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Does marginal employment promote regular employment for unemployed welfare benefit recipients in Germany?

Torsten Lietzmann
Paul Schmelzer
Jürgen Wiemers

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Torsten Lietzmann (Institute for Employment Research)
Paul Schmelzer (German Institute for Economic Research)
Jürgen Wiemers (Institute for Employment Research)

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Abstract

Marginal employment (ME) is one of the largest forms of atypical employment in Germany. In this study, we analyse whether ME has a “stepping stone” function for unemployed individuals, i.e., whether ME increases the subsequent probability of regular employment. Our study adds to the literature in the following ways. First, compared to previous studies, it analyses the “stepping stone” function for a more recent time period, i.e., after Germany’s major labour-market reforms (Hartz reforms) at the beginning of the 2000s. Second, we use a new administrative data source which includes previously unavailable information on desired labour supply and household composition. Third, we follow recent methodological developments in the evaluation literature by applying a dynamic evaluation approach that has not previously been used to analyse marginal employment. Our results for single and childless unemployment benefit-II recipients highlight the importance of the dynamic approach: We find differing treatment effects by unemployment duration. According to our results, marginal employment does increase the likelihood of regular employment within a three-year observation period only for those unemployed who take up ME several months after beginning to receive benefits. In contrast, for those starting ME within the first months of receiving benefits, there is no effect on the probability of regular employment.

Zusammenfassung

In dieser Studie untersuchen wir, ob geringfügige Beschäftigung eine „Brückenfunktion“ für arbeitslose ALG-II-Bezieher hat, d. h., ob geringfügige Beschäftigung für diese Gruppe nachfolgend die Wahrscheinlichkeit einer regulären Beschäftigung erhöht. Unsere Studie erweitert die bestehende Literatur in mehrfacher Hinsicht. Erstens betrachten wir im Vergleich zu früheren Untersuchungen erstmalig die Brückenfunktion geringfügiger Beschäftigung nach Einführung der Hartz-Reformen. Zweitens verwenden wir einen neuen administrativen Datensatz, der bisher nicht verfügbare Informationen zum gewünschten Erwerbsumfang und zur Haushaltszusammensetzung enthält. Drittens verwenden wir einen dynamischen Matching-Ansatz, der hier erstmalig zur Analyse der Brückenfunktion geringfügiger Beschäftigung eingesetzt wird. Dieser Ansatz erlaubt es, die Brückenfunktion in Abhängigkeit vom Zeitpunkt der Aufnahme der geringfügigen Beschäftigung zu untersuchen. Unsere Ergebnisse zeigen, dass geringfügige Beschäftigung die Wahrscheinlichkeit regulärer Beschäftigung innerhalb einer dreijährigen Beobachtungsperiode nur für solche arbeitslosen Leistungsbezieher erhöht, die die geringfügige Beschäftigung erst mehrere Monate nach Eintritt in den ALG-II-Bezug aufgenommen haben. Im Gegensatz dazu finden wir keine Brückenfunktion für Personen, die die geringfügige Beschäftigung innerhalb der ersten Monate des Leistungsbezugs aufgenommen haben.

JEL classification: J64, J20, J08, C14

Keywords: Evaluation, Atypical employment, Administrative data, Propensity score matching, Hidden bias

1 Introduction

Throughout Europe, atypical employment is rising (Hipp et al. 2015). In Germany, marginal employment has been increasing since it was substantively reformed in 2003 and is now one of the most common forms of atypical employment. In 2014, about 7.5 million men and women were marginally employed, i.e., employed with low earnings that are not subject to social security contributions. As this segment of the labour market grows, we ask questions about its goals and functions.

This study analyses whether marginal employment has a “stepping stone” function for unemployed individuals, i.e., whether marginal employment increases the subsequent probability of regular employment. We add our results to existing empirical studies that have yielded no conclusive evidence for marginal employment’s “stepping stone” function for Germany’s unemployed. Studies that analyse the “stepping stone” function apply matching techniques to compare the labour market outcomes of initially unemployed individuals who take up marginal employment to the hypothetical counterfactual situation in which these persons remain unemployed.

Freier and Steiner (2007, 2008) find that for the period of 1993-2003, taking up a marginal job had no effect on the probability of subsequent regular employment. Nonetheless, they find that marginally employed men subsequently spent less time in unemployment and have higher cumulative incomes. Caliendo et al. (2012) also do not find a positive effect of marginal employment on regular employment for short-term unemployed men in the period 2001 to 2004. For long-term unemployed men, however, the probability of regular employment is higher for those who did take up a marginal job. In addition, Caliendo et al. (2012) find that those with a preceding temporal marginal job are more likely to find stable regular employment. Lehmer (2012), who focuses on the effects of temporary agency work on long-term unemployed men and women, also provides results for the effects of marginal employment for the period 2004 to 2008, finding that marginal employment has a small positive effect for men but no effect for women.

Our study adds to the literature in the following ways. First, it looks at a more recent time period, i.e., after Germany’s major labour-market reforms (Hartz reforms) at the beginning of the 2000s. These reforms further improved the attractiveness of marginal employment and introduced a new means-tested social benefit system called “unemployment benefit II” that focuses on labour-market activation.

Second, we use a new administrative data source for the recipients of unemployment benefit II. Unlike the data used in previous studies, our data set includes information on desired labour supply, which we use to restrict the sample to those individuals who are searching for full-time employment. Also in contrast to previous studies, our data include information on household composition, allowing us to further restrict the sample to childless single men and women, the latter of which have often been excluded because of the impossibility of controlling for the household context. These sample restrictions reduce unobserved heterogeneity since individuals with children and/or in a relationship might be less prone for active job search for regular employment than single and childless men and women, e.g. because of family obligations.

Third, we follow recent methodological developments in the evaluation literature by applying a dynamic evaluation approach (e.g., Sianesi 2004, Hujer/Thomsen 2010, Voßemer/Schuck 2015) that has not previously been used to analyse marginal employment. The results show the importance of the dynamic approach: We find differing treatment effects by unemployment duration. According to our results, marginal employment does increase the likelihood of regular employment within a three-year observation period only for those unemployed who take up marginal employment several months after beginning to receive benefits. For those starting marginal employment within the first months of receiving benefits, there is no effect on the probability of regular employment.

2 Institutional Background

Because our data refer to the period from 2005 to 2009, we describe the institutional regulations for marginal employment (ME) in Germany applicable at that time. Thus, we focus on the regulations introduced with the most recent major ME reform in 2003. This reform aimed to improve labour market flexibility and create more incentives for low-wage jobs (Jacobi/Kluve 2007). ME is designed to be attractive to low-wage employees because the employees pay no taxes or social security contributions up to a maximum labour income, which until 2012 was 400 € per month.[1] Since 2003, ME has not restricted the number of weekly working hours[2]. It is also possible to hold two or more jobs and still be considered marginally employed provided the combined income from these jobs does not exceed the monthly maximum labour income. Finally, regular employment – i.e., employment subject to taxes and social security contributions – can be combined with one secondary ME job[3]. In that case, taxes and social security contributions must only be paid for the primary job. In contrast, employers must pay a relatively larger share of social security contributions for ME (31 % of gross labour earnings) than for regular employment subject to social security (19 %). Otherwise, ME is subject to the same regulations as regular em-

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[1] This implies that marginally employed workers have no entitlement to social security benefits from health, unemployment, and pension insurances. Since 2013, ME has included liability for pension insurance contributions, although marginally employed workers can opt out of these contributions. Additionally, the maximum labour income from ME was increased to 450 € per month in 2015. Otherwise, regulations for ME have remained fundamentally unchanged since 2003.

[2] Until 2003, weekly working hours in ME were restricted to 15 hours. The introduction of the minimum wage of 8.50 €/h in 2015 implicitly restricted the maximum number of weekly working hours in ME to approximately 12 hours.

[3] If a worker takes up more than one secondary job, the combined income from the primary and secondary jobs is subject to taxes and social security contributions, even if the combined income from the secondary jobs does not exceed the maximum monthly labour income of 400 € (450 € since 2013) per month.

ployment, e.g., dismissal protection, entitlements to holidays, continued remuneration in case of sickness, and pay for public holidays.

The design of the ME regulations leads to a special attractiveness of ME for groups that can draw on other sources of private income within the household or the family or who have access to social security entitlements based on some other status; those groups include not only the unemployed but also pensioners, students, and housewives and -husbands (Bäcker/Neuffer 2012; Körner et al. 2013). Unemployed individuals with marginal jobs also have access to health care via the unemployment benefit system, and their earnings are supplemented by benefits if they pass a means test.

In 2005, Germany's social security system for the unemployed experienced major changes with the introduction of the so-called Hartz IV reform (Eichhorst et al. 2010), which arguably further increased the attractiveness of ME to the unemployed. In this reform, social and unemployment assistance were combined to form the new unemployment benefit II (UB II), which provides basic income support for job-seekers and their families if their total household income is insufficient to meet a minimal standard of living and they have no or no sufficient entitlement to unemployment insurance benefits. Along with this new benefit system, a stronger focus on activating the unemployed was included in German labour market policy so that the unemployed should be more willing to accept low-quality jobs. A refusal to accept job offers or a lacking willingness for job search activities can lead to benefit cuts. Compared to the previous systems of social and unemployment assistance, the amount of earned income that benefit recipients are allowed to retain is higher under UB II (Bruckmeier/Wiemers 2011). Recipients can earn a gross income of 100 € per month before their welfare benefits are reduced. For earnings above 100 € per month, the benefit reduction rate is 80 %. Above 800 € per month, that rate is increased to 90 %. Earnings above a threshold of 1,200 € (1,500 € for recipients with children) per month reduce the benefits at a rate of 100 %. For example, a marginally employed benefit recipient with a monthly earned income of 400 € can retain 160 € of his labour income. The high marginal benefit reduction rates between 80 % and 100 % create low monetary incentives for marginally employed UB-II recipients to extend their working hours and earnings and to leave ME in favour of regular employment.

For marginally employed workers, a maximum labour income of 400 € is insufficient to secure a minimum standard of living as defined by either UB II or commonly applied poverty thresholds. Furthermore, the exemption from social security contributions is accompanied by a lack of entitlements to health and unemployment insurance. Additionally, people in ME have less access to benefits (such as paid vacation, continued remuneration in case of sickness, and pay for public holidays) than do workers in employment subject to social security contributions (Stegmaier et al. 2015; RWI 2012). Finally, ME also yields negative long-term risks, missing pension entitlements and an associated higher probability of poverty in old age. Therefore, for UB-II recipients, it is of particular interest to know whether ME serves as a stepping stone to regular employment or whether it results in a long-term “lock-in” of employees in a situation of low labour income topped up with UB II benefits.

3 Theoretical Considerations

The effect of taking up ME on the probability of subsequent regular employment is theoretically undetermined. On the one hand, ME can improve the employment prospects of formerly unemployed men and women because it may decrease human capital depreciation in times of unemployment. ME might also provide the opportunity not only to increase a worker’s general and/or specific human capital through on-the-job training but also to alleviate negative consequences of unemployment, for example, on (mental) health (Paul/Batinic 2010; Jahoda 1982). Furthermore, employers might use ME as a screening device for productivity. Finally, the marginally employed might be able to extend their social network and establish contacts that provide them with information about job openings and assist them in their job search (Granovetter 1973) either inside or outside of the firm. These aspects should increase marginally employed individuals’ likelihood of finding regular employment compared to unemployed individuals.

On the other hand, there are reasons that individuals receiving unemployment or social benefits and working in a marginal job are less likely to take up regular employment. First, the marginally employed should have higher reservation wages than the unemployed without ME, and the time that they can devote to job search should be lower because of the time spent in ME (i.e., the “lock-in effect” of taking up ME). Second, ME is concentrated in a few sectors of the economy, which might limit both the scope of employment opportunities and the transferability of gained human capital. Third, ME largely consists of low-qualified tasks, which should limit human capital gains.

Moreover, the relative importance of the arguments for and against a “stepping stone” function will arguably change with the time of treatment. On the one hand, the longer an individual stays unemployed, the more important it will become to take up ME at least to prevent a further loss of human capital and deterioration of (mental) health. On the other hand, the arguments against a “stepping stone” effect (higher reservation wages, lock-in effect, demand-side restrictions) should have an impact on the “stepping stone” function of ME that is relatively independent from the time of treatment. Thus, we expect the effect of taking up ME on the probability of subsequent regular employment to increase with the time spent in unemployment prior to taking up ME.

For these reasons, the question whether ME provides a “stepping stone” function for the unemployed must be decided empirically. In addition, the existence of an ME “stepping stone” function is an empirically important question because the number of marginally employed UB-II recipients is quite substantial, with annual averages between 450,000-500,000 in recent years[4] (Statistik der Bundesagentur für Arbeit 2011).

4 Data and Methodology

Evaluating the effect of ME on the subsequent probability of taking up regular employment by individuals who were formerly unemployed requires longitudinal data that cover labour market histories (unemployment, benefit receipt, employment) for an extended time. To select a sample of unemployed people who are actually at risk of transitioning from ME to regular employment, it is necessary to use a data set that includes information about the extent of the individual labour supply, the existence of a desire to be regularly employed and possible competing obligations and roles to which unemployed people can allocate their time instead of employment and job-search (i.e., family obligations). We use administrative data (Administrative Panel SGB II) provided by the German Federal Employment Agency (Rudolph et al. 2013), which is a 10 % sample of UB-II recipients in Germany. Our data include longitudinal information about UB-II receipt and provide previously unavailable information about unemployed people's household context because this information has only been gathered since the introduction of UB II. These data are enriched by longitudinal information on whether the unemployed are actually looking for full-time employment from a different administrative data source (Job-Search Histories data; Köhler 2015) and on times spent in marginal and regular employment along with times spent in measures of active labour market policy (from Integrated Employment Biographies (IEB); Jacobebbinghaus/Seth 2007). These data cover almost complete (un-) employment and benefit histories for the period from 2005 to 2009.

More precisely, this study's aim is to estimate the causal effect of ME on the subsequent chances of regular employment for a group of persons who a) receive UB II and b) were unemployed at the beginning of the UB-II receipt compared to the outcome in which these persons did not take up ME. The latter outcome is obviously unobserved for the group of people who actually took up ME. Therefore, the potential outcome approach of causality (see, e.g., Roy 1951, Rubin 1974, Heckman/LaLonde/Smith 1999) is a natural point of departure for our analysis. In this approach, using standard notation, Y_1 is the potential outcome (in our case, probability of regular employment) if the person is treated (being marginally employed, $\mathrm{D} = 1$), whereas Y_0 is the potential outcome if the person is not treated (not marginally employed, $\mathrm{D} = 0$).

Recent literature notes the importance of the timing of treatment events and selection of an appropriate control group for the treated individuals (Abbring/van den Berg 2003, Sianesi 2004, Stephan 2008, Hujer/Thomsen 2010). For our application, taking up ME likely has a positive effect on regular employment because on the one

[4] Thus, approximately 10 % of all UB-II recipients of working age (15 to 67 years) were marginally employed in recent years.

hand, ME provides a form of occupational stabilisation. On the other hand, there might be a negative lock-in effect of ME in the form of reduced search efforts for regular employment. The relative importance of these effects will arguably change with the time spent receiving benefits: the former effect might become more important the longer an individual is unemployed before taking up ME, whereas the lock-in effect might be relatively more important if a person takes up ME early after beginning to receive benefits. For these reasons, we follow the dynamic matching approach of Sianesi (2004), which allows the effect of taking up ME on the probability of regular employment to change with the timing of the treatment. This approach requires that we discretise the time after entering benefit receipt. Let $\mathrm{U} = \{\mathrm{u}_1, \mathrm{u}_2, \dots, \mathrm{u}_{\max}\}$ be the set of elapsed durations after receiving UB II for the first time. Then, for each elapsed duration u, we estimate the dynamic average treatment effect on the treated, $\mathrm{ATT}_{\mathrm{t,u}}$, which is defined as the mean of the differences between the outcomes for persons in period $\mathrm{t} > \mathrm{u}$, who took up ME after an elapsed duration u of receiving UB II and the outcomes in the counterfactual situation, in which these persons would not have been treated at least until period u. Formally,

$$\begin{aligned} ATT_{t,u} &= E\left(Y_{t,u}^{1} - Y_{t,u}^{0} | D_u = 1,\ D_1 = \cdots = D_{u-1} = 0\right) \\ &= E\left(Y_{t,u}^{1} | D_u = 1,\ D_1 = \cdots = D_{u-1} = 0\right) \\ &- E\left(Y_{t,u}^{0} | D_u = 1,\ D_1 = \cdots = D_{u-1} = 0\right), \end{aligned} \qquad (1)$$

with $\mathrm{u} = \mathrm{u}_1,\ \mathrm{u}_2, \dots, \mathrm{u}_{\max}$, where $\mathrm{Y}_{\mathrm{t,u}}^1$ is the potential outcome for time t of a person who took up ME after u, whereas $\mathrm{Y}_{\mathrm{t,u}}^0$ is the corresponding potential outcome for a person who did not take up ME at least up to time u. Thus, in the dynamic matching approach, there is no clear-cut distinction between treated and non-treated persons: everyone who is regarded as non-treated after a specific period u may be treated at a later period. In contrast, in a non-dynamic matching approach, the control group would instead only consist of persons who never take up ME in the treatment period. As noted in, e.g., Hujer and Thomsen (2010), this latter definition of the control group might bias the estimated treatment effect because it is conditioned on future outcomes. For example, if individuals are never observed to take up ME because they find regular employment before doing so, the treatment effect of taking up ME will be negatively biased.

Whereas the first term of the last expression in (1) is identified by the data, the second term must be estimated. In a non-experimental study such as this one, simply substituting the counterfactual outcome $\mathrm{E}\left(\mathrm{Y}_{\mathrm{t,u}}^0 | \mathrm{D}_\mathrm{u} = 1,\ \mathrm{D}_1 = \cdots = \mathrm{D}_{\mathrm{u}-1} = 0\right)$ with the (observed) mean outcome of persons untreated until u, $\mathrm{E}(\mathrm{Y}_0 | \mathrm{D}_1 = \cdots = \mathrm{D}_\mathrm{u} = 0)$ will likely lead to selection bias, i.e.,

$$E\left(Y_{t,u}^{1}|D_u = 1,\ D_1 = \cdots = D_{u-1} = 0\right) - E\left(Y_{t,u}^{0}|\ D_1 = \cdots = D_u = 0\right) \neq 0, \tag{2}$$

because individual characteristics that determine the outcome will typically also determine the treatment decision. Thus, for an unbiased estimate of the ATT in non-experimental situations, identifying assumptions to solve the problem of self-selection must be invoked.

In recent years, propensity score matching has become the standard approach in the literature on programme evaluation. Intuitively, this approach involves matching each treated individual to 'statistical twins', i.e., non-treated individuals with similar observed characteristics X, such that differences in the outcomes of both groups can be attributed to the treatment. As suggested by Rosenbaum and Rubin (1983), we match on the propensity score $\mathrm{p(X) = P(D = 1|X)}$, i.e., the probability of being treated given X.

Following this approach, we apply dynamic propensity score matching to estimate the $\mathrm{ATT_{t,u}}$ of taking up ME on the probability of regular employment[5]. Propensity score matching identifies the $\mathrm{ATT_{t,u}}$ if two conditions are satisfied: the conditional independence assumption (CIA) and the common support condition (see e.g., Caliendo and Kopeinig 2008). For the case of dynamic matching, an adjusted dynamic version of the CIA (DCIA) can be stated as (Hujer/Thomsen 2010):

$$Y_{t,u}^{0} \amalg D_u | p(X_u), D_1 = \cdots = D_{u-1} = 0, \tag{3}$$

where $\amalg$ denotes independence. This means that conditional on the propensity score $\mathrm{p(X_u)}$ and not being treated up to time u, in the absence of treatment at u the treated would experience the same outcome as individuals from the control group. The common support condition is stated as $\mathrm{P(D = 1|X_u) < 1}$, which implies that non-treated matches for the treated must be available.

Because the DCIA assumption is fundamentally untestable, we must credibly argue why that assumption is likely to be valid for our application. On the one hand, in a dynamic matching approach, the DCIA is generally more likely to hold than the CIA in a static matching approach (Sianesi 2004). This is the case because the DCIA only must hold at the margin (taking up ME at u versus taking up ME later) and not, like the CIA, once and for all, i.e., taking up ME versus not taking up ME up to $\mathrm{u_{max}}$. Sianesi (2004) also notes that the dynamic matching approach reduces heterogeneity compared to the static approach because the current unemployment duration is controlled for, and this unemployment duration can be considered to capture further unobserved differences among individuals. However, the DCIA only holds if we

[5] Regular employment is defined as either full- or part-time employment subject to social security contributions.

observe all covariates X that jointly influence the participation decision at time u and the outcome variable where taking up ME is postponed further $(Y^0_{t,u})$, conditional on not taking up ME up to time u. However, for our application, we believe that the DCIA is likely to hold for the following reasons: First, because we focus on labour market outcomes, it is particularly important to employ variables on an individual's employment history for matching (see, e.g., Lechner 1999). Thus, we match on past labour market outcomes measured as the number of days spent in full-time work, part-time work, ME, apprenticeship, job search and active labour market programmes (ALMP) one year and five years before entering UB II, along with the number of days spent in unemployment before entering UB II. Second, to control for business cycle effects, we additionally match on the quarter of first entry into UB II. Finally we match on several socio-demographic variables, i.e., age at the time of entering UB II, nationality and qualification, and regional labour market conditions.

Furthermore, we restrict our estimation sample to persons who should only exhibit negligible differences in unobserved heterogeneity. Although this selection comes at the price of reducing (to an extent) the generalisability of our results, it increases the likelihood that the DCIA will hold in our analysis. We select the sample as follows. First, our sample consists of persons who received UB II for the first time between 2005 and 2006.[6] We confine the sample to this entry cohort because this allows us to observe UB-II recipients for as long as possible. Second, we restrict the sample to UB-II recipients who are actually at risk of transitioning into regular employment. Alternative roles and duties within the household context might influence the desired allocation of time between employment, housework and leisure time – particularly for women with children and care responsibilities (Becker 1965; Blau et al. 2001). Therefore, we only select childless singles at the time of entry into UB II who are actually looking for full-time employment. This selection is possible because our administrative data on UB-II receipt include household and job-search information, which was unavailable before the introduction of the Hartz IV reform.

Third, to focus on the core group of the labour force, we only consider persons who were 25 to 55 years old at the time of receiving UB II for the first time. Finally, because there remain remarkable differences in the labour market conditions between East and West Germany and because our database is less reliable for East Germany because of local labour agencies' high level of reporting failures in the first years after the introduction of UB II, we restrict our analysis to West Germany.

Our selection of the estimation sample should substantially reduce differences in motivations and restrictions related to job search. The selection leads to a sample size of 6,506 men and 2,669 women entering UB II for the first time between 2005 and 2006.

[6] Persons who received UB II in the first month of its introduction, January 2005, are excluded from the sample to avoid transitions from the pre-UB II system of social assistance.

After entering benefit receipt, we distinguish four states that individuals can enter: (i) searching (waiting), (ii) taking up ME, (iii) taking up regular employment, and (iv) entering an ALMP programme. After entering the benefit receipt, all individuals begin in state (i) and are at risk of entering any of the states (ii) to (iv) before the end of each period $u = u_1, u_2, \dots, u_{max}$. We regard the states from (ii) to (iv) as absorbing, i.e., we only consider the first spell of types (ii) to (iv) after entering benefit receipt and not the whole sequence of spells in the four possible states. Thus, in each period u, the group of the treated consists of all of the individuals who take up ME for the first time before the end of u, whereas their respective control group consists of all of the individuals who a) did not take up ME in u and b) did not enter states (iii) or (iv) before the end of u. We estimate the propensity scores for entering ME, $p(X_u)$, by a sequence of multinomial logit (ML) models for each period u. This approach can be considered equivalent to a discrete-time hazard-independent competing risk model (Voßemer/Schuck 2015), with all of the estimated parameters allowed to be duration specific (Sianesi 2004). We choose the maximum duration for transitioning to states (ii) to (iv) after entering benefit receipt, u_{max} as nine months. Individuals who are still searching for a job after nine months are considered as right censored. This choice results in 1,125 men and 627 women who first take up ME after entering benefit receipt and captures approximately 80 % of all individuals in the sample who first take up ME in the first two years after entering benefit receipt. Simultaneously, a u_{max} of nine months enables us to follow all of the individuals in our sample for 36 months after each period u. More precisely, we estimate the $ATT_{t,u}$ for $t = 6, 12, \dots, 36$ months after first entering ME at $u = u_1, \dots, u_{max}$. Our sample size does not allow us to estimate the $ATT_{t,u}$ for $u = 1, 2, \dots, 9$ months because this choice leads to a very low number of treated individuals in the later months. Instead, we choose periods $u_i, i = 1, \dots I$ of unequal length such that the share of the treated individuals is approximately equal in each period. As a robustness check, we choose both $I = 3$ and $I = 4$ periods.[7]

Given the validity of the DCIA and estimated propensity scores $p(X_u)$, the ATT can be consistently estimated by computing the mean of the difference of the outcomes for the group of treated individuals (or any subgroup thereof) and a (weighted) control group of non-treated individuals: $ATT_{t,u} = (1/N_{1,u}) \cdot \sum_{i=1}^{N_{1,u}} \left(Y_{1i,t} - \sum_{j=1}^{N_{0,u}} w_{ij,u} Y_{0j,t} \right)$, where $N_{0,u}$ and $N_{1,u}$ are the number of observations in the control group and the treatment group in period u, respectively, and $w_{ij,u}$ are the weights for the outcomes of the j-th individual of the control group used to estimate the ATT for the i-th treated individual. The size of the weights depends on the matching algorithm used.

[7] For $I = 3$, we choose the following periods measured in days after entering UB II: 0-40, 41-120, 121-270 days after entering benefit receipt. For the setting $I = 4$, we choose the periods 0-30, 31-70, 71-150, and 151-270 days after entering benefit receipt.

Asymptotically, all available matching algorithms – given the validity of the DCIA assumption – will lead to unbiased estimates of $\mathrm{ATT}_{\mathrm{t,u}}$. Moreover, there is no superior matching algorithm in finite samples. All of the available algorithms can instead be considered a trade-off between the bias and the variance of the estimated ATT (Caliendo/Kopeinig 2008). In this application, we first employ kernel matching (KM)[8] with the Epanechnikov kernel and a bandwidth of 0.02. We chose the bandwidth with the aim to approximately optimise the matching quality.[9]

Additionally, following Hujer and Thomsen (2010), we use 1-to-1 nearest neighbour (NN) matching without replacement. This latter matching technique allows us to apply the Mantel-Haenszel (1959) test (MH test) for the estimated ATTs' sensitivity to the presence of a possible hidden bias (Rosenbaum 2002). As discussed above, the DCIA only holds if unobserved characteristics are irrelevant to both the treatment and the outcome. Although we argue that our data set is rich enough to describe the individual's labour market situation, we believe that it is nonetheless informative to test the sensitivity of the estimated ATTs with respect to possible unobserved selection ("hidden bias"). For example, if unobserved factors that increase the probability of being treated also increase the probability of a positive outcome, ATTs would be overestimated. Intuitively, the MH test calculates bounds for the "amount" of hidden bias at which the inference about the treatment effect is altered. Thus, and crucially,

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8 For the matching, we use the programme "psmatch2" (Leuven/Sianesi 2003).

9 Varying the bandwidth between 0.005 and 0.03 does not have a substantial effect on the matching quality and the estimated ATTs.

the MH test cannot indicate whether a hidden bias is present; it merely shows how robust the estimated ATTs are with respect to a possible hidden bias.[10]

5 Results

Turning to the results of the analysis, we first show that our matching procedure can balance the treatment and control groups on our selected covariates[11]. Table 1 dis-plays several indicators for the quality of the kernel matching (KM) procedure for men and women and the different time intervals (the two specifications with four and three periods displayed separately). First, the results show that in the case of 4 (3) treatment periods, there are an average of approximately 220 (290) treated men and

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[10] Formally, the treatment probability of a treated individual is given by the propensity score $p_i = P(X_i, u_i) = P(D_i = 1|X_i, u_i) = F(\beta X_i + \gamma v_i)$, where v_i denotes an unobserved variable and γ measures the impact on that variable on the treatment probability. If there is no hidden bias, $\gamma = 0$ and the treatment will be solely determined by the observed variables X_i. Given a matched individual j with $X_i = X_j$ and assuming that $F(\cdot)$ is the logistic distribution, the odds ratio of treatment is given by

$$\frac{p_i(1-p_j)}{p_j(1-p_i)} = \frac{exp(\beta X_i + \gamma v_i)}{exp(\beta X_j + \gamma v_j)} = \exp(\gamma[v_i - v_j]).$$

Following Aakvik (2001) in assuming that the unobserved influence takes the form of a dummy, $v_\mathrm{i}, v_\mathrm{j} \in \{0,1\}$, Rosenbaum (2002) shows that the odds ratio is bounded by

$$\frac{1}{\exp(\gamma)} \leq \frac{p_i(1-p_j)}{p_j(1-p_i)} \leq \exp(\gamma).$$

In the case of no hidden bias, $\gamma = 0$, and both individuals have the same probability of being treated. However, if $\gamma \neq 0$, the odds of being treated could differ by a factor of at most $\exp(\gamma) \equiv \Gamma$. In this sense, Γ is a measure of the departure from the assumption of no hidden bias. Aakvik (2001) shows that the MH test can be used to test the null hypothesis of no treatment effect. The MH test statistic asymptotically follows a standard normal distribution and is given by

$$\mathrm{Q_{MH}} = \frac{|\mathrm{y_1} - \mathrm{E(y_1)}| - 0.5}{\sqrt{\mathrm{Var(y_1)}}},$$

with $\mathrm{E(y_1) = (n_1 y)/n}$ and $\mathrm{Var(y_1) = [n_1 n_0 y(n-y)]/[n^2(n-1)]}$, where $\mathrm{n_1}$ ($\mathrm{n_0}$) denotes the number of treated (matched non-treated), and $\mathrm{y_1}$ ($\mathrm{y_0}$) is the number treated (matched non-treated) individuals with a successful outcome. Finally, $\mathrm{n = n_0 + n_1}$ and $\mathrm{y = y_0 + y_1}$. Rosenbaum (2002) shows that the MH test statistic can be bounded by two statistics $\mathrm{Q_{MH}^{+}}$ and $\mathrm{Q_{MH}^{-}}$, given by

$$\mathrm{Q_{MH}^{+}} = \frac{|\mathrm{y_1} - \tilde{\mathrm{E}}^{+}| - 0.5}{\sqrt{\mathrm{Var}(\tilde{\mathrm{E}}^{+})}} \leq \mathrm{Q_{MH}} \leq \mathrm{Q_{MH}^{-}} = \frac{|\mathrm{y_1} - \tilde{\mathrm{E}}^{-}| - 0.5}{\sqrt{\mathrm{Var}(\tilde{\mathrm{E}}^{-})}},$$

where $\tilde{\mathrm{E}}$ and $\mathrm{Var}(\tilde{\mathrm{E}})$ are the large-sample approximation to the expectation and variance of the number of treated persons with a successful outcome when v is binary and for a given γ. The statistics coincide for $\Gamma = 1$ and move apart for increasing Γ. If, for example, the estimated ATT is positive and significant under the assumption of no hidden bias ($\Gamma = 1$), the robustness of the ATT can be tested by increasing Γ (starting with 1) in small increments and finding the Γ at which the statistic $\mathrm{Q_{MH}^{+}}$ becomes insignificant. On the other hand, in case of a significantly negative ATT, Γ is increased until the statistic $\mathrm{Q_{MH}^{-}}$ becomes insignificant. The higher Γ can be increased without altering inference about $\mathrm{Q_{MH}^{+}}$ or $\mathrm{Q_{MH}^{-}}$, the more robust is the estimated ATT with respect to hidden bias. We apply the MH test with the programme “mhbounds” by Becker and Caliendo (2007).

[11] Selected results of the propensity score estimations using multinomial logit models are presented in appendix Tables A.1 and A.2.

130 (170) treated women in each treatment period. Almost no individuals are lost because of the common support condition. The KM procedure can reduce bias in every single case to a sufficient degree, with a median bias of well below three after matching (Caliendo/Kopeinig 2008), and a Pseudo-R^2 at almost zero. The results for 1-to-1 nearest-neighbour matching without replacement (NN) (Table 2) show less favourable – but still reasonably good – values. The median bias is reduced quite substantially, and the Pseudo-R^2 values are also reduced in all cases except for the two latest periods for women. In these two cases, the inspection of covariate-specific bias (not displayed here) shows that there is only one case in which one covariate is still significantly different between the treated and control groups after matching.

Given the good matching quality – particularly for KM – we can now turn to the results concerning the effects on regular employment. As described above, we measured the share of workers in regular employment every six months following their respective treatment period for up to 36 months. In Tables 3 and 4, KM results are reported not only for men and women but also for the 3- and 4-period specifications of u. We find a unanimous pattern in all groups: There are substantial treatment effects for the unemployed who enter ME in the later treatment periods, whereas there are almost no highly statistically significant effects for the unemployed taking up ME in the first searching period, that is, within the first 30 or 40 days, respectively. For the last searching period, we find statistically highly significant (1 % level) treatment effects for all outcome periods except one, for which the significance level is 5 %. This holds for both men and women, and the effects are of a substantial magnitude of approximately 10 to 20 percentage points. For example, for men in the 4-period case taking up ME after at least 150 days (5 months) spent in unemployment and UB-II receipt, the probability of being regularly employed 30 months after the treatment is 14 percentage points higher than for men who did not take up ME until the end of the at-risk-period (270 days after entering UB-II receipt). For women, the treatment effect is even larger, at 20 percentage points.

Table 1
Overview on Matching Quality – Kernel Matching

Men							
u[a] : 4 Periods	No. of treated before	No. of non-treated before	Median bias before[b]	Median bias after[b]	Probit ps-R^2 before[c]	Probit ps-R^2 after[c]	CS[d]
1: 0-30 days	225	5758	8.7	1.2	0.083	0.004	1
2: 31-70 days	206	4762	8.1	1.7	0.080	0.003	2
3: 71-150 days	236	3526	9	1.2	0.058	0.002	0
4: 151-270 days	209	2387	6.7	1.6	0.055	0.004	1
u[a] : 3 Periods							
1: 0-40 days	303	5435	8.3	1.2	0.076	0.003	1
2: 41-120 days	292	3928	9.6	2.2	0.070	0.003	0
3: 121-270 days	281	2387	5.6	2	0.061	0.005	1
Women							
u[a] : 4 Periods	No. of treated before	No. of non-treated before	Median bias before[b]	Median bias after[b]	Probit ps-R^2 before[c]	Probit ps-R^2 after[c]	CS[d]
1: 0-30 days	130	2326	10.8	1.1	0.089	0.003	0
2: 31-70 days	143	1864	11.7	2.7	0.110	0.005	2
3: 71-150 days	126	1345	10.6	1.9	0.084	0.004	1
4: 151-270 days	113	890	10.6	1.6	0.061	0.006	0
u[a] : 3 Periods							
1: 0-40 days	182	2180	10	1.2	0.103	0.003	0
2: 41-120 days	170	1517	9.7	2.1	0.088	0.003	0
3: 121-270 days	160	890	11.9	2.2	0.060	0.008	0

[a] Period u refers to the time (in days) at which the treatment starts in the individual UB-II spell.

[b] Median bias denotes the median of the standardised difference in percent following Rosenbaum and Rubin (1985) before and after matching.

[c] Probit ps-R^2 refers to the pseudo R^2 computed for the full sample (before) and the matched sample (after).

[d] Number of treated individuals lost after imposing the common support condition.

Table 2
Overview on Matching Quality – 1-to-1 Nearest Neighbour Matching

Men							
u[a] : 4 Periods	No. of treated before	No. of non-treated before	Median bias before[b]	Median bias after[b]	Probit ps-R^2 before[c]	Probit ps-R^2 after[c]	CS[d]
1: 0-30 days	225	5758	8.7	5.5	0.083	0.039	10
2: 31-70 days	206	4762	8.1	4.2	0.08	0.035	9
3: 71-150 days	236	3526	9	3.9	0.058	0.02	8
4: 151-270 days	209	2387	6.7	5.9	0.055	0.031	13
u[a] : 3 Periods							
1: 0-40 days	303	5435	8.3	3.4	0.076	0.016	14
2: 41-120 days	292	3928	9.6	3.2	0.070	0.022	12
3: 121-270 days	281	2387	5.6	4.2	0.061	0.028	21
Women							
u[a] : 4 Periods	No. of treated before	No. of non-treated before	Median bias before[b]	Median bias after[b]	Probit ps-R^2 before[c]	Probit ps-R^2 after[c]	CS[d]
1: 0-30 days	130	2326	10.8	5.3	0.089	0.053	9
2: 31-70 days	143	1864	11.7	3.7	0.110	0.039	14
3: 71-150 days	126	1345	10.6	3.2	0.084	0.052	10
4: 151-270 days	113	890	10.6	7	0.061	0.062	12
u[a] : 3 Periods							
1: 0-40 days	182	2180	10.0	3.4	0.103	0.024	18
2: 41-120 days	170	1517	9.7	6.4	0.088	0.041	19
3: 121-270 days	160	890	11.9	7.6	0.060	0.062	16

[a] Period u refers to the time (in days) at which the treatment starts in the individual UB-II spell.

[b] Median bias denotes the median of the standardised difference in percent following Rosenbaum and Rubin (1985) before and after matching.

[c] Probit ps-R2 refers to the pseudo R^2 computed for the full sample (before) and the matched sample (after).

[d] Number of treated individuals lost after imposing the common support condition.

A very similar picture emerges in the NN case (Tables 5 and 6). Here, too, we find comparatively small and mostly insignificant treatment effects for the first searching periods of up to 30 or 40 days. In the later periods of unemployment durations – particularly after 150 days – there again are significant and large effects of between 10 and (in some cases) 25 percentage points. Compared to the KM results, the NN estimates do not reach the highest significance levels in as many cases for the group of women in the 4th period. This might be partially explained by the fact that we lose more treated observations for NN-matching than for KM because of the common support assumption. Because the results obtained with two matching estimators at opposite extremes of the bias-variance trade-off (Caliendo/Kopeinig 2008) are highly similar, it is safe to assume that the general pattern of effects holds, not at least because there are clear-cut explanations from a substantial perspective.

Finally, the general dynamic pattern of the treatment effects of our results is consistent with our theoretical discussion (see Section 3) of the “stepping stone” function of ME because we argued that taking up ME later in the unemployment spell should likely have a more substantial effect on subsequent regular employment than does taking up ME after a relatively short period of unemployment.

Table 3
Average Treatment Effects (ATTs) of ME on Regular Employment – Kernel Matching – 4 Periods

Men		Outcome Period					
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-30 days	225	0.046	***0.060***	**0.065**	*0.067*	**0.064**	0.048
2: 31-70 days	206	0.048	***0.067***	0.034	0.033	**0.066**	0.065
3: 71-150 days	236	***0.119***	***0.083***	***0.141***	***0.111***	***0.100***	***0.105***
4: 151-270 days	209	***0.100***	***0.152***	***0.186***	***0.135***	***0.139***	***0.125***
Women							
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-30 days	130	**0.056**	**0.063**	0.029	0.052	**0.043**	0.011
2: 31-70 days	143	0.060	*0.096*	***0.131***	***0.125***	*0.063*	0.028
3: 71-150 days	126	*0.099*	***0.149***	***0.106***	***0.145***	***0.159***	***0.169***
4: 151-270 days	113	**0.111**	***0.165***	***0.146***	***0.123***	***0.197***	***0.175***

Bold and italic values indicate significance at the 1 % level, bold values refer to the 5 % level and italic values refer to the 10 % level. For the kernel estimates bootstrapped and bias-corrected standard errors are used.

[a] Period u refers to the time (in days) at which the treatment starts in the individual UB-II spell.

[b] Treated refers to the number of treated observations when using kernel matching. Common support is imposed by the minimum-maximum comparison.

Table 4
Average Treatment Effects (ATTs) of ME on Regular Employment – Kernel Matching – 3 Periods

Men		Outcome Period					
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-40 days	303	0.047	***0.064***	**0.072**	***0.057***	**0.070**	0.043
2: 41-120 days	292	***0.135***	***0.084***	***0.114***	***0.144***	***0.111***	***0.162***
3: 121-270 days	281	***0.090***	***0.123***	***0.156***	***0.111***	***0.132***	***0.131***
Women							
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-40 days	182	*0.066*	***0.109***	0.074	***0.092***	***0.094***	0.024
2: 41-120 days	170	*0.105*	0.102	***0.127***	*0.085*	***0.110***	***0.128***
3: 121-270 days	160	***0.140***	***0.183***	***0.187***	***0.168***	***0.215***	***0.198***

Bold and italic values indicate significance at the 1 % level, bold values refer to the 5 % level and italic values refer to the 10 % level. For the kernel estimates bootstrapped and bias-corrected standard errors are used.

[a] Period u refers to the time (in days) at which the treatment starts in the individual UB-II spell.

[b] Treated refers to the number of treated observations when using kernel matching. Common support is imposed by the minimum-maximum comparison.

Table 5
Average Treatment Effects (ATTs) of ME on Regular Employment – 1-to-1 Nearest Neighbour Matching – 4 Periods

Men		Outcome Period					
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-30 days	215	0.042	0.019	0.037	0.033	0.047	0.051
2: 31-70 days	197	0.066	0.066	0.051	0.046	**0.107**	**0.107**
3: 71-150 days	228	***0.110***	**0.101**	***0.140***	***0.158***	***0.149***	***0.162***
4: 151-270 days	96	***0.107***	***0.148***	***0.204***	***0.143***	***0.173***	***0.128***
Women							
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-30 days	121	0.066	**0.124**	0.050	0.099	0.091	0.041
2: 31-70 days	129	0.039	0.047	*0.116*	0.101	0.070	0.000
3: 71-150 days	116	0.060	**0.147**	*0.112*	**0.155**	0.103	0.103
4: 151-270 days	101	**0.109**	***0.178***	**0.139**	*0.109*	***0.228***	***0.228***

Bold and italic values indicate significance at the 1 % level, bold values refer to the 5 % level and italic values refer to the 10 % level. For the kernel estimates, bootstrapped and bias-corrected standard errors are used.

[a] Period u refers to the time (in days) at which the treatment starts in the individual UB-II spell.

[b] Treated refers to the number of treated observations when using 1-to-1 nearest-neighbour matching without replacement. Common support is imposed by the minimum-maximum comparison.

Table 6
Average Treatment Effects (ATTs) of ME on Regular Employment – 1-to-1 Nearest Neighbour Matching – 3 Periods

Men		Outcome Period					
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-40 days	289	0.055	**0.087**	***0.114***	0.048	*0.073*	0.028
2: 41-120 days	280	***0.132***	0.039	0.064	***0.121***	*0.075*	***0.146***
3: 121-270 days	260	***0.104***	***0.131***	***0.150***	**0.100**	***0.131***	***0.146***
Women							
u[a]	Treated[b]	$\Delta_{6,u}$	$\Delta_{12,u}$	$\Delta_{18,u}$	$\Delta_{24,u}$	$\Delta_{30,u}$	$\Delta_{36,u}$
1: 0-40 days	164	0.030	0.079	0.043	0.043	0.104	0.067
2: 41-120 days	151	*0.093*	***0.146***	**0.139**	0.079	0.053	0.086
3: 121-270 days	144	***0.146***	***0.208***	***0.229***	***0.194***	***0.243***	***0.250***

Bold and italic values indicate significance at the 1 % level, bold values refer to the 5 % level and italic values refer to the 10 % level. For the kernel estimates, bootstrapped and bias-corrected standard errors are used.

[a] Period u refers to the time (in days) at which the treatment starts in the individual UB-II spell.

[b] Treated refers to the number of treated observations when using 1-to-1 nearest-neighbour matching without replacement. Common support is imposed by the minimum-maximum comparison.

6 Conclusion

In this paper we analysed whether ME can improve the chances of regular employment for a special group of unemployed benefit recipients and applied a dynamic matching approach that is state of the art but has not yet been used for the ME analysis. The results presented here have shown the appropriateness of the dynamic evaluation approach when analysing the employment effects of ME on unemployed men and women. We find a clear-cut trend of substantial employment effects for unemployed men and women who remain unemployed after entering UB-II receipt for at least four or five months, whereas there are no unambiguous effects for individuals with shorter unemployment durations. These results are more or less in accordance with existing empirical evidence of the positive effects of ME for either unemployed individuals with higher unemployment duration (Caliendo et al. 2012) or a sample of long-term unemployed (Lehmer 2012). The analysis used newly available data for UB-II recipients, thus enabling us to focus on a special group of unemployed men and women who should be equally likely to profit from ME. We restricted our analysis to the single and childless unemployed who are searching for full-time employment. Therefore, there should be no factors that could inhibit transitions to regular employment, such as a missing wish for extended employment, and restricting factors, such as family obligations. This construction of the sample made it possible to analyse women, who often have been excluded from the analysis because of the impossibility of controlling for household context and job-search activities with administrative data. Our results show that single and childless women can profit from ME to a similar extent as men.

The generalisability of our results is somewhat limited by our sample selection. Whereas we find evidence that ME can improve chances for regular employment for single unemployed individuals with longer unemployment durations, we cannot draw conclusions about individuals living in family households. Thus, a question for further research could be to compare results for the single unemployed with unemployed individuals in families who (in addition to job search) must balance work and family life as well. Nonetheless, our findings are highly relevant to the discussion of whether it is desirable for unemployed recipients of UB II to take up ME versus continue to search for a job in the hope of finding regular employment: singles constitute the clear majority among both the recipients of UB II and the marginally employed recipients of UB II.

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Appendix

Table A.1
Discrete-time hazard competing risk duration model – Men – 4-Period-Model – u: 0-30 days

	Marginal Employment		Regular Employment		ALMP	
	Coef.	P>|z|	Coef.	P>|z|	Coef.	P>|z|
Time of entry (Ref.: 1st quarter 2005)						
2nd quarter 2005	-0.116	0.633	0.485	0.051	0.256	0.221
3rd quarter 2005	-0.027	0.914	-0.039	0.890	0.067	0.762
4th quarter 2005	0.062	0.806	0.191	0.501	0.298	0.201
1st quarter 2006	0.048	0.845	0.076	0.786	-0.241	0.347
2nd quarter 2006	0.299	0.272	0.614	0.029	-0.010	0.972
3rd quarter 2006	-0.170	0.605	0.120	0.717	0.336	0.224
4th quarter 2006	-0.135	0.689	-0.010	0.976	0.480	0.083
Qualification (Ref.: high)						
Low	0.692	0.025	-0.535	0.102	0.067	0.791
Medium	0.495	0.070	0.123	0.599	0.038	0.853
Age	-0.014	0.154	-0.026	0.011	-0.008	0.371
Nationality (Ref.: German)						
Not German	0.346	0.040	0.030	0.878	-0.164	0.358
Past (un-) employment experience (days)						
Duration of unemployment prior to UB-II entry	0.000	0.963	-0.001	0.388	0.002	0.013
Duration of unemployment prior to UB-II entry squared	0.000	0.773	0.000	0.607	0.000	0.022
Duration of full-time employment last year	0.003	0.018	0.002	0.135	0.002	0.165
Duration of part-time employment last year	0.009	0.000	0.005	0.004	-0.001	0.830
Duration of marginal employment last year	0.008	0.000	-0.001	0.730	0.002	0.343
Duration of training participation last year	0.004	0.023	0.008	0.000	0.005	0.002
Duration of job search w/o unemployment last year	0.002	0.591	0.006	0.018	0.004	0.135
Duration of job search with unemployment last year	0.001	0.359	0.001	0.308	0.003	0.021
Duration of ALMP participation last year	-0.001	0.784	0.000	0.812	0.011	0.000

	Marginal Employment		Regular Employment		ALMP	
	Coef.	P>|z|	Coef.	P>|z|	Coef.	P>|z|
Duration in other states last year	0.001	0.719	-0.003	0.455	-0.006	0.228
Duration of full-time employment last 5 years	0.000	0.108	0.000	0.045	0.000	0.862
Duration of part-time employment last 5 years	0.000	0.516	0.000	0.867	0.000	0.500
Duration of marginal employment last 5 years	0.001	0.061	0.000	0.415	0.001	0.110
Duration of Training participation last 5 years	0.000	0.963	-0.001	0.078	0.000	0.883
Duration of job search w/o unemployment last 5 years	-0.003	0.189	-0.002	0.210	0.000	0.759
Duration of job search with unemployment last 5 years	0.000	0.712	0.000	0.932	0.000	0.084
Duration of ALMP participation last 5 years	0.000	0.447	0.000	0.808	0.000	0.598
Duration in other states last 5 years	0.000	0.961	0.001	0.463	0.001	0.600
Regional unemployment rate	-0.019	0.308	-0.063	0.001	0.004	0.789
Constant	-4.242	0.000	-2.604	0.000	-4.628	0.000
Pseudo R-Square	0.1031					
Log Likelihood	-2786.7223					
N	6498					

Table A.2
Discrete-time hazard competing risk duration model – Women – 4-Period-Model – u: 0-30 days

	Marginal Employment		Regular Employment		ALMP	
	Coef.	P>|z|	Coef.	P>|z|	Coef.	P>|z|
Time of entry (Ref.: 1st quarter 2005)						
2nd quarter 2005	0.087	0.798	0.782	0.111	0.825	0.055
3rd quarter 2005	-0.222	0.545	0.848	0.080	0.664	0.133
4th quarter 2005	0.154	0.660	0.796	0.114	0.004	0.994
1st quarter 2006	-0.119	0.740	0.617	0.224	0.172	0.724
2nd quarter 2006	0.361	0.326	1.293	0.009	1.139	0.016
3rd quarter 2006	0.277	0.479	1.020	0.054	1.034	0.034
4th quarter 2006	-0.327	0.517	0.474	0.433	1.137	0.024
Qualification (Ref.: high)						
Low	0.666	0.044	-0.812	0.065	-0.913	0.056
Medium	0.290	0.280	-0.386	0.129	-0.272	0.305
Age	0.013	0.236	-0.021	0.152	0.000	0.997
Nationality (Ref.: German)						
Not German	-0.188	0.515	-0.287	0.434	0.005	0.990
Past (un-) employment experience (days)						
Duration of unemployment prior to UB-II entry	-0.003	0.228	-0.001	0.715	0.001	0.721
Duration of unemployment prior to UB-II entry squared	0.000	0.746	0.000	0.960	0.000	0.913
Duration of full-time employment last year	0.002	0.274	0.001	0.541	0.002	0.485
Duration of part-time employment last year	0.000	0.906	0.003	0.133	-0.005	0.219
Duration of marginal employment last year	0.008	0.000	0.001	0.596	0.002	0.397
Duration of training participation last year	0.004	0.099	0.005	0.010	0.003	0.347
Duration of job search w/o unemployment last year	-0.003	0.323	-0.001	0.859	0.014	0.021
Duration of job search with unemployment last year	0.001	0.447	-0.001	0.694	0.003	0.273
Duration of ALMP participation last year	0.001	0.706	-0.002	0.552	0.012	0.000
Duration in other states last year	0.006	0.408	-0.004	0.570	-0.001	0.820

	Marginal Employment		Regular Employment		ALMP	
	Coef.	P>|z|	Coef.	P>|z|	Coef.	P>|z|
Duration of full-time employment last 5 years	0.000	0.753	0.000	0.162	0.001	0.250
Duration of part-time employment last 5 years	0.000	0.179	0.000	0.928	0.001	0.009
Duration of marginal employment last 5 years	-0.001	0.101	0.000	0.501	0.001	0.147
Duration of Training participation last 5 years	-0.001	0.069	0.000	0.335	0.001	0.171
Duration of job search w/o unemployment last 5 years	0.002	0.116	0.002	0.103	-0.008	0.109
Duration of job search with unemployment last 5 years	0.000	0.819	0.001	0.188	0.001	0.086
Duration of ALMP participation last 5 years	0.001	0.090	0.000	0.907	0.000	0.938
Duration in other states last 5 years	-0.005	0.353	0.002	0.333	0.004	0.010
Regional unemployment rate	-0.039	0.123	-0.063	0.031	-0.063	0.028
Constant	-3.350	0.000	-2.769	0.001	-5.092	0.000
Pseudo R-Square	0.1173					
Log Likelihood	-1214.7598					
N	2662					

Table A.3
Sensitivity of the estimates to possible hidden bias – Mantel-Haenszel Test

Men	$\Delta_{30,u}$				$\Delta_{36,u}$			
4 Periods	Q_{MH} $\Gamma=1$	Value[a] of Γ	Bounds for Q_{MH} Q^{+}_{MH}	Q^{-}_{MH}	Q_{MH} $\Gamma=1$	Value[a] of Γ	Bounds for Q_{MH} Q^{+}_{MH}	Q^{-}_{MH}
1: 0-30 days	n.s.				n.s.			
2: 31-70 days	2.452	1.25	1.368	3.551	2.461	1.30	1.382	3.556
3: 71-150 days	3.840	1.60	1.422	6.326	4.058	1.70	1.353	6.858
4: 151-270 days	3.497	1.60	1.311	5.754	2.557	1.30	1.337	3.798
3 Periods								
1: 0-40 days	n.s.				n.s.			
2: 41-120 days	2.975	1.30	1.441	4.527	2.206	1.15	1.386	3.032
3: 121-270 days	3.055	1.35	1.449	4.691	3.456	1.50	1.310	5.657
Women	$\Delta_{30.u}$				$\Delta_{36.u}$			
4 Periods	Q_{MH} $\Gamma=1$	Value[a] of Γ	Bounds for Q_{MH} Q^{+}_{MH}	Q^{-}_{MH}	Q_{MH} $\Gamma=1$	Value[a] of Γ	Bounds for Q_{MH} Q^{+}_{MH}	Q^{-}_{MH}
1: 0-30 days	n.s.				n.s.			
2: 31-70 days	n.s.				n.s.			
3: 71-150 days	1.321	1.00			n.s.			
4: 151-270 days	3.305	1.85	1.315	5.422	3.256	1.80	1.320	5.301
3 Periods								
1: 0-40 days	n.s.				1.998	1.15	1.374	2.631
2: 41-120 days	n.s.				1.387	1.00		
3: 121-270 days	4.211	2.05	1.301	7.326	4.305	2.15	1.282	7.543

[a] $\Gamma = 1$ denotes the case of no hidden bias. The higher Γ can be increased without altering inference about Q^{+}_{MH} or Q^{-}_{MH}, the more robust is the estimated ATT with respect to hidden bias. n.s. denotes insignificant treatment effects.

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Ricardo Martinez Moya, Jutta Palm-Nowak

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For further inquiries contact the authors:

Torsten Lietzmann
Phone +49.911.179 4516
E-mail torsten.lietzmann@iab.de

Jürgen Wiemers
Phone +49.911.179.8671
E-mail juergen.wiemers@iab.de