respective control group, but in each quarter the difference is very small and not statistically significantly different from zero.

Estimated zero treatment effects reported in Table (4) do not result from a dynamic pattern with, for example, initially less time spent in regular employment by the treatment group relative to the control group being later compensated for by relatively more time spent in regular employment by the former group. Thus, referring to the question asked in the title of our paper, ME does not seem to act as a stepping stone to regular employment.

However, neither is ME a dead end, as the pattern of dynamic treatment effects for the ME and unemployment outcome variables in the middle panel of Figure (3) shows: The average ME treatment effect in west (east) Germany , i.e. the difference in time spent in ME by the treatment and the control group, declines from about 35 (30) days per quarter at the beginning of the outcome period to 10 (8) days after 12 quarters. For older people both in west and east Germany, the decline of treatment effects is similar in relative terms, although their level is substantially higher, corresponding to the larger absolute average treatment effects documented in Table (4).

For the unemployment outcome variable, Figure (3) shows roughly the opposite pattern of dynamic treatment effects: The difference in unemployment between the treatment and the control group declines rapidly in the first few quarters of the outcome period, then increases again (in absolute terms), or at least does not further decline, for a couple of quarters, and subsequently continues to decline until it

Figure 3: Dynamic (un)emplomyent effects



reaches only a few days at the end of the outcome period. There is little difference here between estimated average treatment effects between the two regions and also between the treatment group as a whole and the sub-sample of older people. Since the dynamic treatment effects plotted in Figure (3) measure differences in the outcome variables, they are not informative on the question whether these effects are driven by changes of a particular outcome variable in the treatment group or in the control group, or both. Furthermore, the number of days spent in a particular labour market state in any given quarter by each of the two groups can be split up into the share of people with at least one day spent in each of these states and the average number of days spent in each state. Figure (A2) in the Appendix plots the shares of people in both the treatment and the control groups with zero days spent in a particular state in a given quarter.

For the treatment group as a whole, the share of people with zero days in ME increases substantially over time, whereas this share declines slightly but steadily in the respective control group. This shows that the pattern of dynamic treatment effects for the ME outcome variable depicted in Figure (3) is mainly driven by the increasing share of people in the treatment group who terminate ME, although the slightly increasing share of people who took up ME during the outcome period has also contributed to the adjustment process.

The opposite dynamic pattern is observed for the unemployment outcome variable: The share of people with zero days of unemployment in the treatment group remains more or less at its initial level, whereas for the respective control group this share strongly increases over time reaching a similar level as the treatment group at the end of the observation period.

As mentioned above, an increasing share of people whom we included in our control group because they did not take up ME in the pre-specified risk period have actually become marginally employed during the outcome period. As a sensitivity check, we have excluded people from the control group who changed treatment status during the outcome period, i.e. moved from unemployment or regular employment into ME, and re-estimated treatment effects for the (un)employment outcome variables. Estimation results, summarized in the upper part of Table (A3) in the Appendix, show that our conclusions derived from Table (4) do not change qualitatively if we use this modified control group. Although treatment effects estimated for the ME and unemployment outcome variables increase in absolute terms, the magnitude of these changes is rather small. For example, for the unemployment outcome variable the estimated average effect for the west-German treatment group as a whole increases from about -90 days (with an estimated s.e. of 4.8) to -107 days (s.e. = 5.4), hardly a

statistically significant change. Of course, estimates of these 'conditional' treatment effects, conditional of not taking up ME in the risk period may induce selection bias. Furthermore, even if these conditional treatment effects were unbiased, their interpretation is difficult since they do not refer to a clearly defined reference group.

5.3 Earnings Effects

Estimated earnings effects of taking up ME in the reference period measured as differences in monthly earnings between the treatment and the control group are summarized in Table (5) for all treated persons and for those aged 50 years or older. The control group is defined as in the main analysis of employment effects above, i.e. including those who took up ME after the risk period.

Table 5: Average earnings effects

	West Germany		East Germany	
	All	**(50+)**	**All**	**(50+)**
Average effects on earnings in (monthly earnings in Euro)				
Employment (reg. and marg.)	-473.39***	-742.55***	-303.09***	-492.38***
	(36.03)	(120.42)	(33.63)	(81.49)
Regular employment only	-166.47***	-109.91	-58.09*	-31.25
	(38.49)	(149.56)	(32.76)	(97.01)
Cumulative effects (Earnings in Euro per year)	189.37	-642.83	732.87	779.11

Bootstrapped standard errors in parentheses, see Efron and Tibshirani (1986), were calculated on the basis of 100 repetitions. *, **, *** indicate statistical significance at the usual levels 10%, 5% and 1% respectively.

The first row in Table (5) shows that, conditional on either ME or regular employment, average monthly earnings of the treatment group were much lower than those of the control group during the outcome period; the (negative) average treatment effect amounts to about 470 (300) Euro in west (east) Germany. In both regions, the treatment effects on earnings are particularly strong for older men, amounting to about 740 Euro per month in west Germany and to 490 Euro in the east. Since these effects are conditional on being either in ME or in regular employment, the large negative earnings differential virtually arises by definition of ME. More interesting is the estimated treatment effect on earnings in regular employment. This effect amounts to about 166 (60) Euro in west (east) Germany for the treatment group as a whole, but is not statistically significant for older people.[9] Regional differences in

[9]We obtain qualitatively similar results when people who have changed treatment status during the outcome period are excluded from the control group; see the estimation results in the lower

estimated treatment effects on earnings are probably related to the still much lower level of wages in regular employment in the east-German labour market.

Since we know from the previous section that the treatment group spends roughly the same time in regular employment as the control group, but less time in unemployment, one would expect that earnings cumulated over the outcome period are higher for the treatment group, on average. As the last row in Table (5) shows, this conjecture is indeed true, on average, although the estimated cumulated earnings effect differs substantially by region and age: Whereas it amounts to about 730 Euro per year in east Germany, where it differs little by age, the cumulative average earnings effect is less than 200 Euro in the west, and even slightly negative for older people.[10] Thus, this group's lower unemployment in the outcome period does not fully compensate for the negative earnings differential in either form of employment born by the treatment group during the outcome period, if this only amounts to a fairly small negative cumulative earnings effect of about 640 Euro per year.

6 Conclusion

In this paper we have investigated, for the German economy, whether marginal employment (ME) acts as a stepping stone for unemployed people into regular employment or rather leads to dead-end jobs with little pay and a high degree of job insecurity, as critics claim. Using newly available register data from the Federal Employment Agency covering the period 1998 - 2005 and statistical matching techniques, we have analyzed differences in various labour market outcome variables between a treatment group of previously unemployed men who took up ME at the beginning of their unemployment spell and a control group of people who did not.

Our empirical results show that, although ME does not significantly increase the treatment group's chance to gain regular employment during an outcome period of at least three years, it reduces future unemployment and, on average, slightly increases cumulated future earnings of the treatment group relative to the control group. The treatment effect on future unemployment is substantial in both west and east Germany: During the three-years' outcome period the total duration of unemployment experienced by the treatment group is reduced by about 9 months, on average, relative to the control group. For older people, unemployment treatment effects are almost twice as large as those obtained for the treatment group as a whole,

part of Table (A3) in the Appendix.

[10] We have used the estimated negative earnings differential in regular employment for older people, although it is not statistically significant.

amounting to a reduction in the total duration of unemployment of about 1.3 years during the whole outcome period. Still, the cumulative treatment effect on earnings for older men is slightly negative in west Germany and only modestly positive in east Germany. From a policy perspective, our evaluation results suggest that exempting social security contributions may be effective in reducing unemployment, especially regarding older men who have been an important target group of recent 'active' labour market policy in Germany, see, e.g., Haan and Steiner (2006).

These results seem robust with respect to our definition of the control group and to the inclusion of ME with earnings below the maximum threshold for receiving unemployment benefits. Excluding people from the control group who changed treatment status during the outcome period, i.e. moved from unemployment or regular employment into ME, did not significantly change estimated treatment effects for the (un)employment outcome variables. We also could not find any evidence for the hypothesis that ME is just a means to supplement unemployment benefits thereby prolonging unemployment and reducing incentives to take up regular employment. However, given the limitations of the statistical matching approach, we cannot rule out substitution effects between ME, which is exempted from social security contributions, and regular employment. In fact, recent empirical evidence for Germany suggests that ME and regular employment are substitutes in production, even if the size of the substitution elasticity is not very large for men in Germany (see Freier and Steiner, 2007).

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8 Appendix

Table A1: Descriptive statistics

	West				East			
	Control		Treated		Control		Treated	
	Mean	Std	Mean	Std	Mean	Std	Mean	Std
30< Age <50 years	0.458	0.248	0.482	0.250	0.459	0.248	0.493	0.250
Age >49 years	0.160	0.135	0.191	0.155	0.199	0.159	0.224	0.174
Wage	1721.0	1037.8	1290.3	924.4	1272.2	750.0	1069.3	705.1
Education: (Base: Unskilled)								
Skilled	0.522	0.250	0.438	0.246	0.650	0.227	0.657	0.225
High-skilled	0.035	0.034	0.009	0.009	0.038	0.036	0.023	0.023
Education missing	0.167	0.139	0.283	0.203	0.158	0.133	0.196	0.158
Share in:								
Marginal Employment (ME)	0.037	0.146	0.122	0.239	0.035	0.137	0.088	0.194
Regular Employment (RE)	0.771	0.278	0.647	0.354	0.724	0.290	0.656	0.326
Out of the labor force (OLF)	0.108	0.177	0.113	0.171	0.096	0.161	0.087	0.150
Unemployment (UE)	0.213	0.293	0.257	0.315	0.268	0.315	0.283	0.325
Interaction with Age > 49 yrs:								
50+ × Wage	336.7	928.3	264.3	752.2	289.0	687.1	270.9	614.9
50+ × Share in ME	0.005	0.060	0.037	0.157	0.005	0.055	0.021	0.107
50+ × Share in RE	0.131	0.320	0.112	0.292	0.147	0.319	0.154	0.321
50+ × Share in OOLF	0.010	0.057	0.015	0.064	0.016	0.067	0.015	0.07
50+ × Share in UE	0.026	0.123	0.048	0.174	0.050	0.166	0.051	0.163
Nationality	0.190	0.154	0.275	0.199	0.040	0.039	0.052	0.049
Firm size: (Base: <5 employees)								
5-9 employees	0.129	0.112	0.163	0.136	0.126	0.110	0.153	0.130
10-19 employees	0.146	0.125	0.150	0.128	0.137	0.118	0.156	0.132
20-49 employees	0.181	0.148	0.146	0.124	0.194	0.156	0.172	0.142
50-199 employees	0.221	0.172	0.197	0.158	0.229	0.176	0.203	0.162
200+ employees	0.189	0.153	0.146	0.125	0.186	0.152	0.125	0.110
Cohort dummies: (Base: Cohort 1)								
Cohort 2	0.216	0.169	0.243	0.184	0.220	0.172	0.233	0.179
Cohort 3	0.308	0.213	0.320	0.217	0.288	0.205	0.303	0.211
Cohort 4	0.282	0.202	0.232	0.178	0.295	0.208	0.284	0.203
Industries: (Base: Agriculture)								
Manufacturing	0.219	0.171	0.176	0.145	0.106	0.094	0.098	0.088
Construction	0.211	0.166	0.175	0.144	0.306	0.212	0.310	0.214
Trade	0.135	0.117	0.141	0.121	0.079	0.072	0.088	0.080
Transportation	0.073	0.068	0.122	0.107	0.055	0.052	0.070	0.065
Business services	0.175	0.144	0.160	0.134	0.144	0.123	0.152	0.129
Personal services	0.078	0.072	0.124	0.109	0.070	0.065	0.094	0.085
Public services	0.067	0.062	0.052	0.049	0.159	0.134	0.114	0.101
Occupational status: (Base: Apprentice)								
Worker	0.372	0.234	0.399	0.240	0.211	0.166	0.213	0.168
Craftsmen	0.299	0.209	0.220	0.172	0.425	0.244	0.390	0.238
Appointee /Clerk /Employee	0.174	0.144	0.096	0.087	0.126	0.110	0.085	0.078
Part-time workers	0.094	0.085	0.244	0.185	0.166	0.138	0.261	0.193

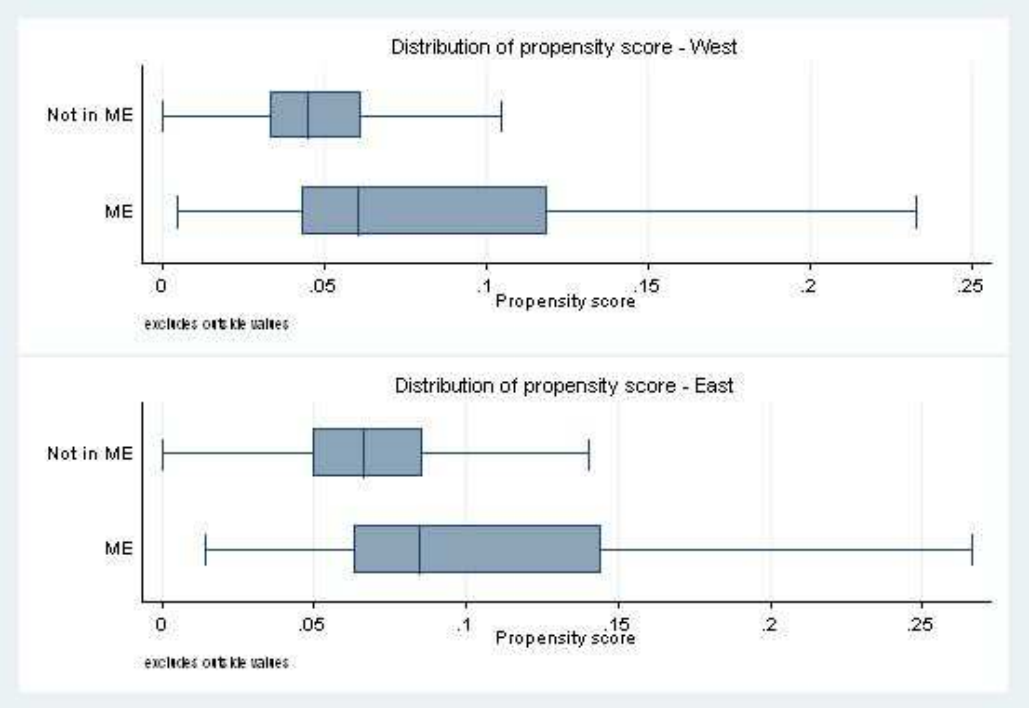
Source: Own calculations based on the EP-FEA data.

Table A2: Probit estimates of the propensity scores

	All		<166	
	West	**East**	**West**	**East**
30< Age <50 years	0.117***	0.132***	0.177***	0.141***
	(0.034)	(0.045)	(0.045)	(0.051)
Age >49 years	-0.376	0.244	-0.170	0.369
	(0.448)	(0.477)	(0.562)	(0.523)
Wage	0.000***	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
$(\text{Wage})^2$	-0.000***	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
$(\text{Wage})^3$	0.000***	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Education: (Base: Unskilled)				
Skilled	0.047	0.096	0.001	0.148**
	(0.040)	(0.067)	(0.052)	(0.074)
High-skilled	-0.297**	0.010	-0.380**	0.079
	(0.129)	(0.127)	(0.191)	(0.142)
Education missing	0.124***	0.088	0.084	0.074
	(0.043)	(0.074)	(0.056)	(0.083)
Share in:				
ME	4.659***	3.868***	4.352***	4.203***
	(0.571)	(0.730)	(0.727)	(0.794)
$(\text{Share in ME})^2$	-9.328***	-6.923***	-7.945***	-8.002***
	(1.685)	(2.282)	(2.144)	(2.472)
$(\text{Share in ME})^3$	4.753***	3.229*	3.591**	3.894**
	(1.239)	(1.758)	(1.587)	(1.920)
RE	-0.826	0.650	-0.895	0.293
	(0.568)	(0.719)	(0.710)	(0.782)
$(\text{Share in RE})^2$	0.869	-1.644	1.288	-1.304
	(1.249)	(1.542)	(1.605)	(1.679)
$(\text{Share in RE})^3$	-0.146	1.008	-0.429	0.864
	(0.787)	(0.966)	(1.021)	(1.055)
OLF	-0.240	-0.459	-0.885	-0.362
	(0.491)	(0.693)	(0.641)	(0.767)
$(\text{Share in OLF})^2$	0.560	1.285	2.240	2.052
	(2.019)	(2.934)	(2.665)	(3.323)
$(\text{Share in OLF})^3$	-0.885	-1.687	-2.207	-3.405
	(2.051)	(3.048)	(2.721)	(3.568)
UE	0.375	0.197	0.844	0.125
	(0.421)	(0.530)	(0.538)	(0.576)
$(\text{Share in UE})^2$	-1.359	-1.171	-2.019	-1.047
	(1.094)	(1.342)	(1.391)	(1.473)
$\text{Share in UE})^3$	0.987	1.012	1.342	0.892
	(0.758)	(0.915)	(0.965)	(1.011)
Interaction with Age > 49 yrs:				
50+ × Wage	0.000***	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
50+ × Share in ME	1.244**	0.265	0.763	0.100
	(0.490)	(0.551)	(0.621)	(0.600)
50+ × Share in RE	0.268	-0.076	0.396	-0.032
	(0.456)	(0.491)	(0.573)	(0.541)
50+ × Share in OOLF	-0.317	-0.049	-0.000	0.188
	(0.316)	(0.385)	(0.378)	(0.416)
50+ × Share in UE	0.451	-0.260	0.151	-0.346
	(0.370)	(0.396)	(0.454)	(0.438)
Nationality	0.147***	0.084	0.135***	-0.085
	(0.034)	(0.089)	(0.044)	(0.110)
Firm size: (Base: <5 employees)				
5-9 employees	-0.008	-0.080	-0.078	-0.026
	(0.051)	(0.065)	(0.066)	(0.070)
10-19 employees	-0.066	-0.068	-0.147**	-0.120*
	(0.051)	(0.064)	(0.068)	(0.072)
20-49 employees	-0.165***	-0.188***	-0.136**	-0.211***
	(0.051)	(0.062)	(0.065)	(0.069)
50-199 employees	-0.114**	-0.190***	-0.067	-0.161**
	(0.049)	(0.061)	(0.062)	(0.067)
200+ employees	-0.152***	-0.282***	-0.215***	-0.212***

	(0.053)	(0.069)	(0.069)	(0.075)
Cohort dummies: (Base: Cohort 1)				
Cohort 2	0.040	0.053	0.008	0.008
	(0.044)	(0.057)	(0.056)	(0.063)
Cohort 3	0.025	0.051	-0.054	0.088
	(0.043)	(0.056)	(0.055)	(0.061)
Cohort 4	-0.078*	-0.002	-0.095*	0.003
	(0.044)	(0.057)	(0.056)	(0.063)
Industries: (Base: Agriculture)				
Manufacturing	-0.012	0.086	0.085	0.098
	(0.074)	(0.086)	(0.103)	(0.094)
Construction	-0.066	0.094	-0.066	0.073
	(0.073)	(0.073)	(0.103)	(0.081)
Trade	-0.001	0.070	0.091	0.172*
	(0.077)	(0.091)	(0.106)	(0.097)
Transportation	0.127	0.145	0.237**	0.143
	(0.079)	(0.095)	(0.107)	(0.105)
Business services	-0.064	0.092	0.088	0.070
	(0.075)	(0.080)	(0.104)	(0.088)
Personal services	0.056	0.129	0.153	0.116
	(0.080)	(0.091)	(0.108)	(0.100)
Public services	-0.093	0.002	0.109	0.008
	(0.090)	(0.084)	(0.120)	(0.091)
Occu. status: (Base: Apprentice)				
Worker	0.115	-0.008	0.074	-0.091
	(0.077)	(0.107)	(0.105)	(0.119)
Craftsmen	0.014	-0.089	0.055	-0.149
	(0.083)	(0.111)	(0.113)	(0.121)
Appointee /Clerk /Employee	0.005	-0.141	-0.027	-0.194
	(0.092)	(0.123)	(0.125)	(0.133)
Part-time workers	0.187**	-0.019	0.114	-0.145
	(0.086)	(0.111)	(0.118)	(0.122)
Constant	-1.603***	-1.431***	-2.065***	-1.456***
	(0.204)	(0.265)	(0.267)	(0.296)
N	21672	10821	21672	10821
N (treated)	1275	908	577	648
Log likelyhood (LL) full	-4468.38	-2965.47	-2474.77	2323.65
Log likelyhood restricted (const. only)	-4818.36	3102.05	-2657.81	2444.28

Figure A1: Distribution of propensity scores for treatment and control group



Note: The width of a rectangular box gives the distance between the 25th and the 75th percentile, the line within the box gives the median. The lines ('whiskers') at both sides of the boxplot indicate the range of one standarddeviation to each side respectively. In the graphs, a few observations with propensity propensity scores outside the indicated range are excluded.

Table A3: Sensitivity analysis - leaving out ME from the control group

	West		East	
	All	**(50+)**	**All**	**(50+)**
Reg. Employment	-8.77*	-11.83	0.06	-3.93
(in days per year)	(5.33)	(12.32)	(6.25)	(11.87)
Marginal Employment	115.12***	206.07***	93.55***	171.32***
(in days per year)	(3.39)	(9.29)	(4.37)	(12.17)
Unemployment	-107.35***	-194.67***	-95.48***	-168.77***
(in days per year)	(5.43)	(11.24)	(6.51)	(14.02)
Average effects on earnings in (monthly earnings in Euro)				
Employment	-771.85***	-1230.30***	-552.56***	-783.77***
(reg. and marg.)	(36.37)	(113.40)	(36.78)	(81.67)
Regular Employment only	- 202.67***	-100.95	-76.17***	-69.35
	(36.81)	(168.63)	(33.87)	(92.58)

Figure A2: Quarterly shares of people with zero days spent in a particular labour market state in the treatment (ME=1) and the control group (ME=0)