published in The Economic Journal, 2010

A CASEWORKER LIKE ME - DOES THE SIMILARITY BETWEEN THE UNEMPLOYED AND THEIR CASEWORKERS INCREASE JOB PLACEMENTS? *

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This version: January 2010

Date this version has been printed: 29 January 2010

Abstract

This paper examines whether the chances of job placements improve if unemployed persons are counselled by caseworkers who belong to the same social group, defined by gender, age, education, and nationality. Based on an unusually informative dataset, which links Swiss unemployed to their caseworkers, we find positive employment effects of about 3 percentage points if the caseworker and his unemployed client belong to the same social group. Coincidence in a single characteristic, e.g. same gender of caseworker and unemployed, does not lead to detectable effects on employment. These results, obtained by statistical matching methods, are confirmed by several robustness checks.

Keywords: Social identity, social interactions, public employment services, unemployment, gender, age, education, treatment effects, matching estimators.

JEL classification: J64, J68, C31

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Most research on the determinants of unemployment durations has focused either on institutional aspects of the unemployment insurance system (e.g., Abbring *et al.*, 2005; Dorsett, 2006; Fredriksson and Holmlund, 2001; Lalive, 2008; Lalive *et al.*, 2005, 2006; Svarer, 2007; van den Berg *et al.*, 2004; Wunsch, 2005, 2007), effects of active labour market programmes (e.g., Heckman *et al.*; 1999; Brodaty *et al.*, 2001; Gerfin and Lechner, 2002; Larsson 2003) or characteristics of the employment offices (Bloom *et al.*, 2003; Sheldon, 2003). The personal relationship between the unemployed person and his caseworker in the employment office might also be an important, though much less researched, determinant. In this paper, we examine whether *similarity* (in several characteristics) between the unemployed person and his caseworker affects re-employment probabilities. We find a positive employment effect of about 3 percentage points when the caseworker and the unemployed person are of the same gender, age, nationality, and educational background. An interesting finding is that same gender, age, or education alone does *not* lead to positive effects, though.

So far, the effects of similarity between the unemployed persons and their caseworkers have not been researched, presumably due to the absence of informative linked caseworker-client datasets. In this paper, we combine administrative data on the population of all unemployed persons in Switzerland with survey data on their corresponding caseworkers. This combined dataset contains information on gender, age, nationality, and education for unemployed persons *and* for caseworkers. Several additional variables are available for the unemployed, the caseworker, and the employment office to control for potentially confounding factors. We define a caseworker to be similar to his client, if he/she has the same nationality, gender, the same educational level, and a similar age. Using matching, logit regressions and fixed effects estimators, we find positive employment effects of 3 percentage points when having a caseworker who is similar in all these dimensions. Similarity in fewer characteristics leads to zero effects. These results indicate that simply assigning female clients to female caseworkers and male clients to male caseworkers is insufficient to obtain advantages from selective assignment of unemployed to caseworkers. A larger degree of similarity is required. Regarding policy conclusions, these findings suggest that a deliberate allocation of the unemployed to caseworkers could enhance employment outcomes. The relationship between the unemployed person and his caseworker matters and a similar social background can enhance it.

In Section 1, we provide some background on social interactions between similar individuals and the key features of the public employment system in Switzerland. Section 2 describes the databases and provides a descriptive analysis. Section 3 discusses the econometric identification strategy and the estimation methodology. Section 4 presents the results and Section 5 concludes.

1 Background

1.1 Social interactions, social identity, and similarity

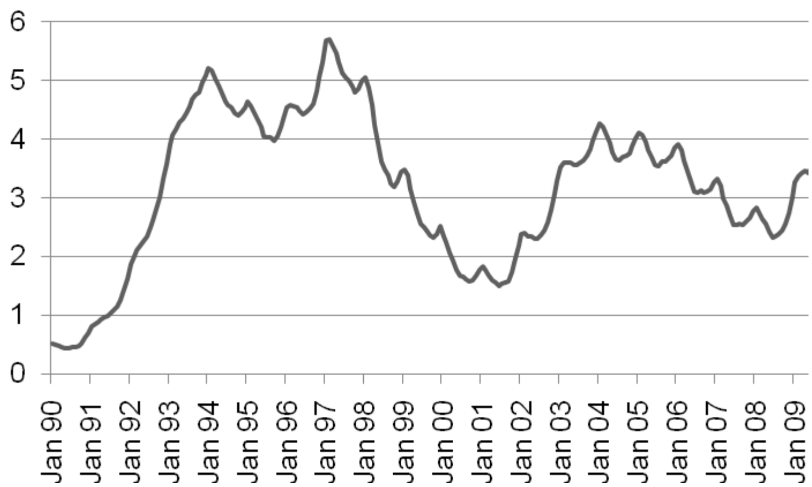
The effects of social interactions, social identity, and similarity have been examined in various disciplines, including economics, pedagogy, and sociology. Sociologists have introduced the concept of *social identity* (Sherif *et al.*, 1961; Tajfel, 1970; Tajfel and Turner, 1979; Brewer, 1979), which argues that the mere perception of belonging to distinct groups is sufficient to trigger intergroup discrimination favouring the in-group, at the expense of the out-group. Results also indicate that explicit *similarity* within in-group members, e.g. in ethnicity, increases the in-group bias. The educational sciences have also devoted substantial attention to the possible interaction effect between teachers' and students' ethnicity or gender. There is evidence for positive effects of having the same ethnicity (Dee, 2004; Lindahl, 2007) and mixed evidence for having a same gender teacher, where some find positive effects (Neumark and Gardecki, 1998; Bettinger and Long, 2005; Dee, 2007; Lindahl, 2007) and others find insignificant effects (Holmlund and Sund, 2005; Hilmer and Hilmer, 2007; Hoffmann and Oreopoulos, 2007). A related literature examines *trust*, *fairness,* and *gift-exchange*. It is likely that individuals with a similar background may either naturally trust themselves more or are more efficient in developing an effective gift-exchange relationship to their mutual benefit. Gächter and Thöni (2005) find higher levels of cooperation if all participants knew that all other group members are "like-minded people", in that they had a similar preference towards cooperation. Similarly, Giuliano *et al.* (2006) find that demographic differences between managers and their subordinates adversely affect quit, dismissal and promotion rates.

In our particular application, various channels might be at work, which could explain the positive effects found, in particular communication and trust: A similar social background can improve the efficiency of *communication*, or information exchange, between caseworker and unemployed, because people with similar backgrounds use nonverbal and verbal concepts that are more similar (Hyde, 2005). This may enhance the caseworker's understanding of the labour market prospects of the unemployed. Thus, it may help him to identify useful job search strategies and active measures and to be more effective in counselling. Similarity may also induce more *trust* and commitment in the relationship of the caseworker and his unemployed client. The unemployed person may be more willing to report his job search activities truthfully, and the caseworker may be more willing to give truthful advice about duties and rights of the unemployed person. This could also stimulate a *gift-exchange relationship* where the unemployed person is more willing to apply job search effort and to accept a job, instead of rejecting job offers in order to continue living on unemployment benefits. Such kind of self-enacted cooperation may act as a substitute for strong legal sanctions (Tyran and Feld, 2006) and solve the agency problem between caseworker and client, as it is, e.g., often difficult for the caseworker to prove factually that an unemployed person displayed insufficient search effort.

1.2 The public employment services in Switzerland

Until the recession of the early 1990s, unemployment was extremely low in Switzerland, a small country with 26 different administrative regions, called *cantons*. As shown in Figure 1, with the recession the unemployment rate rose rapidly to 5% and triggered a comprehensive revision of the federal unemployment insurance act in 1996/1997. The municipal employment offices were consolidated to around 100 regional employment offices (REO). With the exception of the canton of Geneva (and one particular employment office in the canton of Solothurn), all regional employment offices were geographically organised in 2003. This means that each employment office is responsible for a particular region, and all persons becoming unemployed have to register with the employment office where they live. We will use only the geographically organised offices for our empirical analysis.

Fig. 1: Unemployment rate in Switzerland (January 1990 - September 2009)



Note: Monthly unemployment rate in %, January 1990 - September 2009, Source: Swiss National Bank Monatshefte.

1.3 Allocation of the unemployed to caseworkers

When a person becomes unemployed, he/she registers at the nearest employment office. The first meeting usually takes place shortly thereafter with a secretarial staff member to collect basic information and to request additional documents from the unemployed person, e.g. employer certificates. The unemployed person is then often sent to a one-day workshop to inform him about the unemployment law, obligations and rights, job search requirements, etc. The first meeting with a caseworker usually takes place within the first two months of unemployment. In a survey, described in Section 2, caseworkers and office managers were asked about the criteria used for the allocation of unemployed persons to caseworkers. The most important criteria are by occupation group (55%), by industry sector (50%), and by caseload (43%).[1] Other criteria are at random (24%), by region (10%), and by employability (7%). By age (3%) and by name (via alphabet, 4%) of the unemployed person are rarely mentioned. With the option "other" (10%), caseworkers could also give fill-in answers.

Having been allocated to a caseworker, the unemployed person meets with his caseworker about once a month for a consultative meeting. Usually the same caseworker remains in charge for the entire unemployment spell. There are two exceptions: (i) A number of employment offices enact a policy where a caseworker change takes place automatically every 8 to 12 months, or on request of the caseworker, to initiate new ideas in the counselling process. (ii) Very rarely, an unemployed person requests a caseworker change for personal reasons. To avoid any concerns about endogenous caseworker changes, we focus entirely on the first caseworker in an unemployment spell.

During the unemployment spell, benefits are paid according to federal law.[2] Benefits amount to 70-80% of the former salary depending on age, dependent persons (children) and salary. The maximum benefit entitlement period is 24 months. In July 2003, the rules for benefit entitlement were tightened for individuals younger than 55: the minimum contribution time was raised from 6 to 12 months and the maximum benefit entitlement period was reduced to 18.5 months. These regulations are set by federal law and do not depend on the region, employment office or the caseworker. Hence, conditional on the characteristics of the unemployed, the *similarity* status, defined below, has no bearing on the level of unemployment benefits or the entitlement period. (It could only affect monetary job search incentives via the imposition of sanctions, as discussed in Section 4.2.)

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[1] These answers sum up to more than 100% since multiple answers were permitted.

[2] Registration at an employment office is a pre-condition for receiving unemployment benefits.

2 Data

2.1 Data sources and sample selection

The population for our analysis consists of all individuals who registered as unemployed anytime during the year 2003. Their outcomes are followed until the end of 2006. For these individuals very detailed information from the databases of the unemployment insurance system (AVAM/ASAL) and the social security records (AHV) is available. These data sources contain socio-economic characteristics including nationality and type of work permit, qualification, education, language skills (mother tongue, proficiency of foreign languages), experience, profession, position, and industry of last job, occupation and industry of desired job and an employability rating by the caseworker.[3] The data also contains detailed information on registration and de-registration, benefit payments and sanctions, participation in ALMP, and the employment histories from January 1990 with monthly information on earnings and employment status (employed, unemployed, non-employed, self-employed). We further complemented this data with local and regional information from the national statistical yearbooks, e.g. cantonal and industry unemployment rates and vacancies.

We link each newly registered unemployed person in 2003 to his first caseworker by exploiting the information from the so-called "user database" of the employment offices. This database contains basic information about each caseworker, such as age etc. In order to complement this information we conducted an extensive survey of all caseworkers. A written questionnaire was sent to all caseworkers and employment office managers who were employed at an employment office between 2001 and 2003 and were still active at the time the questionnaire was sent (December 2004). The

[3] This employability rating is a subjective judgement done by the caseworker on a scale from 1 to 5. The categories "medium employability" and "difficult to employ" further distinguish between individuals with or without need for training. The employability rating is highly correlated with the formal qualifications and skills of the unemployed, but caseworkers also take personality factors (e.g. alcohol abuse, pregnancy), language skills, family issues, mobility, business cycles, and vacancy numbers in the industry and occupation of the unemployed person into account.

questionnaire contained questions about caseworker's characteristics, the aims and strategies of the caseworker and the employment office and about the processes and the organisation of the latter (for details, see Frölich *et al.* (2007)).

In total, 239,004 persons registered as newly unemployed during the year 2003. We exclude unemployed with missing caseworker information or missing information on age, gender, education, or nationality. We restrict our analysis to the prime-aged unemployed who are entitled to the same services from the unemployment insurance and are registered in unemployment offices that are comparable to others. In our main analysis, we focus on *Swiss caseworkers and Swiss unemployed workers whose mother tongues are identical to the cantonal language*. This definition ensures that caseworkers and unemployed are already identical in two dimensions: Nationality and mother tongue.[4] This population contains 38,620 unemployed persons.

2.2 Definition of outcome and treatment variables

An individual is considered as employed in month t if he has de-registered at the employment office because of having found an occupation, and has not re-registered yet. To analyse the dynamic impacts of the caseworker's characteristics on the employment probabilities, the employment status $Y_{i,t_0+\tau}$ is measured, relative to the time of first registration t_0 until the end of 2006. Hence, for individuals who registered in January 2003, their employment situation is followed up for 48 months, whereas only 36 months are observed for those registering in December 2003. (In Section 4.2 we also look at other outcome variables, which are constructed analogously.)

For our main analysis in the sample with Swiss unemployed and caseworkers, we define for the unemployed person i the variable D_i=1 if person i and his caseworker are of same *gender*, similar

[4] We do not observe the mother tongue of the caseworkers. However, since we only retain Swiss caseworkers and since many Swiss persons are at least bilingual or have a working knowledge of several European languages, it seems reasonable to assume that they are proficient in the main language of the region where they are employed.

age and same *educational* background. Otherwise, the similarity indicator D_i is set to zero. Thus, D_i=0 if there is dissimilarity in *at least one* characteristic. According to this definition, there are 1,455 unemployed with similar and 37,165 unemployed with dissimilar caseworker.

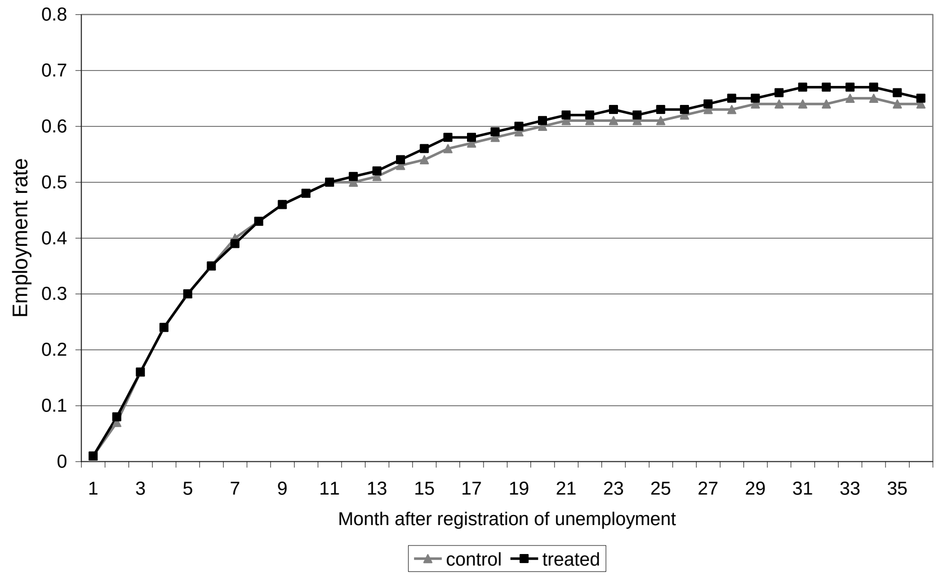
More precisely, caseworker and unemployed are considered to be of "similar age" if the absolute difference between their age is less than or equal to 4 years. Educational background is classified into four categories: Primary education (i.e. no degree from secondary education); lower secondary education or apprenticeship; upper secondary education; graduate from university or college or polytechnic. We consider caseworker and unemployed to have the same educational background if their highest educational attainment falls into the same of these four classes.[5]

2.3 Descriptive analysis

Figure 2 shows the average monthly employment rates after registering at the employment office (month 0). The black line shows the average employment rates for the *D*=1 group, i.e. for those unemployed whose caseworker had the same gender, age, and education. The grey line shows average employment rates for the *D*=0 group. After three months, around 16% of both groups have deregistered from the employment office because of having found a job. After one year, more than 50% of the unemployed are employed again. In the subsequent months, the employment rate is about 2 percentage points higher in the *D*=1 group than in the *D*=0 group.

[5] Observations with missing education are deleted from the sample. In an appendix to Behncke *et al.* (2008), we examined alternative ways of handling missing values in the education variable via subgroup analysis or by coding *D* as zero and obtained rather stable results.

Fig. 2: Average employment rate in month t after registering as unemployed



Note: Average employment rates are for the main sample. The black line shows the employment rate for the 1,455 unemployed who are counselled by a caseworker with the same gender, age, and education. The grey line shows the employment rate for the 37,165 individuals whose caseworker is different in at least one of the three characteristics. Abscissa: Month after registration of unemployment. Ordinate: Employment rate in month *t* after registering as unemployed.

Table 1 shows how the characteristics of the unemployed, the local labour market and the caseworkers differ between the D=1 and the D=0 group. Using a Logit regression, D_i is regressed on a set of variables that could potentially explain the selection process. The Logit estimates are also a crucial element of the propensity score estimation, as will be discussed in the next section. The last two columns show the means of these variables in the D=1 and D=0 group.

When comparing characteristics of the unemployed persons in both groups, we observe clear differences for age: the unemployed in the D=1 group are on average five years older, which is natural because the caseworkers happen to be on average older than the unemployed. With respect to gender and educational attainment, both groups are quite similar. Regarding employment history, unemployed in the D=1 group had on average more months of employment in the last ten years and somewhat higher earnings. This could partly reflect their higher age and thus longer labour market experience.[6] None of the other characteristics of the unemployed person are significant.

Table 1: Estimation of the propensity score: Determinants of similarity

	Marginal effects	Sample average	
	Marginal effects	Same age, gender *and* education (D=1)	Different in at least one characteristic (D=0)
Characteristics of the unemployed clients			
Age in years	.00148*** (.00017)	41	36
Female	-.00154 (.00395)	.43	.45
Education: primary education	-.00286 (.00422)	.14	.15
lower secondary education and apprenticeship	.00128 (.00407)	.63	.61
higher secondary education	.00664 (.00600)	.04	.03
graduate from university/college/polytechnic	- -	.19	.20
Qualification: unskilled	.00213 (.00327)	.10	.10
semi-skilled	.00253 (.00305)	.14	.13
skilled	- -	.76	.77
Months employed in last ten years	.00009** (.00004)	97	90
Monthly earnings in previous job (divided by 1000)	.00090* (.00053)	5	4.5
Number of dependent persons	--	2.0	1.8
Looking for part-time job (dummy)	.00471 (.00351)	.13	.11
Industry of previous job: agriculture and forestry	.01585 (.01359)	.01	.01
construction	.00730 (.00941)	.07	.06
processing industry	-.00097 (.00642)	.14	.15
tourism	-.00642 (.00705)	.07	.07
services	.00383 (.00621)	.52	.49
public	.00796 (.00709)	.17	.16
other	-	.03	.05

Table 1 to be continued.

[6] These employment history differences vanish when one examines more narrowly defined age subgroups.

Table 1: Continued …

	Marginal effects	Sample average	
	Marginal effects	Same age, gender *and* education (D=1)	Different in at least one characteristic (D=0)
Local labour market characteristics			
Language of employment office: French	-.01209*** (.00395)	.16	.23
Italian	.01214 (.00836)	.09	.07
German	- -	.75	.71
Registering in second half 2003 (dummy)	.00107 (.00191)	.58	.56
Size of municipality ≥200000 inhabitants	- -	.09	.08
≥150000	.00395 (.00721)	.10	.09
≥75000	-.01168* (.00645)	.04	.05
≥40000	-.00794 (.00709)	.02	.04
≥25000	.00110 (.00704)	.05	.05
≥15000	-.00320 (.00555)	.15	.15
≥8000	-.00417 (.00545)	.13	.13
≥3000	.00120 (.00601)	.23	.20
≥2000	-.00153 (.00582)	.10	.10
<2000	-.00299 (.00640)	.11	.11
Unemployment rate of canton (in %)	.00425* (.00238)	3.8	3.8
Unemployment rate in industry (in %)	.01475** (.00066)	4.6	4.6

Table 1 to be continued.

Table 1: Continued …

	Marginal effects	Sample average	
	Marginal effects	Same age, gender *and* education (D=1)	Different in at least one characteristic (D=0)
Characteristics of their caseworkers			
Age in years	- -	40	46
Female	- -	.43	.42
Tenure in employment office (in years)	-.00172*** (.00055)	5.28	5.80
Previous experience in municipality office (dummy)	.01401 (.00968)	.13	.10
Previous experience in private placement office (dummy)	.00786* (.00417)	.30	.23
Own experience of unemployment (dummy)	-.00926** (.00375)	.56	.62
Education: primary education	- -	.00	.01
lower secondary education and apprenticeship	- -	.76	.31
higher secondary education	- -	.16	.46
graduate from university/college/polytechnic	- -	.07	.23
Special vocational training of caseworker (Eidg. Fachaus.)	-.00205 (.00463)	.23	.25
Average caseload per month (number of cases)	.00003 (.00005)	131	132
Allocation of unemployed to caseworker			
by industry	-.00726** (.00330)	.48	.52
by occupation	-.00188 (.00375)	.52	.54
by age	-.01257* (.00680)	.02	.03
by employability	-.01214** (.00533)	.04	.06
by region	-.01244*** (.00414)	.08	.12
other	-.00443 (.00623)	.06	.07
at random	-	.25	.22
by alphabet	-	.03	.04
by caseload	-	.40	.41

Note: Maximum Likelihood logit regression, using Stata logit command. Marginal effects are shown, obtained by Stata mfx. Standard errors are in parentheses. Dependent variable is the binary indicator for similarity D_i. 1,455 observations with D_i=1, 37,165 observations with D_i=0. Standard errors clustered at the caseworker level. Significance at the 1%, 5% and 10% level, respectively, is indicated by ***, **, *.

With respect to local labour market characteristics, unemployed in cantons and industries with higher unemployment rates are more likely to be counselled by a similar caseworker. We find that unemployed in French-speaking offices are significantly less likely to be counselled by a caseworker with same gender, age and education, whereas it is the other way around for Italian-speaking job-seekers. The main reasons for this are the differences in the educational level of the caseworkers. In the French-speaking employment offices, many more caseworkers have a university degree than in the German-speaking employment offices. In the Italian-speaking offices, on the other hand, many more caseworkers have a lower secondary education or apprenticeship.[7] In this sense, the caseworkers in the French part are on average more dissimilar to their unemployed, whereas the caseworkers in the Italian part are more similar. We do not find any significant differences with respect to municipality size or the time of registration.

When looking at caseworker characteristics, we find some significant differences. Caseworkers in the D=1 group are on average six years younger due to the reasons discussed above, but the difference in tenure is only half a year. Whereas the D=0 group has more tenure, the D=1 group more frequently has obtained previous work experience either in a municipality employment office or in a private placement agency. Previous own experience of unemployment is more frequent in the D=0 group. The most striking differences are in the educational attainment of the caseworkers: Caseworkers with lower secondary education and apprenticeship are more often in the D=1 group, whereas caseworkers with higher education are more often in the D=0 group. This pattern is as expected since three quarters of the unemployed have only lower secondary education or an apprenticeship. Hence, even with a purely random allocation we would expect such a pattern.[8]

There are also some significant differences with respect to the allocation of unemployed to caseworkers. Individuals in the D=1 group are less likely to be assigned according to industry, age or employability compared to the reference group (which is “random”, “alphabetically” or “caseload”).

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[7] One reason for this could be differences in the hiring practices of the employment offices. The main reason, however, is probably the generally much higher inclination to academic study in the French part of Switzerland.

A striking finding of the Logit regression is that most of the coefficients are small and insignificant. We interpret this as an indication that there is almost no selection based on these characteristics. It is important to note that those characteristics form the knowledge of the employment office about the unemployed before the counselling process starts. Thus, they determine the matching between the specific unemployed client and the caseworker.

3 Identification and estimation of treatment effects

3.1 Conditional independence assumption as identification strategy

Consider an individual i who registers as unemployed at time t_0 at the nearest regional employment office. This person is then assigned to a caseworker of that office.[9] Let D_i=1 if the caseworker is similar to the unemployed person, and D_i=0 otherwise. We are interested in the impact of similarity on the subsequent employment prospects of this unemployed person, which we measure by the employment status $Y_{i,t_0+\tau}$ in month τ after registration. In particular, we would like to compare the employment status with the potential employment status if the same unemployed person was counselled by a caseworker with similarity index D=0. Therefore, we define the *potential outcomes* $Y^d_{i,t_0+\tau}$ at some time τ after unemployment registration at time t_0 if the similarity index was set to d, where d can take the values 0 or 1. We will focus mainly on estimating the average treatment effect on the treated (ATT), for each month τ after registration of unemployment.[10] To identify the ATT we assume that we can control for all variables X that jointly affect D as well as the employment outcome

[8] The averages for education are identical by definition for unemployed and caseworkers in the D=1 group.

[9] This may take a few weeks because the office may require all relevant documents before assigning a counselling meeting. They may also send the unemployed person first to a one-day information workshop.

[10] We focus on the ATT, and not on the average treatment effect for the untreated, because we have a large number of D=0 observations but only rather few D=1 observations. If we were to estimate the effect on the non-treated, with a matching estimator we would have to re-use the few D=1 observations very often to match them to the D=0 observations. This would lead to very noisy estimates.

Y^0. This conditional independence assumption (CIA) is also referred to as 'selection on observables' or 'unconfoundedness' (e.g. Rubin, 1974). We additionally need common support, which requires that every value of X in the D=1 population is also observed in the D=0 population.

Before we discuss possible estimators, we need to assess whether the conditional independence assumption is plausible with our data, i.e. whether we are able to observe all confounding variables X. The first two columns of Table 1 already indicated that there are only rather few significant differences in observable characteristics between treated and controls. As noted from Table 1, the unemployed in the D=1 group differ from the D=0 group in their average age and, to a lesser extent, in their education. To avoid bias due to e.g. differences in age, we want to control for the characteristics of the unemployed used to define D, i.e. age, gender and education of the unemployed. Note that we cannot simultaneously control for age, gender and education of the unemployed *and* of the caseworker as this would determine D with probability one and thus violate the common support assumption. Consider an illustrative example, where for simplicity we had defined similarity only with respect to gender. If we included gender of the unemployed and of the caseworker in the set of control variables X, there would be values of X for which the common support condition is violated. Consider e.g. the value X=(female, female), i.e. a female unemployed assigned to a female caseworker, which implies D=1 with probability one. Hence, for this combination it would be impossible to observe the similarity status D=0, violating the common support condition. This example extends analogously to the case where we define similarity by the three characteristics age, gender and education. Controlling for the characteristics of the unemployed *and* the caseworker would only be possible via restrictions on treatment effect heterogeneity. Therefore, we control for age, gender and education of the unemployed and for a number of *other* characteristics of the caseworker.[11]

We now discuss which potentially confounding variables we would like to include in X. Consider two unemployed persons with identical age, gender and education, but different values of D. Which could be reasons why D is different for these two individuals? We can distinguish between allocation patterns between and within employment offices. Regarding differences between offices, we control for several characteristics of the local labour market. (We also used a specification with employment office dummies, which did not change the results very much.)

Regarding within office allocation we can consider various channels. Occupational background could be one reason why a male or a female caseworker is assigned. Caseworkers are often assigned by industry sector, where male caseworkers are more often experienced e.g. in the construction, engineering or technical sector than female caseworkers. We thus control for the qualification and industry sector of the unemployed person.

When two individuals are identical on these characteristics it is probably more or less random whether D=0 or D=1, mostly depending on the random fluctuations in the office, i.e. the caseload and available time of the caseworkers. To be on the safe side, we nevertheless include many characteristics of the caseworker to ensure that their average quality is the same irrespective of whether D=0 or D=1. These variables include tenure, previous experience in a municipal employment office, previous experience in a private placement agency, own experience of unemployment, participation in special caseworker training, and caseload. These variables capture the information of the labour office at the time of the decision to allocate a specific caseworker to a specific unemployed client.

[11] In several initial analyses we examined the effects of caseworker's age, gender and education on the employment chances of their unemployed and did not find any significant effects. Hence, we are confident that the estimated effects in Section 4 are the effects of similarity and not of the caseworker characteristics per se.

Overall, Table 1 suggested that there is no clear selection rule, which assigns unemployed to similar caseworkers. We interpret this as indication that the similarity indicator D_i is more or less random.[12] Although these estimates do not rule out selection-on-unobservables, it seems implausible that this would be of a concern. If the indicator D_i were driven by selection-on-unobservables, we would expect D to be correlated with some observed characteristics as well. This is particularly so since some of the X variables included in the regression are not available in many other datasets, but most of the characteristics of the unemployed person are insignificant in Table 1.[13] (In Behncke *et al.* (2008) we explored even larger regressor sets.)

3.2 Estimation methods

For estimating the effects for various alternative definitions of D and Y we implement two estimation approaches: regression and matching. Since Y is mostly binary, Maximum Likelihood logistic regression is used and the ATT is calculated. The ATT is obtained by first computing the marginal effect for a value of X and then averaging these effects over the distribution of X in the D=1 population. Maximum Likelihood regression has the advantage of being efficient if the outcome model is correctly specified.

In addition to regression, we use propensity score matching (PSM) which has the advantage of allowing for arbitrary individual treatment effect heterogeneity. PSM has the further advantage of permitting model specification (e.g. variable selection for the propensity score) that is not affected by the outcome variable Y, such that the treatment effects themselves cannot affect the model specifica-

[12] In Section 4.6 we will also explore a specification where we use only the subset of caseworkers, who had indicated that allocation was indeed at random.

[13] As mentioned, one exception is the age of the unemployed person. This occurs naturally because the average age of the caseworkers is larger than the average age of the unemployed persons. For a young unemployed person it is thus naturally less likely to be allocated to a caseworker of similar age, even if the entire assignment process is at random.

tion procedure.[14] In any case, using these two very different estimation approaches (regression and PSM) permits us to assess the robustness of our empirical results.

In this paper we use an extension of conventional PSM in that we match not only on the propensity score but also on gender, age and three education dummies in order to improve finite sample properties. In addition, we include a bias reduction technique via regression on covariates within matched pairs. More details on the estimator are given in the corresponding working paper (Behncke *et al.*, 2008). Inference for the PSM estimator is based on the bootstrap by re-sampling caseworkers (together with all their clients) to account for possible dependencies among the unemployed counselled by the same caseworker.[15] Following MacKinnon (2006), the t-statistic is bootstrapped and the bootstrap p-value is based on symmetric rejection regions of the t-statistic. We report bootstrap standard errors in the tables, but our inference is based on the bootstrap p-values in order to obtain asymptotic refinements.

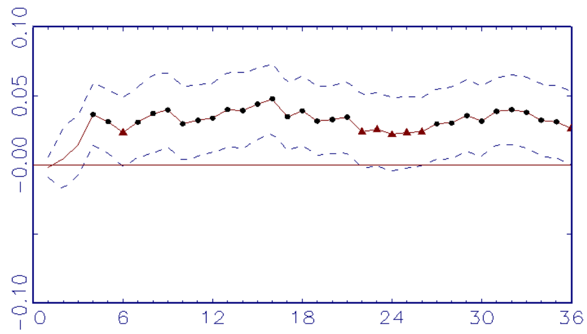
4 Empirical results

4.1 Effects of similarity on employment

Figure 3 shows the effects of similarity on employment (in a non-subsidised job) from the matching estimator, for our main population of Swiss caseworkers and Swiss unemployed (whose mother tongue is identical to the cantonal language) of age 24 to 55. The effects are shown for months 1 to 36 after registration, together with pointwise 95% confidence intervals. The estimates show a stable positive effect of additional employment of about 3-percentage points. Thus, unemployed have higher employment probabilities if their caseworker is similar to them.

[14] See e.g. Heckman *et al.* (1999), Imbens (2000), Lechner (2001), and Gerfin and Lechner (2002), Frölich (2004, 2007) for matching with binary or non-binary treatments. Imbens (2004) provides an excellent survey.

Fig. 3: Effects of similarity in age, gender and education on employment



Note: ATT Treatment effect of similarity in age, gender *and* education on employment, estimated by propensity score matching. Matching is on the propensity score (as given by Table 1) and on age, gender and education of the unemployed as additional variables. 1,455 observations with D_i=1, 37,165 observations with D_i=0. Abscissa: Month after registration of unemployment. Ordinate: Treatment effect on employment in month *t* after registering as unemployed. Inference is based on bootstrapping the t-statistic via re-sampling caseworkers. Dots indicate significance at the 5% level, triangles at the 10% level. The dashed lines represent pointwise 95% confidence intervals.

4.2 Effects of similarity on alternative outcomes

In this section, we show the effects of similarity on other outcome variables, including alternative definitions of employment as well as the use of sanctions and active labour market programmes. We first examine the stability of employment in order to judge whether the results of the previous section might be driven by outflows into unstable jobs. We define a worker to be in *stable employment* in a given month if his employment spell is at least of 12 months duration. This variable should thus shed light on whether job retention is affected. We find positive effects on stable employment and virtually no differences to Figure 3. We find similar results when defining stable employment as an employment spell of at least 3 or 6 months duration, respectively. (Results are not shown.) This suggests that having a similar caseworker also has a positive effect on job stability.

Further, we examine the effects of similarity on job seeker status. The variable *seeking for a job* measures whether an individual is registered at the regional employment office as a job seeker. In Switzerland, people can register at the employment office as seeking for a job, even if they are cur-

[15] Note that due to the regression step incorporated in the matching procedure, one could argue that the estimator is much smoother than the case studied by Abadie and Imbens (2008) so that their negative results about the bootstrap should apply.

rently employed. Reasons for a registration might be that they are looking for a new job either because they know or anticipate that they will lose their current employment or because they are not satisfied with their current job and actively search for a new one. Since this variable partly reflects job satisfaction and partly job stability, it also serves as an indicator for job quality. Figure 4 shows that clients with a similar caseworker are significantly less often registered as seeking for a job in the first 20 months after becoming unemployed. This suggests that the employment gain comes not at the cost of reduced job satisfaction.

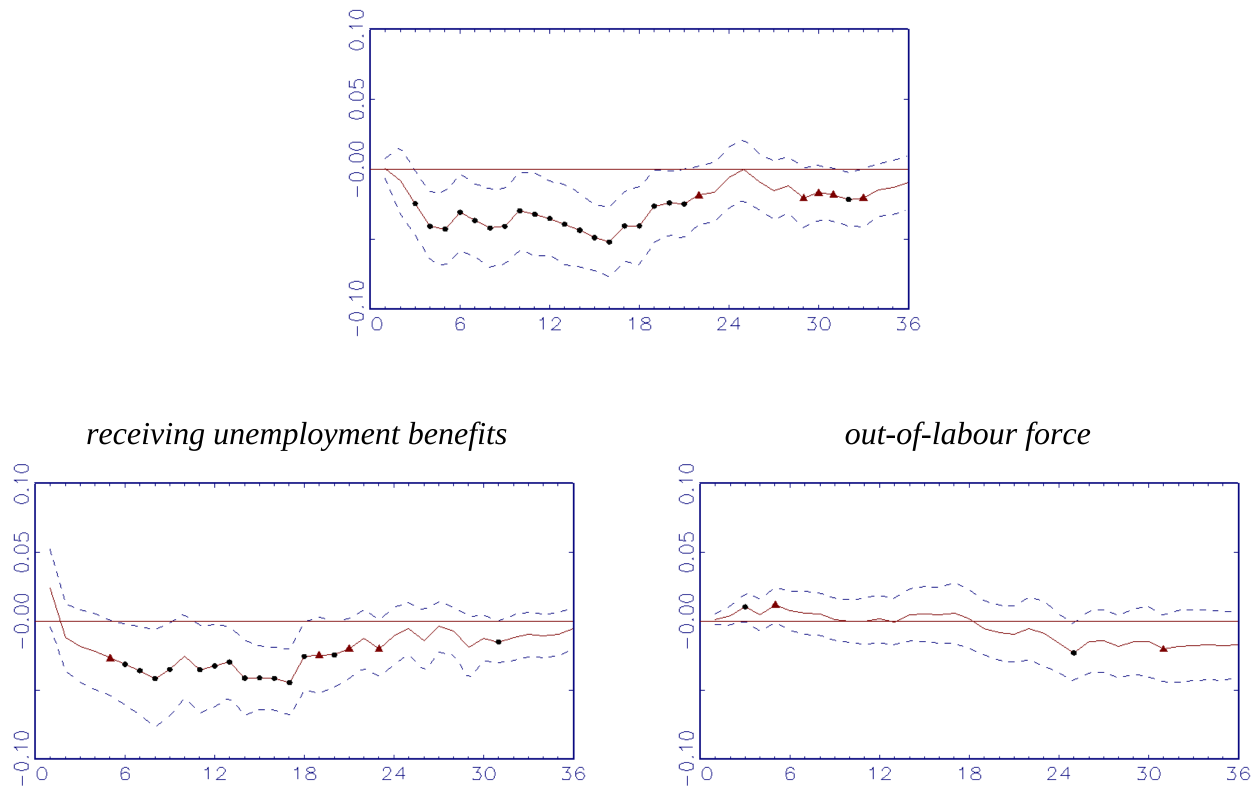
Next, we examine the effects of similarity on unemployment status, defined as *receiving benefits*, in Figure 4. This outcome variable measures whether an individual receives benefits from the unemployment insurance. These benefits incorporate the usual unemployment compensation, but also any payments for active labour market programmes, subsidies for temporary employment or internships. Consistent with our estimates for the employment outcomes, we find that individuals with similar caseworkers receive fewer benefits from the unemployment insurance, which is significant for the first 20 months.

In the last graph in Figure 4 we show the effects on *out-of-labour force* status, which are initially zero and later negative. Overall, Figure 4 shows an interesting dynamic pattern. Whereas the effects on unemployment and jobseeking are negative during most of the time, they become smaller around month 18, become zero around month 24, and are small thereafter. At the same time, the effects on out-of-labour force are zero in the beginning and become negative from about month 20 onwards.[16] This indicates that the positive effects of Figure 3 are due to reductions in unemployment during the first 20 to 24 months, whereas afterwards they are also due to reductions in out-of-labour force.

[16] The estimated effects are mostly insignificant. Yet, in a previous version of the paper where we used probit instead of logit estimation, the effects were significantly negative everywhere from month 20 onwards.

Fig. 4: Effects of similarity in age, gender and education on

seeking for a job



See note below Figure 3.

A possible, but highly tentative, explanation for this pattern may be the maximum benefit entitlement period, which is 18.5 to 24 months.[17] In the beginning of an unemployment spell, similarity (D=1) reduces unemployment and increases employment. When the threat of benefit exhaustion approaches, the individuals in the control group (D=0) catch up in their job findings rates (perhaps into unstable jobs), which could explain the little dip around month 24 in Figure 3. (This also explains, why we find no effects on benefit exhaustion in Table 2.) This catch-up seems to be of only temporary nature, though, followed by increases in out-of-labour force rates among the control group. (I.e. the negative effect on out-of-labour force in Figure 4 and the increase in Figure 3 after month 24.)

[17] The maximum benefit entitlement period for the unemployed under the age of 55 was 24 months until July 2003 when it was reduced to 18.5 months.

Table 2 summarises these results and additionally shows the effects on *benefit exhaustion*. The variable benefit exhaustion measures whether individuals have lost their eligibility for unemployment insurance benefits (after 18.5 or 24 months). Thus, this variable reflects the negative consequences of long-term unemployment. The very small (and insignificant) point estimates in Table 2 suggest that there are no effects on benefit exhaustion. Hence, despite the positive employment effects of similarity, the control group nevertheless also manages to avoid benefit exhaustion. In fact, there are only very few unemployed individuals (around 2%) who lose their eligibility for unemployment benefits.

Table 2: Effects of similarity in age, gender and education on labour market outcomes

		Month 12	Month 24	Month 36
		ATT	ATT	ATT
Employed	psm	.034***	.022*	.027**
		(.017)	(.016)	(.016)
	logit	.036***	.031**	.036***
		(.013)	(.014)	(.012)
Long-term employed	psm	.038***	.032**	.031**
		(.016)	(.016)	(.016)
Seeking for a job	psm	-.035***	-.006	-.010
		(.016)	(.014)	(.012)
Receiving UI benefits	psm	-.033**	-.011	-.006
		(.016)	(.013)	(.010)
Out-of-labour force	psm	.001	-.016	-.017
		(.010)	(.013)	(.014)
Exhausted UI benefits	psm	.001	.004	.000
		(.004)	(.004)	(.006)
Number of sanction days	psm	-.059	.009	NA
		(.257)	(.711)	NA

Note: ATT Treatment effect of similarity in age, gender *and* education on labour market outcomes, estimated by propensity score matching (psm) and maximum likelihood logistic regression (logit). Matching is on the propensity score (as given by Table 1) and on age, gender and education of the unemployed as additional variables. Logit regression is on similarity and Xset 1 (i.e. the variables in Table 1). Standard errors are in parentheses. We only report logit results for the main employment outcome. (For the other outcome variables the PSM results and the logit results are similar.) 1,455 observations with D_i=1, 37,165 observations with D_i=0. Bootstrap standard errors are shown. However, inference is based on bootstrapping the t-statistic via re-sampling caseworkers for matching and on robust clustered standard errors for logit. Significance at the 1%, 5% and 10% level, respectively, is indicated by ***, **, *. The estimates for number of sanction days for month 36 are not available. The information on sanction days is only available to us for up to two years after becoming unemployed.

In the first two rows of Table 2 we also examine the sensitivity of the results to the estimation methods. We estimated the ATT via propensity score matching (psm) and also by parametric logit regression (logit).[18] Overall, the results are rather similar, with the PSM estimates being the most conservative. The parametric estimates are always larger than 3-percentage points. In the remainder of the paper we report only the, conservative, PSM results.

In the following, we examine the effects of similarity on potential mediating channels. Two channels stand out: caseworkers may apply *sanctions* to different degrees and/or they make use of various active labour market *programmes* differently. We observe the number of realised sanction days and actual participation in programmes in the data.[19]

Caseworkers exert substantial discretion in the imposition of sanctions. Sanctions can be imposed for non-compliance with the regulations of the unemployment system, for example insufficient search effort. Caseworkers can be more or less lenient towards the unemployed and they have some discretion in the number of sanction days. We estimate the effects of similarity on the *number* of sanction days in every month of the unemployment spell. The estimated effects are always very small, insignificant and often change sign from one month to the next. In the last row of Table 2, the effects for month 12 and 24 are shown.[20] We also estimated the effects on the total (i.e. cumulated) number of sanction days during the first year of unemployment. While the average number of sanction days in our sample is 4.33 days (with 80% of the unemployed having zero sanction days), the estimated effect of similarity is only -0.139 (0.682).

[18] As an alternative to the ATT, we also calculated the marginal effect at the means of the independent variables from the logit regressions. The effects of similarity on employment for months 12, 24 and 36, respectively are: 0.038 (0.014), 0.032 (0.014), 0.037 (0.013), with robust clustered standard errors in brackets. (These are the effects at the mean and not the average treatment effects.) When using linear regressions of employment in months 12, 24 and 36, respectively, on similarity and the other control variables, we obtain as coefficient: 0.036 (0.013), 0.031 (0.014), 0.035 (0.012).

[19] We do *not* observe whether caseworkers have threatened to use sanctions or to assign onerous programmes.

[20] The information on sanction days is only available to us for up to two years after becoming unemployed.

As a second channel, we examine the use of active labour market programmes (ALMP). We estimated the effects on various indicators such as whether the person ever attended an ALMP (until the end of 2006), the number of ALMP, as well as which types of ALMP were attended. None of these results was significant, such that we omit their presentation.[21] The only significant result was an estimate of 0.032 (0.018) on the probability of *never* attending ALMP (until the end of 2006).[22] Hence, similarity reduces the likelihood of attending ALMP. It is unclear, however, whether this is due to the positive effect of similarity on employment, as witnessed in Section 4.1, or whether similar caseworkers assign less ALMP per se. In any case, however, it does not seem to be the case that similarity would increase the use of ALMP, which could have meant an increase in the costs to the unemployment insurance funds.

This weak evidence on the intermediate channels implies that the positive employment effects found in Figure 3 are mostly due to channels such as motivation, trust or a more effective communication or counselling, rather than through sanctions or additional spending on ALMP. If anything, similarity leads to fewer ALMP.

4.3 Regressions with caseworker main effects

The previous sections displayed the estimation results when we controlled for age, gender and education of the unemployed and for a number of *other* characteristics of the caseworker. We did not control for the age, gender and education of the caseworkers, as this would invalidate the identification of treatment effects: For any observed values of age, gender and education of both the unemployed *and* the caseworker, a logical counterfactual in our sense does not exist. Hence, it would be impossible to observe treated and non-treated with the same values of X. In addition, (parametric) estimation of the propensity score would lead to problems of perfect prediction.

[21] The estimated tables are available in a previous version of this paper.

[22] Of the entire sample, 43% attended at least one ALMP until the end of 2006.

In this section, we report estimation results where we *do* include age, gender and education of both caseworker *and* unemployed in order to examine the sensitivity of the previous results. Clearly, in this case we cannot use nonparametric approaches due to non-identification nor semiparametric propensity score matching due to additional problems with perfect predictions (this approach could be thought of as something like a fixed effects/difference-in-difference strategy). We therefore have to impose certain homogeneity assumptions to be able to identify and estimate a causal effect.

The following Table 3 gives logit regressions of employment on all the regressors shown in Table 1. In addition we add caseworker's age, sex and education, and in the right half of the table the similarity indicator. The table shows the marginal effects (at the mean) only for the regressors age, sex, education and similarity. Two main results emerge from this table. First, the caseworker characteristics age, sex and education are all small and insignificant. Hence, their omission in Sections 4.1 and 4.2 appears to have been without concern. Second, the effects of similarity remain highly significant, and are stable around 3 percentage points.

Table 3: Employment effects, including caseworker's age, sex and education

	without similarity regressor			with similarity regressor		
	Month 12	Month 24	Month 36	Month 12	Month 24	Month 36
	Marginal effect	Marginal effect	Marginal effect	Marginal effect	Marginal effect	Marginal effect
Characteristics of the unemployed						
Same age, sex and education	-	-	-	.04***	.03**	.04***
	-	-	-	(.01)	(.01)	(.01)
Age	-.01***	-.01***	-.01***	-.01***	-.009***	-.01***
	(.0004)	(.0003)	(.0003)	(.0004)	(.0003)	(.0003)
Female	.07***	.07***	.079***	.06***	.07***	.07***
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)
Education: primary education	-.15***	-.02***	-.17***	-.15***	-.19***	-.17***
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
lower secondary education	-.09***	-.01***	-.10***	-.09***	-.12***	-.10***
	(.01)	(.007)	(.007)	(.007)	(.007)	(.007)
higher secondary education	-.08***	-.01***	-.09***	-.08***	-.11***	-.09***
	(.02)	(.01)	(.01)	(.02)	(.01)	(.02)
Characteristics of the caseworker						
Age of caseworker	.0002	.0003	.0003	.0003	.0004	.0003
	(.0004)	(.0004)	.0003)	(.0004)	(.04)	(.0003)
Female	.003	.002	.003	.004	.002	.003
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)
Education: higher secondary	-.002	-.006	-.0074	.0011	-.004	-.004
	(.007)	(.007)	(.007)	(.007)	(.007)	(.007)
graduated from university	-.02*	-.02	-.01	-.01	-.01	-.006
	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)

Note: Maximum Likelihood logit regression of employment on characteristics of the unemployed, characteristics of the caseworker and similarity status, using Stata logit command. Marginal effects are shown, obtained by Stata mfx. Dependent variable is the binary indicator for employment at month 12, 24, and 36 after becoming unemployed. Standard errors are in parentheses. Other covariates (Xset 1) included, but not reported. Robust standard errors (clustered at the caseworker level) are shown. Significance at the 1%, 5% and 10% level, respectively, is indicated by ***, **, *.

4.4 Effects of alternative definitions of similarity

In this section, we explore different definitions of similarity. So far, we compared the treatment group with same sex, age *and* education (=1,455 observations) to all other observations who differed in *at least one* characteristic (=37,165 observations). This latter control group contains the subpopulations of individuals who differed in all three characteristics, or in only two characteristics or in only one characteristic. In this section, we therefore want to disentangle the effects between these subgroups to see which characteristic(s) matter most.

Similarity can be defined according to three, two or one characteristic. Equivalently, the caseworker and his client can be dissimilar in just one criterion, in two, or in three criteria. Estimating *dose-response* functions is a concise way to summarize the effects of the degree of similarity.[23] These *dose response* estimates are shown in Table 4.[24] Here we define the degree of similarity as the *number* of identical characteristics (i.e. 0, 1, 2 or 3). The estimates show a clear pattern: Increasing similarity from 0 to 1 or from 1 to 2 has no employment effects. On the other hand, increasing similarity from 2 to 3 characteristics, increases employment by about 4 to 5 percentage points.

Hence, the dose-response function (i.e. the effect on employment as a function of the degree of similarity) does not appear to be linear: Being similar to the caseworker in one or two dimensions does not seem to matter, but when one increases similarity from 2 to 3 dimensions, a large positive employment effect is observed. This result is interesting as it suggests that the underlying mechanism for the effects of similarity may not be a conventional production technology of counselling since one would expect that each additional dimension of similarity should improve communication, information processing and understanding. It may rather be due to a non-linear psychological process, which requires a minimum threshold of similarity in various dimensions before caseworker and jobseeker feel a 'similarity' or affection/sympathy.

[23] In a previous version of this paper, we estimated separately the effects for each of the 22 different possible combinations of treatment and control group. Those results showed that similarity in one or two characteristics is not sufficient to generate positive employment effects. Instead, similarity in all three characteristics is needed. Regardless of the chosen control group, the effects of similarity in *three* characteristics were in the range of roughly 2 to 5 percentage points and mostly significant. The effects of similarity in 3 versus 2 characteristics were not systematically smaller or larger than the effects of 3 versus 1 or 3 versus 0. Furthermore, the effects for similarity in *two* characteristics or similarity in *one* characteristic were in most cases insignificant and always small.

[24] Note that these estimates are less precise than those of Figure 3 because of the smaller sample sizes. In the main specification corresponding to Figure 3 the control group contained all unemployed who differed from their caseworker in *at least one* characteristic, which gives the largest sample size of 37,165 control persons.

Table 4: Employment effects of different definitions of similarity (dose response)

	Month 12	Month 24	Month 36
Degree of similarity	ATT	ATT	ATT
1 versus 0	-.010	-.003	-.004
	(.007)	(.007)	(.007)
2 versus 1	-.013	-.004	-.004
	(.007)	(.007)	(.007)
3 versus 2	.052***	.058**	.046***
	(.018)	(.017)	(.017)

See note below Table 2. We only report results for propensity score matching, but logit results are similar.

In addition to the nonparametric results of Table 4, we also examine parametric regressions. Analogously to Table 3, we show in the following Table 5 the results of linear logistic regressions where we include one-, two- and three-way interaction terms.[25] We report marginal effects, but note that their interpretation is difficult because switching the three-way interaction term from 0 to 1 logically implies that also all the other interaction terms will be 1. Hence, the marginal effects cannot be interpreted in isolation. Nevertheless, as Table 5 clearly shows, only the three-way interaction term is significant. (All one- and two-way interactions are jointly insignificant.) This supports the interpretation that similarity in all three dimensions is required. (Note that including all seven similarity regressors in the regression leads to a high degree of collinearity, such that the three-way interaction term is only significant at the 17% level for month 24 and at the 11% level for month 36. The p-value for month 12 is 1.7%.)

[25] There are three one-way interaction terms: Caseworker and unemployed have same gender; Caseworker and unemployed have same age; Caseworker and unemployed have same education. There are three two-way interaction terms: “Same gender and same age”; “Same gender and same education”; “Same age and same education”. Finally, there is one three-way interaction term: “Same gender, same age *and* same education”.

Table 5: Employment effects with similarity interaction terms

	without caseworker characteristics			with caseworker characteristics		
	Month 12	Month 24	Month 36	Month 12	Month 24	Month 36
	Marginal effect	Marginal effect	Marginal effect	Marginal effect	Marginal effect	Marginal effect
Same gender, same age *and* same education	.687** (.028)	.0376 (.0277)	.0421 (.0265)	.00685** (.0286)	.0374 (.0276)	.0419 (.0265)
Two-way interaction terms						
Same gender and same age	-.0254 (.0168)	-.0169 (.0162)	-.0201 (.0160)	-.0252 (.0168)	-.0167 (.0162)	-.0200 (.0160)
Same gender and same education	.0104 (.0129)	.0096 (.0128)	.095 (.0122)	.0101 (.0129)	.0091 (.0128)	.0092 (.0123)
Same age and same education	-.0169 (.0218)	-.0056 (.0222)	.0014 (.0214)	-.0172 (.0218)	-.0060 (.0222)	.0014 (.0214)
One-way interaction terms						
Same gender	-.0064 (.0078)	-.0080 (.077)	-.0020 (.0075)	-0.0062 (.0079)	-.0079 (.0077)	-.0018 (.0075)
Same Age	.0082 (.0130)	.0157 (.0117)	0.0083 (.0120)	.0094 (.0132)	.0181 (.0117)	.0098 (.0121)
Same Education	-.0058 (.0101)	-.0027 (.0101)	-.0039 (.0099)	-0.0076 (.0107)	-.0075 (.0108)	-.0069 (.0103)
Characteristics of the caseworker						
Age of caseworker				.0002 (.0004)	.0005 (.0004)	.0003 (.0004)
Female				.0032 (.0063)	.0017 (.0060)	.0031 (.0059)
Education: higher secondary				.0001 (.0080)	-.0059 (.0087)	-.0047 (.0083)
graduated from university				-.0132 (.0097)	-.0154 (.0100)	-.0066 (.0096)
Joint test (p-value): all one- and two-way interaction terms are zero	.409	.513	.736	.452	.440	.787

See note below Table 3. The last row reports the p-value of the F-test (Chi-square) for the null hypothesis that all six one- and two-way interaction terms (i.e. not excluding the three-way interaction term) are zero.

4.5 Effects of similarity in subpopulations

In the previous sections, the effects of similarity were estimated for the entire population. Next we examine whether these effects differ across subpopulations. In the following table, only the effects for the definition of similarity as in Sections 4.1 and 4.2 are shown (and not for the various alternative definitions of Section 4.4). The reason for this is that the results are overall similar for the different definitions, but are most precise when we compare similarity in all three versus dissimilarity in *at least one* characteristic because this definition leads to the largest sample size of the control group.

In the first two rows of Table 6, we consider female and male unemployed. The employment effects are positive for both subpopulations, but the estimates are less precise due to the smaller sample sizes. Next we stratify by age. The effects are positive for both age groups, but are much larger for the younger unemployed (24 to 35 years). Hence, having a young caseworker appears to be quite important for them. Stratifying by educational attainment of the unemployed, we distinguish between low and high-educated unemployed. The effects are always positive, but too noisy to discern which of these two education groups benefits more from a similar caseworker.[26]

[26] We note that there are only very few (200) observations with D_i=1, because most caseworkers have achieved a higher educational degree.

Table 6: Effects of similarity in age, gender and education for different subgroups

			Month 12	Month 24	Month 36
Subpopulation	n_0	n_1	ATT	ATT	ATT
			Subpopulations defined by characteristics of the unemployed individual		
Female	16706	627	.024	.019	.013
			(.025)	(.027)	(.026)
Male	20459	828	.052**	.019	.026
			(.023)	(.022)	(.021)
Age 24 to 35	19291	443	.067***	.075**	.068**
			(.031)	(.029)	(.029)
Age 36 to 55	17874	1012	.035**	.016	.027*
			(.020)	(.020)	(.020)
Low educated	5755	200	.051	.027	.003
			(.043)	(.043)	(.045)
High educated	31410	1255	.031**	.022	.052***
			(.018)	(.018)	(.018)
			Subpopulations defined by regional (=canton) or industry-specific unemployment rate		
Low regional unemployment	18924	682	.042**	.013	.036*
			.025)	(.024)	(.024)
High regional unemployment	18241	773	.015	.049*	.044*
			(.024)	(.023)	(.022)
Low industrial unemployment	16504	633	.006	.004	.012
			(.024)	(.023)	(.023)
High industrial unemployment	20661	822	.033*	* .034	.048**
			(.022)	(.023)	(.022)

See note below Table 2. Only results for propensity score matching shown, but logit results are similar.
Young unemployed are between 24 and 35 years old; old unemployed are between 36 and 55 years old. Low educated unemployed have achieved primary, lower secondary education or apprenticeship as their highest degree; high educated unemployed have achieved higher secondary education, or graduation from University or polytechnic as their highest degree. The regional/industrial unemployment rate is considered to be high if it is larger or equal than 4 percentage points, otherwise it is considered as low. The unemployment rate is measured at month of registration.

Finally, we distinguish between subgroups facing different tightness of the labour market. First, we distinguish between low and high local (=cantonal) unemployment rate. Second, we distinguish between a low and high unemployment rate in the (previous) industry of the unemployed person. An interesting pattern is observed: When the *industry* unemployment rate is low, the effects of similarity are small or zero and insignificant. On the other hand, when the cantonal or industrial unemployment rate is high, significant and positive employment effects are found. Hence, similarity has a larger effect in environments that are more difficult. This may be an indication that the supportive character of similarity may dominate. (When the unemployment rate is low, the main job of the caseworker is to push unemployed into accepting job offers. When the unemployment rate is high, counselling, placements and contacts to employers become more important.) One should note, however, that there is not such a clear pattern with respect to the cantonal unemployment rate.

4.6 Robustness analysis

In this section, we examine the robustness of our main estimation results of Section 4.1 to potential violations of the conditional independence assumption (3).[27] While we found that selection on observables is very small (see Table 1), one might nevertheless be concerned about unobserved heterogeneity of the caseworker, the employment office or the unemployed person. In Table 7, we therefore consider different specifications to examine the sensitivity of the results.

In the first rows of Table 7, we consider unobserved caseworker heterogeneity. The first row presents a *caseworker fixed effects* specification where we include dummies for each caseworker as additional covariates. We estimate caseworker fixed effects to rule out any concerns that unobserved heterogeneity, e.g. placement efficiency, between caseworkers drives our results. Because the overall number of D=1 observations is small compared to the large number of D=0 observations in our sample, many caseworkers had counselled none or only a very small number of D=1 cases. These caseworkers contain only very little information about the treatment effects when we include fixed effects. Therefore we restrict the caseworker fixed effects regression to those caseworkers whose clients contain at least 5% D=1 observations. This leaves us with 37 caseworkers. Table 7 shows positive effects of about 3 to 5 percentage points, which thus tend to be even somewhat larger than the results of Section 4.1. (No p-values for PSM are reported since the large number of caseworker fixed effects led to numerical problems in the bootstrap replications.)[28][29]

[27] In an appendix to Behncke *et al.* (2008) we also examined different ways of handling missingness in the education variable and obtained rather stable results.

[28] When we apply PSM to this restricted subsample with 37 caseworkers, but do *not* include caseworker fixed effects, we obtain as results 0.059 (0.017), 0.037 (0.024) and 0.063 (0.020), which are thus rather similar to those results *with* caseworker fixed effects.

The second row of Table 7 reports the results for a specification without fixed effects but with a larger number of caseworker characteristics. Here we control for caseworker's attitude and strategy when counselling his client, which might be correlated with the caseworker's efficiency when placing his clients.[30] The estimates are positive throughout and are a bit larger than in the main specification.

Apart from caseworker heterogeneity, there might also be unobserved differences between employment offices, e.g. in the form of idiosyncratic selection rules, that could be correlated with individual labour market success. To be more precise, it might be that the similarity between caseworkers and their unemployed clients is more frequent in some offices than in others. This could be due to the demographic structure of the caseworkers or the unemployed or due to a deliberate strategy by the employment office management, which in some offices might seek to match unemployed persons to caseworkers according to their characteristics whereas such strategies might not be used in other offices. The third row therefore shows *employment office fixed effects* by including employment office dummies in the regression. The effects remain positive but are somewhat smaller than for the main specification. The estimated ATT of the parametric logit regressions are 0.036 (0.013), 0.032

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[29] As a further check, we also conducted linear fixed-effect regressions for different subpopulations. For the full sample we obtain as results for month 12, 24 and 36, respectively: 0.032 (0.014), 0.029 (0.013), 0.034 (0.013) when we include fixed effects. In comparison the OLS results, i.e. without fixed effects, are 0.036 (0.013), 0.031 (0.014), 0.035 (0.012). Second, when examining the subsample of all unemployed who have a caseworker who counsels at least one similar (i.e. D=1) observation, which gives a sample size N=19,274, we obtain as fixed effects results 0.033 (0.014), 0.030 (0.013), 0.035 (0.013), whereas the corresponding OLS results are 0.034 (0.013), 0.029 (0.014), 0.035 (0.012). Finally, for the subsample of the 37 caseworkers described in the main text, we obtain the fixed-effects results 0.072 (0.027), 0.067 (0.026), 0.064 (0.026), and the corresponding OLS results 0.072 (0.028), 0.067 (0.028), 0.067 (0.021). Hence, in all these three samples, the fixed-effects and the OLS results are nearly identical, such that unobserved caseworker heterogeneity does not seem to be of much concern.

[30] These are dummies for responding in the questionnaire to prefer (1) a rapid reintegration over prevention of long-term unemployment, (2) to place the clients via personal contact with employers over via a decree, (3) to assign many job openings over a few, selected job openings, (4) to maintain existing contacts with employers over to contact new firms (5) to let clients find a temporary job themselves over to assign temporary jobs, (6) cooperation with the clients when assigning jobs and active labour market programmes over to assign those sometimes or in general irrespective of their will. With the exception of the latter, these covariates were not significant in the propensity score.

(0.014) and 0.036 (0.012), which are highly significant and very similar to the logit results without fixed effects of Table 2.

As an alternative to employment office fixed effects, we can pursue an alternative approach to examine the potential degree of "selection-on-unobservables". In the questionnaire, the caseworkers were asked about how unemployed persons are allocated to them. We split the sample according to the caseworkers' answers. The first subsample consists of those unemployed whose caseworker mentioned any of the items "allocation by industry", "by occupation group", "by age of unemployed", "by employability", "other" or gave no answer at all to this question. This group contains caseworkers who might have received unemployed clients based on an active selection rule. The remaining subsample consists only of caseworkers who had not mentioned any of the above items, in other words, they had mentioned only "randomly", "alphabetically", "by caseload", or "by region". Assuming that caseworkers responded carefully to the survey, this second group contains only unemployed who had *not* been assigned to a caseworker by a deliberate choice.[31] Therefore, we can be confident that selection-on-unobservables cannot be present in the second group, whereas it might be biasing the results in the first group. The estimates are quite noisy, particularly since the second subpopulation contains only 319 observations with D=1, but are always positive for both populations.

In the following rows we examine alternative specifications of the set of control variables X. In row 6, we included a larger number of additional characteristics of the unemployed person in X, in addition to those already shown in Table 1. These additional variables are 15 dummies for the occupation group of the last job of the unemployed and family size. Furthermore, we included the *number of staff members in the employment office in December 2002* as an additional regressor for the following reason: If the management of the employment office indeed were actively seeking to allocate

[31] Note that region means small parts of the local labour market of which the office is in charge. This criterion is mentioned usually only in rural areas.

unemployed persons to caseworkers with a similar "social identity", we would expect the possibilities for such deliberate allocation to be larger in larger offices. However, the coefficient is negative and insignificant in the propensity score. (Results not shown.) Even with this larger set of regressors, the Pseudo R^2 remains at 0.013. This low value is much more in line with a random assignment process for *D* than with a deliberate and effective allocation by the office management. The estimated employment effects remain significantly positive.

Table 7: Sensitivity analysis of the effects of similarity in age, gender and education

			Month 12	Month 24	Month 36
	n_0	n_1	ATT	ATT	ATT
1) Caseworker fixed effects	2244	401	.052	.034	.041
			-	-	-
2) More caseworker covariates	37165	1455	.057***	.042**	.044***
			(.017)	(.017)	(.017)
3) Employm. office fixed effects	37165	1455	.032	.008	.017
			-	-	-
4) Allocation not at random	30377	1136	.032*	.041***	.040***
			(.019)	(.019)	(.018)
5) Allocation at random	6788	319	.037	.030	.027
			(.033)	(.035)	(.035)
6) More covariates of the unemployed person	37165	1455	.024*	.021	.032**
			(.017)	(.018)	(.016)
7) With employability rating	37165	1455	.046***	.025*	.042***
			(.016)	(.016)	(.015)
8) With caseload of caseworker	37165	1455	.039***	.023*	.031**
			(.017)	(.017)	(.016)
9) Similar if unemployed caseworker	37804	816	.046***	.013	.024
			(.022)	(.023)	(.023)

See note below Table 2. Only results for propensity score matching shown, but logit results are similar. Specification 9 includes 'previous unemployment of caseworker' in the definition of similarity.

In row 7 we add the 'employability of the unemployed person' as another control variable to the main specification (of Table 1). This employability rating is a subjective judgement done by the caseworker. This employability rating could be a confounding variable if it were used to assign unemployed persons to caseworkers. On the other hand, the employability rating could be endogenous in the sense that similarity itself might affect how caseworkers rate the employability of their clients.

In this latter case we would therefore not want to control for it. In any case, the estimation results turn out to be similar with and without controlling for employability.

In row 8 we add the 'caseload of the caseworker' as another control variable to the main specification (of Table 1).[32] Caseload affects the counselling time available per client and may at the same time reflect the tightness of the (local) labour market. The results remain significantly positive.

Finally, in row 9 we augment the definition of similarity by adding another dimension. In our survey, caseworkers are asked whether they had ever been *unemployed* themselves. About two thirds of the caseworkers made this experience. We now define D=1 if the caseworker has same sex, age and education *and* had been unemployed in the past. This reduces the number of D=1 observations to 816. The estimated employment effects are similar to those of Section 4.1. (The logit estimates of the ATT are even larger than those of Section 4.1.)

5 Conclusions

In this paper, we examined the impact of similarity between caseworkers and the unemployed persons on their chances to find a job. A positive employment effect of about 3 percentage points was found when caseworker and unemployed are identical in several dimensions, including age, gender, education, nationality, mother tongue, and caseworker's own experience of unemployment. These effects were obtained by nonparametric matching estimators and were robust to a number of sensitivity analyses. The positive employment effects are mirrored by negative effects on unemployment. On the other hand, no effects on sanction days or the use of active labour market programmes were found. Hence, the employment effects of similarity imply a saving for the public un-

[32] In Table 1 we controlled for a self-reported three-year average caseload obtained from the questionnaire. Here, we construct a time-varying caseload measure, which, however, is based only on the inflows from 2003 (and thus does not contain old cases).

employment insurance funds. The reductions in unemployment benefits payments are not at the expense of increases in other costs.

Interestingly, similarity in only one or two dimensions does not seem to be sufficient to reap substantial benefits. Hence, simply matching female jobseekers to female caseworkers and male jobseekers to male caseworkers does not seem to be a useful option. To obtain advantages from selective assignment of unemployed to caseworkers, similarity on several dimensions is needed.

While our analysis is based on caseworker-to-unemployed matches that happen to be similar most likely by coincidence and not as part of some strategy, the results suggest that such a strategy could be worth implementing. A reallocation of unemployed to caseworkers could enhance reemployment outcomes. In our empirical analysis we found a 3 percentage-points increase in the employment rate, stable for at least 3 years, for those who benefitted from being counselled by a similar caseworker. (As seen in Figure 2, the average employment rates were 50% after 12 months, slightly above 60% after 24 months and 65% after 36 months.) In our main dataset (containing only Swiss unemployed and Swiss caseworkers) only 3.8% of the unemployed were matched to a similar caseworker. One may wonder by how much this matching rate could be improved by a simple re-allocation of unemployed and caseworkers (i.e. without changing the composition of the caseworkers).We examined various different re-allocation algorithms. All these algorithms had in common that re-allocation takes place only within the same employment office and that each caseworker counsels the same number of clients as before. (I.e. the caseload for each caseworker remains the same as in our original dataset. Only the unemployed are exchanged between caseworkers, within the same office.)

The algorithms proceed as follows: First, separately for each employment office, we determine for each possible *caseworker-unemployed pair* whether they are similar. Then, we seek to allocate as many as possible similarity matches subject to the condition that each unemployed is assigned to exactly one caseworker and that the caseload of each caseworker is the same as before. In seeking to maximize matches, we calculate (again separately for each unemployment office) for each unemployed how many caseworkers of the office are similar to him/her. The higher this number the easier it is to match this unemployed. Similarly, we calculate for each caseworker how many unemployed are similar to him/her. From this number we subtract the caseload of the caseworker to obtain the "excess matchability". A high excess matchability of, say, 20 means that it is easy to find matches for this caseworker. E.g., the caseworker may have a caseload of 55 while 75 of the unemployed of that office are similar to him. Low numbers and in particular negative numbers of excess matchability indicate that it is more difficult to find matches for this caseworker. E.g., a value of -10 for a caseworker with caseload 55 means that only 45 similar unemployed are available in that employment office. A matching algorithm that seeks to maximize matches may first start with the difficult cases and thereafter examines the progressively easier cases.

The first algorithm starts, in each office, with the youngest unemployed and chooses among the similar caseworkers (with number of unemployed still below their caseload) the one with lowest excess matchability. (The youngest unemployed are usually more difficult to match to a similar caseworker since in 102 out of the 103 employment offices the average age of the caseworkers is larger, often substantially so, than the average age of the unemployed.) By this algorithm, 10735 similarity matches can be formed, which represents 27.80% of the dataset. The second algorithm starts, in each office, with the youngest unemployed and assigns the youngest similar caseworker. This algorithm gives 27.96 % similarity matches. The third algorithm, starts, in each office, with that unemployed person with the least matching possibilities and matches him/her to the similar caseworker with lowest excess matchability. Thereby we obtain 27.92% similarity matches. Finally, if we start, in each office, with that unemployed person with the least matching possibilities and match him/her to the youngest similar caseworker we obtain 27.86% similarity matches.[33]

Hence, by a better allocation of unemployed to caseworkers we could increase the number of similar unemployed-caseworker pairs from a mere 3.8% to nearly 28%, who would then benefit from the positive employment effects of similarity.

Finally, we also examined the optimal allocation if we ignore the caseload constraint. In this case, the number of similarity matches can be increased to 41.66%. This is the maximum number of similarity matches if one permits that a caseworker may be assigned an unlimited number of unemployed, but takes the composition of the staff of the employment offices as given. The other about 60% of the unemployed cannot be matched to a similar caseworker, as there is no caseworker of similar age and education available in that office. Further increases in the number of matches could only be possible by hiring caseworkers that are more similar to the unemployed.

Hence, one could even consider hiring more caseworkers who are more similar to the average of the unemployed. Specifically, caseworkers with "lower secondary education and apprenticeship" and of young age are under-represented, relative to the population of unemployed.

One may also note that matching similar unemployed and caseworkers would generally be easier to achieve in larger employment offices, or when employment offices specialise on certain types of clients *and* caseworkers. Hence, smaller employment offices could be merged to bigger ones, or alternatively specialize on particular types of clients and caseworkers.

[33] Certainly, none of these algorithms described is optimal. Nevertheless, given the similarity of the results, we suspect that it would be difficult to obtain much higher matching rates through better allocations. When interpreting these results one should keep in mind that in reality the allocation of unemployed to caseworkers may be even more difficult since the composition of the jobseekers changes over the business cycle whereas the composition of the caseworkers would remain more stable. Therefore, also the fraction of well-matched unemployed-jobseeker pairs would change

Besides these policy implications, the results give support to various theories of social identity. Although we are not able to test specific elements of these theories, we suspect that more effective communication as well as trust and cooperation among people with similar background are important aspects. The magnitude of the estimated effects is quite remarkable.

Appendix A: sample selection

The population for the microeconometric analysis are all individuals who registered as unemployed anytime during the year 2003 at one of the 103 employment offices under study. In total 239,004 persons registered as new *jobseekers* during the year 2003. Notice that we consider only the first registration in 2003 for each person and subsume any further registrations within the outcome variables, i.e. the analysis is person based and not spell based.

We restrict our analysis to the 103 regional employment offices that were independently operating agencies responsible for a specific geographic area.[34] We do not include the canton Geneva in our study since in this canton the employment offices are functionally specialised according to professions and employability of the jobseekers. This is in striking contrast to all other cantons, which largely follow a geographic structuring. We further exclude five employment offices from the analysis: three offices that were newly established, split, or re-organised during the year 2003, one employment office that specialised on the difficult cases in Solothurn, and the tiny employment office in Appenzell-Innerrhoden, which did not participate in the survey.

over the seasons and particularly over the business cycle: during boom periods only very few highly educated people are unemployed, which changes during economic recessions.

[34] These employment offices had their own staff, a chief officer, and some flexibility in implementing the federal and cantonal policies. Some employment offices operate a number of smaller branches, e.g., in remote areas, or separate between short- and longer-term unemployed. These employment offices usually swap staff between these branches and pursue a common strategy. Thus, we consider them as a single entity.

After excluding those offices, 219,540 persons remain who registered in one of the 103 offices. For 215,251 persons the first caseworker was well defined, whereas for the other 4,289 no caseworker was (yet) assigned. The reason for this is that it may take several weeks until the first counselling meeting with a caseworker takes place. In total, 1,891 different caseworkers were identified in the data.

We exclude foreigners without yearly or permanent work permit, as they are not fully entitled to all services of the employment services. We also exclude individuals on disability or applying for it, and restrict the sample to the prime-age population. The remaining sample size is 136,606.

We further restrict the sample to Swiss caseworkers, Swiss unemployed and require that they speak the cantonal language as mother tongue. (We thereby also eliminate all caseworkers of unknown nationality.) Thereby we obtain a more homogenous population, which should make the interpretation of our estimated treatment effects easier.[35] Finally, we eliminate caseworkers with unknown gender (= 7 unemployed lost) and unknown age (= 266 unemployed lost), such that we retain 61,011 unemployed persons.

Unfortunately, for 22,391 cases information on education is missing for either the unemployed or the caseworker. Eliminating these observations, we obtain the final sample size of 38,620 observations. Table A.1 summarizes the number of observations lost due to these various restrictions.

Table A.2 examines how these sample restrictions affected the distributions of the observed characteristics. In the first column we show the average characteristics for the full sample of 239,004 persons. In the next column, we show the means for the sample with 61,011 unemployed persons,

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[35] A substantial fraction of the unemployed in Switzerland is foreigners. Not all of them are fully attached to the Swiss labour market, partly due to language problems (Switzerland has four official languages) or cultural differences. Furthermore, seasonal work permits, seasonal migration, repeat and return migration are not uncommon, implying that foreigners who temporally or permanently leave Switzerland are not further tracked in the unemployment data system. Such issues are of much less concern for the prime-age Swiss population.

which is obtained after applying the sample selection criteria on the basis of employment office, age, nationality, eligibility and having been unemployed long enough to be assigned to a caseworker. Clearly this changes the average characteristics somewhat, e.g. deleting all the foreigners increases earnings and reduces the fraction of married people. These selection criteria were applied in order to obtain a more homogenous population, and selection is based mostly on *exogenous* characteristics. (The only concern could be missingness of caseworkers' information including nationality, when we restrict our sample to Swiss caseworkers, as we suspect that many of these caseworkers with missing nationality are in fact Swiss.)

For many of these 61,011 cases, however, information on education is missing either for the caseworker and/or for the unemployed. (See the last two rows of Table A.1.) This is a more serious concern since education is a key variable in our analysis. We therefore show in the last two columns of Table A.2 the average characteristics for the subsample of the 38,620 persons with full information on education and for the subsample of 22,391 persons where the information on education is missing either for the unemployed or for the caseworker. From Table A.2 we see that these two subsamples are very similar in almost all characteristics, such that it appears that education is missing at random. The only exception is that those unemployed with known education have been somewhat more often unemployed during the last years. In other words, for those unemployed who have been unemployed repeatedly, the information on education is more frequently available.

The main reason why education is missing for so many unemployed is that the online data information system used by the caseworkers previously did not elicit information on school education, until the introduction of a new data warehouse system. Until then the caseworkers only had to enter information on the job qualifications and labour market experiences after the first interview with the jobseeker (and some sociodemographic information). With the new data warehouse system an extended list of variables was collected in the online system. Quite a number of caseworkers were, at least initially, reluctant to accept the additional administrative burden of entering the additional requested information (on education and other variables) into the computer. Given this background it is not surprising to see from Table A.2 that education seems to be missing at random.[36]

In addition to the descriptive analysis of missing education in Table A.2, we had examined several alternative ways of handling missing values in the education variable in an appendix to the first version of this paper, Behncke *et al.* (2008). Those results confirmed that the estimated treatment effects do not seem to be biased due to selective missingness in the education variable.

Table A.1: Sample selection

	Number of individuals	
	deleted	remaining
Population: all new jobseekers during the year 2003		**239,004**
Exclude Geneva and five other employment offices	-19,464	219,540
Exclude jobseekers not (yet) assigned to a caseworker	-4,289	215,251
Exclude foreigners without yearly or permanent work permit	-5,399	209,852
Exclude jobseekers without unemployment benefit claim	-18,434	191,418
Exclude jobseekers who applied for or claim disability insurance	-3,163	188,255
Restrict to prime-age population (24 to 55 years old)	-51,649	136,606
Exclude jobseekers whose caseworker's nationality information is missing	-12,185	124,421
Exclude jobseekers whose caseworker's gender is missing	-7	124,414
Exclude jobseekers whose caseworker's age is missing	-266	124,148
Retain only Swiss caseworkers	-10,193	113,955
Retain only Swiss unemployed	-42,922	71,033
Retain only unemployed whose mother tongue corresponds to the cantonal language	-10,022	**61,011**
Exclude unemployed whose caseworker's education is missing	-10,829	50,182
Exclude unemployed if information on their education is missing	-11,562	**38,620**

[36] We would have been more concerned about this missing education if we had included foreigners in our sample because assigning foreign school degrees to any of the available education classes in the data information system could often be ambiguous, and caseworkers facing this ambiguity might decide to not enter any information at all.

Table A.2: Descriptive statistics, sample averages

	Full sample	Sample after exogenous restrictions	Education known	Education missing
Number of unemployed	239004	61011	38620	22391
Female	.44	.45	.45	.46
Age	34.9	36.6	36.4	36.9
Swiss nationality	.64	1	1	1
Earnings (in last job, Swiss Francs)	3800	4525	4513	4547
Unemployment rate in industry (of last job)	4.83	4.57	4.58	4.56
Looking for part-time job	.09	.11	.11	.11
Not unemployed	.05	0	0	0
Not eligible for unemployment benefits	.03	0	0	0
Has exhausted unemployment benefits	< .01	0	0	0
Marital status: single	.48	.54	.54	.54
married	.42	.33	.33	.33
divorced	.09	.12	.12	.12
widowed	.01	.01	.01	.01
Number of (dependent) persons in household	1.97	1.84	1.84	1.84
Qualification: missing	0	0	0	0
unskilled	.24	.10	.10	.12
semiskilled	.16	.12	.13	.10
skilled, but no recognized degree	.05	.02	.02	.01
skilled	.55	.75	.75	.77
Employability missing	< .01	< .01	< .01	< .01
very good, no need for help	.01	.02	.02	.01
good	.09	.11	.11	.11
average	.53	.53	.52	.54
average, without need for training [a]	.14	.17	.16	.17
average, with need for training [b]	.06	.06	.05	.05
difficult to employ	.07	.05	.04	.05
difficult, without need for training [c]	.04	.04	.04	.03
difficult, with need for training [d]	.03	.02	.02	.02
difficult, with need for training in basic skills [e]	< .01	< .01	< .01	< .01
very difficult case	< .01	< .01	.01	< .01
Job position (of last job): missing	< .01	< .01	< .01	< .01
self-employed	< .01	< .01	< .01	< .01
management	.06	.10	.10	.09
qualified worker	.56	.71	.71	.71
assistant position	.29	.15	.14	.15
apprentice	.04	0	0	0
working from home	< .01	0	0	0
student (at university)	.02	0	0	0
pupil (at school)	.02	0	0	0
Size of municipality: missing	< .01	0	0	0
≥200000 inhabitants	.07	.08	.08	.08
≥150000	.11	.07	.09	.05
≥75000	.05	.05	.05	.05
≥40000	.04	.03	.04	.03
≥25000	.05	.04	.05	.04
≥15000	.17	.16	.15	.16

≥8000	.14	.14	.13	.16
≥3000	.19	.21	.20	.23
≥2000	.09	.10	.10	.11
<2000	.09	.11	.11	.10
Education of unemployed: unknown	.26	.23	0	-
primary	.04	< .01	< .01	-
lower secondary	.23	.11	.15	-
higher secondary vocational	.35	.48	.61	-
higher secondary academic	.02	.03	.03	-
tertiary vocational	.05	.09	.12	-
tertiary academic	.04	.06	.08	-
Employment and Earnings history in last 10 years				
Number of months employed in last 10 years	74.99	90.97	89.86	92.98
Number of months employed in last 5 years	43.35	48.76	48.29	49.60
Number of employment spells in last 10 years	2.03	2.21	2.28	2.10
Number of employment spells in last 5 years	1.15	1.15	1.19	1.08
Average duration (in months) of employment spells in last 10 years	18.30	23.11	23.10	23.13
Average duration (in months) of employment spells in last 5 years	10.11	11.82	12.16	11.22
Average yearly earnings in last 10 years	30338	38930	38080	40400
Average yearly earnings in last 5 years	35300	44500	43560	46130
Number of months unemployed in last 10 years	6.8	6.6	7.0	5.9
Number of months unemployed in last 5 years	3.5	3.0	3.3	2.4
Number of unemployment spells in last 10 years	1.22	1.27	1.35	1.12
Number of unemployment spells in last 5 years	.69	.64	.70	.53
Average duration (in months) of unemployment spells in last 10 years	2.71	2.59	2.70	2.39
Average duration (in months) of unemployment spells in last 5 years	1.85	1.61	1.73	1.41
Ever been self-employed in last 10 years	.19	.27	.27	.27
Ever been self-employed in last 5 years	.09	.12	.12	.12
Has been out-of-labour-force in last 10 years	.34	.40	.40	.40
Has been out-of-labour-force in last 5 years	.26	.30	.30	.29
No interruptions in administrative data in last 10 years	.55	.46	.46	.47
No interruptions in administrative data in last 5 years	.64	.58	.58	.59
Has participated in long-training programme in last 2 years	.04	.04	.04	.03
Has participated in short-training programme in last 2 years	.01	.01	.01	.01

Note: Education known means that information on educational background is available for unemployed *and* caseworker. Otherwise education is missing for either the unemployed and/or the caseworker.

[a] mittlere Vermittelbarkeit ohne Qualifikationsbedarf

[b] mittlere Vermittelbarkeit mit beruflichem Qualifikationsbedarf

[c] schwer vermittelbar ohne Qualifikationsbedarf

[d] schwer vermittelbar mit beruflichem Qualifikationsbedarf

[e] schwer vermittelbar / Qualifikationsbedarf für Grundqualifikationen

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