A Caseworker Like Me - Does the Similarity between The Unemployed and Their Caseworkers Increase Job Placements?

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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

1 Introduction[*]

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

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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 2, we provide some background on social interactions between similar individuals and the key features of the public employment system in Switzerland. Section 3 describes the databases and provides a descriptive analysis. Section 4 discusses the econometric identification strategy and the estimation methodology. Section 5 presents the results and Section 6 concludes.

2 Background

2.1 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 and 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

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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 (Neumarck 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, Levine and Leonard (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.

2.2 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.

Figure 1: Unemployment rate in Switzerland (January 1990 - November 2008)

Note: Monthly unemployment rate in %, January 1990 - November 2008, Source: Swiss National Bank Monatshefte.

2.3 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 3, 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

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

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 5.2.)

3 Data

3.1 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.

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

[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 and mobility and business cycles and vacany numbers in the industry and occupation of the unemployed person into account.

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 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 new 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 underemployed 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.

3.2 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

[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.

$Y_{i,t_0+\tau}$ 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 the subsequent 47 months, whereas only 36 months are observed for those registering in December 2003. (In Section 5.2 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 *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/college/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]

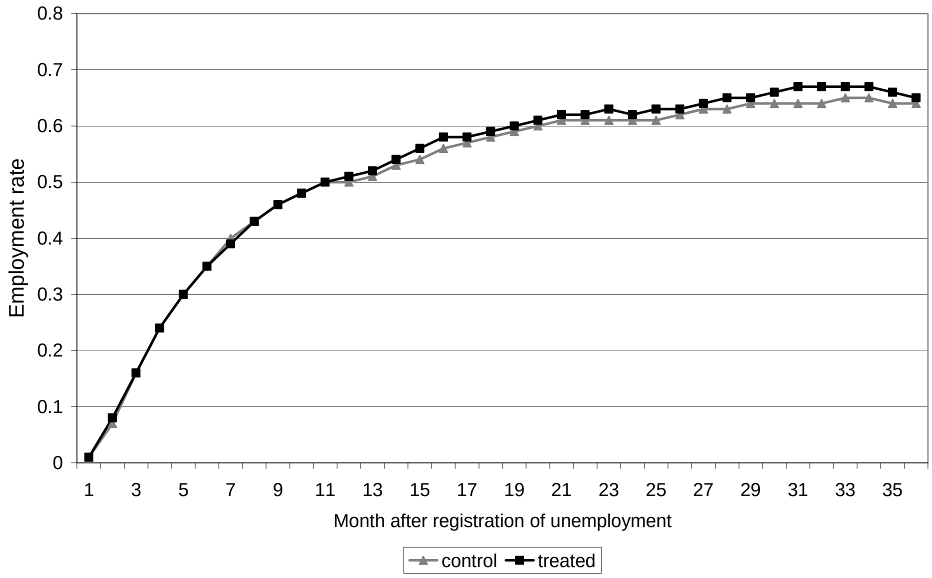
3.3 Descriptive analysis

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

[5] Observations with missing education are deleted from the sample. In an appendix to Behncke, Frölich and Lechner (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.

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.

Figure 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 Probit regression, D_i is regressed on a set of variables that could potentially explain the selection process. The Probit 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_i=1 and D_i=0 group.

When comparing characteristics of the unemployed persons in both groups, we observe clear differences for age: the unemployed in the D_i=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. We observe a significant negative coefficient on "skilled without accredited degree". This could well be a spurious result, as in the population of Swiss unemployed with local mother tongue there are less than 1% "skilled without accredited degree", with only 19 observations in the *D*=1 group.[6] Regarding employment history, unemployed in the *D*=1 group had on average more months of employment in the last ten years. This could partly reflect their higher age and thus longer labour market experience.[7] None of the other characteristics of the unemployed person are significant.

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

		Probit estimates		Sample average	
		Coeffi-cient	t-statistic	Same age, gender *and* education (*D*=1)	Different in at least one characteristic (*D*=0)
Constant	***	-3.019	12.79		
Characteristics of the unemployed clients					
Age (divided by 100)	***	0.242	8.57	.41	.36
Female		-0.025	0.41	.43	.45
Education: primary education		-0.048	0.7	.14	.15
lower secondary education and apprenticeship		0.006	0.1	.63	.61
higher secondary education		0.106	1.35	.04	.03
graduate from university/college/polytechnic		-	-	.19	.20
Qualification: unskilled		0.017	0.35	.10	.10
semi-skilled		0.023	0.53	.14	.13
skilled without degree	***	-0.346	3.45	.01	.03
skilled		-	-	.75	.75
Months employed in last ten years (divided by 10)	***	0.14	2.62	.97	.90
Monthly earnings in previous job (divided by 10000)		0.13	1.6	.50	.45
Number of dependent persons		-	-	2.0	1.8
Looking for part-time job		0.073	1.48	.13	.11
Industry of previous job: agriculture and forestry		0.171	1.25	.01	.01
construction		0.091	0.8	.07	.06
processing industry		-0.02	0.21	.14	.15
tourism		-0.107	0.86	.07	.07
services		0.055	0.62	.52	.49
public		0.106	1.21	.17	.16
other		-	-	.03	.05

Table 1 to be continued.

[6] The classification "skilled without accredited degree" is mainly for foreigners who received a formal professional degree that is not officially recognised in Switzerland.

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

Table 1: Continued …

		Probit estimates		Sample average	
		Coeffi-cient	t-statistic	Same age, gender *and* educa-tion (*D*=1)	Different in at least one charac-teristic (*D*=0)
Local labour market characteristics					
Language of employment office: French	***	-0.196	2.69	.16	.23
Italian	*	0.186	1.89	.09	.07
German		-	-	.75	.71
Registering in second half 2003 (dummy)		0.018	0.63	.58	.56
Size of municipality ≥200000 inhabitants		-	-	.09	.08
≥150000		0.071	0.69	.10	.09
≥75000		-0.204	1.43	.04	.05
≥40000		-0.13	0.95	.02	.04
≥25000		0.022	0.2	.05	.05
≥15000		-0.048	0.51	.15	.15
≥8000		-0.062	0.66	.13	.13
≥3000		0.02	0.22	.23	.20
≥2000		-0.025	0.26	.10	.10
<2000		-0.049	0.47	.11	.11
Unemployment rate of canton	**	0.073	2.06	3.83	3.75
Unemployment rate in industry (divided by 10)	**	0.227	2.28	.46	.46
Characteristics of their caseworkers					
Age in years		-	-	40	46
Female		-	-	.43	.42
Tenure in employment office (in years)	***	-0.028	3.44	5.28	5.80
Previous experience in municipality office (dummy)	*	0.189	1.71	.13	.10
Previous experience in private placement office (dummy)	**	0.126	2.22	.30	.23
Own experience of unemployment (dummy)	**	-0.132	2.45	.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.)		-0.036	0.5	.23	.25
Average caseload per month (divided by 100)		0.041	0.57	1.31	1.32
Allocation of unemployed to caseworker					
by industry	**	-0.12	2.42	.48	.52
by occupation		-0.03	0.53	.52	.54
by age		-0.224	1.43	.02	.03
by employability	**	-0.233	2.01	.04	.06
by region	***	-0.219	2.6	.08	.12
other		-0.064	0.59	.06	.07
at random		-	-	.25	.22
by alphabet		-	-	.03	.04
by caseload		-	-	.40	.41

Note: Maximum Likelihood probit regression. Dependent variable is the binary indicator for similarity D_i. 1455 observations with D_i=1, 37165 observations with D_i=0. Standard errors clustered at the caseworker level. Significance at the 1%, 5% and 10% level, respectively, is indicated by ***, **, *. Degrees of freedom 38576, log Likelihood -5865, sum of squared residuals 1382, Efron's (1978) R-square is 0.013.

With respect to local labour market characteristics, unemployed in cantons and industries with higher employment 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 jobseekers. 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.[8] 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.[9]

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[8] 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.

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

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").

A striking finding of the Probit regression is that most of the coefficients are small and insignificant with a Pseudo R^2 close to zero (0.013). 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.

4 Identification and estimation of treatment effects

4.1 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.[10] 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 outcome

$$Y^d_{i,t_0+\tau} \tag{1}$$

at some time τ after unemployment registration at time t_0 if the similarity index was set to d.

[10] 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.

To simplify the notation in the following we will always consider the outcomes relative to the time of registration and treat the time of registration t_0 as an additional covariate of person *i*. We will therefore drop the subscripts and denote the potential outcomes simply as Y_i^0 and Y_i^1. With this notation, the average treatment effect for the treated (ATT) is defined as

$$E[Y^1 - Y^0 \mid D = 1] \qquad \text{(ATT).}$$

The ATT is the treatment effect for an individual randomly drawn from the population of unemployed individuals who were counselled by a similar caseworker.[11]

For estimating the ATT, we need to identify $E[Y^0 \mid D = 1]$. Generally, we would suspect this to be different from the observed value for those who happened to have a dissimilar caseworker, i.e.

$$E[Y^0 \mid D = 1] \neq E[Y^0 \mid D = 0], \tag{2}$$

since those observations with D_i=1 and those with D_i=0 might differ in other characteristics as well. Although Table 1 did not reveal many differences, some of those are related to the employment chances of the unemployed, e.g. whether the person lives in the German or the French-speaking part.

Our identification strategy is based on controlling for all variables *X* that jointly affect *D* as well as the employment outcome Y^0, such that conditional on *X*

$$E[Y^0 \mid X = x, D = 1] = E[Y^0 \mid X = x, D = 0], \qquad \forall x \in Supp(X \mid D = 1). \tag{3}$$

This conditional independence assumption (CIA) is also referred to as ‘selection on observables’ or ‘unconfoundedness’ (e.g. Rubin, 1974). We additionally need the common support condition, which requires that every value of *X* in the *D*=1 population is also observed among the controls:

[11] 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.

$$Supp(X \mid D=1) \subseteq Supp(X \mid D=0) . \tag{4}$$

4.2 Is the conditional independence assumption plausible with our data?

The most crucial aspect of the identification strategy thus relies on being able to observe all confounding variables *X*, i.e. all variables that affected Y^0 and *D*.

As already observed in 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 (4). 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* that violate assumption (4). 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 assumption (4). 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.[12]

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 value of *D*. Which

[12] 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 5 are the effects of similarity and not of the caseworker characteristics per se.

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 affect the result.)

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.

Given two individuals 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.

Overall, Table 1, particularly the probit estimates, 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.[13] 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 was driven by selection-on-unobservables, we would expect *D* to be correlated with some observed characteristics

[13] In Section 5.5 we will also explore a specification where we use only the subset of caseworkers, who had indicated that allocation was indeed at random.

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.[14] (In Behncke, Frölich, Lechner (2008) we explored even larger regressor sets.)

4.3 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 usually binary, Maximum Likelihood logistic regression is used and average marginal effects are reported. These average marginal effects are 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 in order to obtain the ATT. 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 specification procedure.[15] 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,

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[14] 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.

[15] 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.

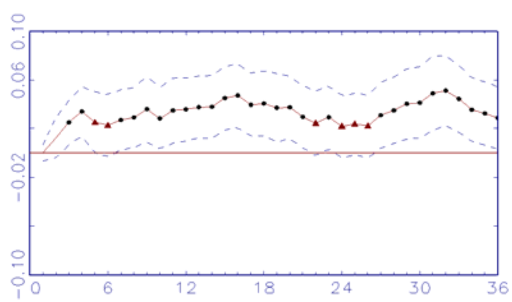
Frölich, Lechner, 2008). Inference for the PSM estimator is based on the bootstrap by re-sampling caseworkers (together with all their clients) to account for possible dependencies among the unemployed counselled by the same caseworker. Following MacKinnon (2006), the t-statistic is bootstrapped and the bootstrap p-value is based on symmetric rejection regions of the t-statistic. Therefore, we report only p-values in the following tables, and suppress the standard errors to save space.

5 Empirical results

5.1 Effects of similarity on employment

Figure 3 shows the effects of similarity on employment (in a non-subsidized 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.

Figure 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. 1455 observations with D_i=1, 37165 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.

5.2 Effects of similarity on alternative outcomes

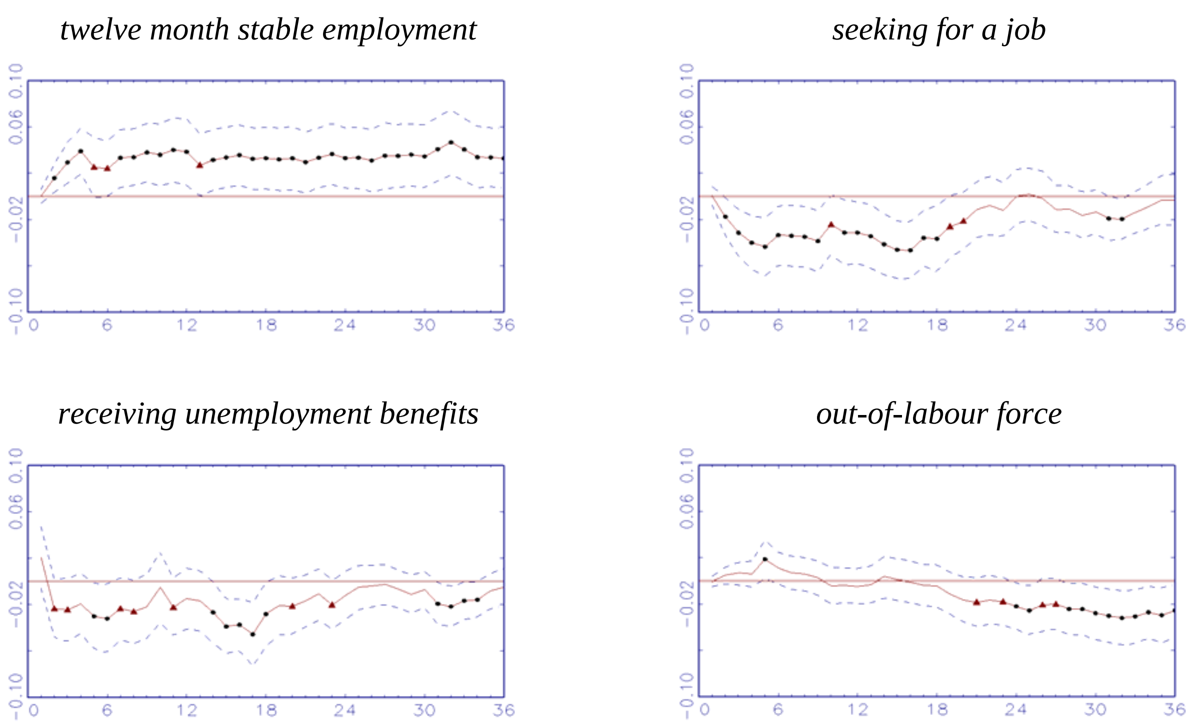
In this section, we show the effects of similarity on other outcome variables, including alternative definitions of employment as well as the use of sanctions and active labour market programmes. We first examine the stability of employment in order to judge whether the results of the previous section 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. The first graph in Figure 4 shows positive effects on stable employment. Moreover, we virtually find no differences to Figure 3. This suggests that having a similar caseworker also has a positive effect on job stability. (We find similar results when defining stable employment as an employment spell of at least 3 or 6 months duration, respectively.)

Further, we examine the effects of similarity on job seeker status in Figure 4. 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 currently 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 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 18 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 any 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 treated individuals receive fewer benefits from the unemployment insurance.

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. 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 due to reductions in out-of-labour force.

Figure 4: Effects of similarity in age, gender and education on employment on



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.[16][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_i=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.)

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

		Month 6		Month 12		Month 18		Month 24		Month 36	
		ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value
Employed	psm	0.023	0.076	0.036	0.01	0.041	0.005	0.022	0.096	0.029	0.028
	logit	0.025	0.054	0.035	0.009	0.036	0.01	0.031	0.031	0.035	0.007
Long-term employed	psm	0.024	0.052	0.038	0.005	0.033	0.014	0.033	0.013	0.033	0.009
	logit	0.033	0.006	0.037	0.006	0.031	0.021	0.037	0.010	0.037	0.005
Seeking for a job	psm	-0.033	0.010	-0.031	0.022	-0.037	0.009	0.000	0.995	-0.003	0.813
	logit	-0.032	0.024	-0.026	0.043	-0.026	0.028	-0.008	0.505	-0.008	0.391
Receiving UI benefits	psm	-0.032	0.034	-0.015	0.258	-0.028	0.043	-0.012	0.226	-0.005	0.513
	logit	-0.021	0.138	-0.017	0.199	-0.017	0.129	-0.01	0.322	-0.008	0.248
Out-of-labour force	psm	0.011	0.107	-0.004	0.568	-0.004	0.656	-0.022	0.040	-0.026	0.028
	logit	0.008	0.270	-0.007	0.361	-0.008	0.362	-0.021	0.040	-0.027	0.026
Exhausted UI benefits	psm	0.004	0.515	-0.004	0.205	0.000	0.925	0.009	0.102	0.004	0.397
	logit	-0.003	0.459	-0.004	0.177	-0.002	0.453	0.003	0.399	0.003	0.460

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). 1455 observations with D_i=1, 37165 observations with D_i=0. Inference is based on bootstrapping the t-statistic via re-sampling caseworkers for matching and on robust clustered standard errors for logit.

Table 2 summarizes 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 conse-

[16] 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.

[17] Another reason could be seasonality effects that would explain dynamics that have a period of 12 months.

quences 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.

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.[18]

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. Table 3 shows the effects of similarity on the number of sanction days in months 1, 3, 6, 12, 18 and 24 of the unemployment spell and on the total number of sanction days during the first year of unemployment. All these effects turn are insignificant.

Table 3: Effects of similarity in age, gender and education on sanction days

Month 1		Month 3		Month 6		Month 12		Month 18		Month 24		Months 1 to 12	
ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value
0.003	0.977	0.161	0.109	-0.059	0.170	-0.027	0.413	0.028	0.381	0.016	0.473	-0.077	0.817

See note below Figure 3.

As a second channel, we examine the use of active labour market programmes (ALMP). Table 4 shows the effects of similarity on the use of ALMP. We first observe a small negative effect on the probability of participation in (at least one) ALMP until the end of 2006, which is significant only at the 10% level and only for the logit estimator. When we examine the effects on the first ALMP, we

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

observe a small negative effect on the participation in an employment programme, which is significant only for the logit estimator. However, the effect on participating in an employment programme is insignificant when we examine the (up to) first three ALMP together. Hence, the effects on the use of ALMP are very weak.

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 counseling, rather than through additional spending on ALMP. If anything, similarity leads to fewer ALMP and perhaps also to delays in using long lasting employment programmes.

Table 4: Effects of similarity in age, gender and education on active labour market programmes

	psm ATT	p-value	logit ATT	p-value
Participated in (at least one) ALMP after registration in 2003 (until end of 2006)	-0.015	0.468	-0.026	0.071
First programme after registration in 2003 is:				
Job search training	-0.004	0.814	-0.013	0.339
Personality courses	-0.003	0.622	0.000	0.994
Language skills training	-0.004	0.472	-0.007	0.120
Computer skills training	-0.002	0.728	0.001	0.858
Vocational training	0.002	0.765	-0.003	0.607
Employment programme or internship	-0.005	0.255	-0.007	0.036
Within the first three programmes, participated at least once in				
Job search training	-0.008	0.597	-0.015	0.250
Personality courses	0.000	0.963	0.001	0.841
Language skills training	-0.004	0.561	-0.005	0.414
Computer skills training	-0.005	0.538	-0.003	0.739
Vocational training	-0.001	0.949	-0.008	0.318
Employment programme or internship	0.009	0.292	0.002	0.770

See note below Table 2.

Job search training is often short-term and provides participants with training in effective job search techniques. Personality courses help participants to position themselves in the labour market. Language skills training covers courses in foreign languages as well as alphabetization courses. Computer skills training includes mainly internet courses and office applications. Vocational training provides applicants with updated skills within their occupation. Employment programmes take place within a sheltered labour market. Internships and work in practice firms are also included in this category.

5.3 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 (=1455 observations) to all other observations who differed in *at least one* characteristic (=37165 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.

In Table 5, we estimate the effects for each of the 22 different possible combinations of treatment and control group. In the first block, we estimate the effects from similarity in all 3 versus similarity in exactly 0, 1 or 2, respectively, characteristics. In the first row, the effect of similarity in *all three* characteristics (=1455 observations) versus dissimilarity in *all three* characteristics (=8438 observations) is shown. In the second row, the effect between similarity in *all three* characteristics (=1455 observations) versus dissimilarity in sex and age and similarity in education is shown and so on.

An interesting finding emerges from the first block. All of these effects are in the range of roughly 2 to 5 percentage points and mostly significant. We do not observe a clear pattern as a function of the *degree* of similarity: The effects of similarity in 3 versus 2 characteristics are not systematically smaller or larger than the effects of 3 versus 1 or 3 versus 0.[19] On the other hand, the blocks below show the effects from similarity in 2 characteristics versus 0 or 1, respectively, followed by the effects of similarity in 1 versus 0 characteristics. Nearly all these estimates are insignificant.

To summarize these results, the final rows of Table 5 give *dose response* estimates. 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. (Perhaps a small effect in month 6.) On the other hand, increasing similarity from 2 to 3 characteristics, increases employment by about 3 to 4 percentage points.

[19] 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 unemployed persons.

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 job-seeker feel a 'similarity' or affection/sympathy.

Table 5: Employment effects of different definitions of similarity

Definition of control group (D=0)				Month 6		Month 12		Month 18		Month 24		Month 36	
same	same	same		ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value
sex	age	edu		Treatment: same sex and same age and same education (1455 individuals)									
0	0	0	psm	0.03	0.034	0.017	0.34	0.016	0.591	0.023	0.114	0.028	0.104
			logit	0.038	0.015	0.035	0.019	0.042	0.007	0.033	0.036	0.038	0.007
0	0	1	psm	0.005	0.932	0.021	0.214	0.029	0.09	0.017	0.329	0.017	0.562
			logit	0.026	0.088	0.036	0.023	0.046	0.005	0.031	0.077	0.04	0.012
0	1	0	psm	0.054	0.017	0.026	0.09	0.037	0.019	0.022	0.155	0.043	0.003
			logit	0.042	0.011	0.035	0.047	0.042	0.015	0.017	0.326	0.031	0.054
1	0	0	psm	0.031	0.043	0.04	0.009	0.039	0.006	0.038	0.055	0.035	0.041
			logit	0.032	0.031	0.044	0.004	0.045	0.004	0.041	0.01	0.035	0.011
0	1	1	psm	0.03	0.11	0.029	0.173	0.03	0.113	0.018	0.334	0.022	0.246
			logit	0.01	0.606	0.043	0.04	0.027	0.147	0.02	0.319	0.027	0.171
1	0	1	psm	0.026	0.05	0.028	0.051	0.028	0.051	0.013	0.387	0.023	0.099
			logit	0.024	0.1	0.031	0.045	0.035	0.023	0.029	0.061	0.028	0.047
1	1	0	psm	0.034	0.035	0.066	0.000	0.054	0.005	0.049	0.001	0.060	0.001
			logit	0.02	0.184	0.057	0.001	0.031	0.058	0.034	0.038	0.044	0.005

Table 5 to be continued.

Table 5 continued …

Definition of control group (D=0)				Month 6		Month 12		Month 18		Month 24		Month 36	
Treatment: same education and same age, but different sex (1046 individuals)													
0	0	0	psm	0.01	0.502	-0.037	0.113	-0.002	0.906	0.003	0.864	0.007	0.707
			logit	0.025	0.131	-0.008	0.64	0.009	0.571	0.01	0.562	0.007	0.656
0	0	1	psm	-0.005	0.804	-0.029	0.177	-0.003	0.847	-0.003	0.847	0.003	0.871
			logit	0.014	0.423	-0.006	0.711	0.012	0.476	0.008	0.654	0.009	0.608
0	1	0	psm	-0.003	0.866	-0.043	0.063	-0.030	0.229	-0.043	0.068	-0.019	0.393
			logit	0.027	0.152	-0.008	0.682	0.007	0.678	-0.003	0.886	-0.002	0.9
1	0	0	psm	0.019	0.245	-0.023	0.181	-0.004	0.792	-0.010	0.575	-0.007	0.656
			logit	0.024	0.154	0.002	0.917	0.014	0.359	0.019	0.257	0.007	0.65
Treatment: same education and same sex, but different age (6086 individuals)													
0	0	0	psm	0.011	0.247	-0.002	0.824	0.005	0.561	-0.002	0.764	0.001	0.91
			logit	0.016	0.096	0.002	0.859	0.006	0.491	0	0.954	0.003	0.727
0	0	1	psm	-0.006	0.524	0.012	0.429	0.009	0.36	0.003	0.755	0.007	0.496
			logit	-0.003	0.772	0.004	0.714	0.007	0.477	0.001	0.931	0.008	0.431
0	1	0	psm	0.011	0.68	-0.023	0.328	-0.021	0.377	-0.026	0.273	0.01	0.662
			logit	0.015	0.292	0.001	0.965	0	0.97	-0.014	0.264	-0.004	0.72
1	0	0	psm	0.000	0.976	-0.007	0.556	-0.001	0.958	-0.005	0.587	-0.005	0.677
			logit	0.003	0.724	0.009	0.273	0.005	0.565	0.008	0.328	0.005	0.542
Treatment: same sex and same age, but different education (3103 individuals)													
0	0	0	psm	0.01	0.337	-0.036	0.002	-0.007	0.587	-0.017	0.118	-0.013	0.2
			logit	0.016	0.174	-0.026	0.031	0.006	0.621	-0.011	0.342	-0.013	0.248
0	0	1	psm	0.023	0.116	-0.015	0.372	0.022	0.186	-0.002	0.944	-0.003	0.885
			logit	0.002	0.853	-0.022	0.106	0.013	0.346	-0.01	0.47	-0.008	0.578
0	1	0	psm	0.022	0.063	-0.02	0.132	0.004	0.778	-0.019	0.177	-0.019	0.105
			logit	0.017	0.221	-0.027	0.062	0.002	0.886	-0.022	0.125	-0.018	0.189
1	0	0	psm	-0.01	0.657	-0.027	0.028	-0.008	0.526	-0.011	0.364	-0.02	0.043
			logit	0.005	0.606	-0.017	0.114	0.008	0.476	-0.002	0.857	-0.014	0.18
Treatment: same education, but different sex and age (4255 individuals)													
0	0	0	psm	0.006	0.513	-0.008	0.384	0.001	0.955	-0.001	0.926	-0.004	0.677
			logit	0.016	0.104	-0.002	0.834	0	0.968	0	0.972	-0.003	0.743
Treatment: same age, but different sex and education (2211 individuals)													
0	0	0	psm	-0.012	0.457	-0.003	0.796	0.002	0.863	0.007	0.564	0.002	0.822
			logit	0.003	0.819	0.006	0.657	0.007	0.554	0.017	0.143	0.012	0.307
Treatment: same sex, but different age and education (12026 individuals)													
0	0	0	psm	0.021	0.016	-0.002	0.912	-0.002	0.827	-0.008	0.231	-0.010	0.147
			logit	0.01	0.193	-0.009	0.249	-0.001	0.88	-0.009	0.224	-0.005	0.527
Degree of similarity				Dose Response to different degrees of similarity									
1 versus 0			psm	0.017	0.016	-0.006	0.314	0.003	0.663	0.001	0.923	0.000	0.976
			logit	0.010	0.145	-0.005	0.474	0.001	0.925	-0.004	0.550	-0.002	0.753
2 versus 1			psm	-0.004	@@	-0.013	@@	0.001	@@	-0.004	@@	-0.004	@@
			logit	0.001	0.821	-0.005	0.408	0.004	0.500	0.001	0.903	-0.001	0.892
3 versus 2			psm	0.035	0.013	0.046	0.003	0.049	0.003	0.050	0.012	0.040	0.006
			logit	0.021	0.097	0.038	0.008	0.030	0.035	0.026	0.069	0.031	0.019

See note below Table 2. The control group with different sex, different age and different education contains 8438 observations.

Another interesting finding is that - while three characteristics are necessary to generate positive effects – not all of them matter to the same extent. From the first block, it can be seen that similarity in education might be more important than similarity in other characteristics: The impact of similarity on employment is smaller if individuals in the control group share at least the same education (rows 2, 5, 6) compared to control groups who share the same gender and age.[20]

5.4 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 within subpopulations. In the following table, only the effects for the definition of similarity as in Sections 5.1 and 5.2 are shown (and not for the various alternative definitions of Section 5.3). 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 tend to be a little bit larger for female unemployed. Women may thus be more affected by belonging to the same social group.

Next we stratify by age. The effects are positive for both age groups, but, from month six onwards, are much larger for the younger unemployed (24 to 35 years).

Stratifying by educational attainment of the unemployed, we distinguish between low and high educated unemployed. For the high educated unemployed, the effects of similarity are always positive. For the low educated unemployed, we note that there are only very few (200) observations with D_i=1, because most caseworkers have achieved a higher educational degree. Due to this small sam-

[20] In particular, the cumulated effect over all 36 months is around 4 percentage points in rows 3 and 4, while it is 2 percentage point in row 2. Thus, unemployed individuals gain twice as much in terms of employment when their caseworker becomes also similar in education compared to becoming similar in gender or age.

ple size, the estimated effects are very noisy, mostly positive but seldom significant. Because of these noisy estimates it is difficult to ascertain whether similarity is more important for low or for high educated unemployed. When examining the point estimates for all months (not shown), it seems that the effects for the low educated decline over time from about 4 percentage points in the first few months to about zero by month 15 and oscillate around zero afterwards. The effects for the high unemployed, in contrast, are sustained until month 36 (with a small dip around month 24).

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

				Month 6		Month 12		Month 18		Month 24		Month 36	
	n_0	n_1		ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value
Subpopulations defined by characteristics of the unemployed individual													
Female	16706	627	psm	0.027	0.149	0.04	0.048	0.024	0.247	0.043	0.143	0.056	0.059
			logit	0.028	0.119	0.036	0.072	0.021	0.296	0.02	0.352	0.029	0.146
Male	20459	828	psm	0.02	0.272	0.036	0.067	0.041	0.026	0.032	0.085	0.025	0.150
			logit	0.024	0.183	0.031	0.079	0.046	0.013	0.035	0.058	0.037	0.024
Age 24 to 35	19291	443	psm	-0.001	0.980	0.081	0.007	0.043	0.073	0.051	0.034	0.056	0.016
			logit	0.021	0.421	0.06	0.015	0.042	0.068	0.056	0.021	0.048	0.035
Age 36 to 55	17874	1012	psm	0.028	0.064	0.014	0.417	0.036	0.037	0.008	0.684	0.014	0.605
			logit	0.028	0.07	0.027	0.094	0.037	0.032	0.021	0.205	0.030	0.060
Low educated	5755	200	psm	0.030	0.373	0.053	0.116	0.034	0.340	0.014	0.697	-0.022	0.594
			logit	0.012	0.716	0.061	0.086	0.039	0.277	0.043	0.239	0.013	0.723
High educated	31410	1255	psm	0.023	0.115	0.021	0.159	0.032	0.043	0.000	0.987	0.028	0.055
			logit	0.027	0.06	0.03	0.039	0.034	0.023	0.026	0.069	0.036	0.008
Subpopulations defined by regional (=canton) or industry-specific unemployment rate													
Low regional unemployment	18924	682	psm	-0.012	0.793	-0.001	0.992	-0.032	0.670	-0.008	0.880	0.012	0.773
			logit	0.023	0.206	0.043	0.022	0.022	0.254	0.032	0.099	0.045	0.018
High regional unemployment	18241	773	psm	0.033	0.082	0.032	0.118	0.053	0.008	0.025	0.190	0.044	0.055
			logit	0.022	0.216	0.029	0.134	0.046	0.014	0.03	0.12	0.026	0.124
Low industrial unemployment	16504	633	psm	-0.006	0.748	-0.003	0.892	-0.012	0.584	-0.005	0.817	0.002	0.906
			logit	0.004	0.802	0.023	0.234	0.009	0.651	0.023	0.241	0.029	0.116
High industrial unemployment	20661	822	psm	0.039	0.035	0.021	0.333	0.040	0.050	0.013	0.599	0.023	0.259
			logit	0.042	0.016	0.045	0.016	0.058	0.003	0.036	0.06	0.039	0.027

See note below Table 2. 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 unemployment rate is low, the effects of similarity are small (logit) or zero (PSM), and mostly 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 more difficult environments. 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.)

5.5 Robustness analysis

In this section, we examine the robustness of our main estimation results of Section 5.1 to potential violations of the conditional independence assumption (3).[21] While we found that selection on observables is negligible (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. 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 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 percent D=1 observations. This leaves us with 37 caseworkers.[22] Table 7 shows positive effects of about 3 to 5 percentage points, which thus tend

[21] In an appendix to Behncke, Frölich and Lechner (2008) we also examined different ways of handling missingness in the education variable and obtained rather stable results.

[22] 686 caseworker have not a single similar client and the remaining 427 caseworker have less than 5 percent similar clients.

to be even larger than the results of Section 5.1. (No p-values for PSM are reported since the large number of caseworker fixed effects led to numerical problems in the bootstrap replications.)

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.[23] The estimates remain nearly unchanged.

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 are somewhat smaller for month 6 but remain highly significant at later months.

As an alternative to employment office fixed effects we can pursue an alternative approach to examine the potential degree of "selection-on-unobservables". In a questionnaire, the caseworkers were asked about how unemployed persons are allocated to the caseworkers. We split the sample according to the caseworkers' answers. The first subsample consists of those unemployed whose case-

[23] 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

worker 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.[24] 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 somewhat noisy, particularly since the second subpopulation contains only 319 observations with D=1. The effects are positive for both subpopulations and suggest that potential selection-on-unobservables might overall not be a big concern.[25]

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 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,

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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.

[24] 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.

[25] The PSM estimates tend to be larger in the “random allocation” group, but the logit estimates do not support this claim. Due to the small sample size, we cannot draw firm conclusions.

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

	Number of observations			Month 6		Month 12		Month 18		Month 24		Month 36	
	n_0	n_1		ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value	ATT	p-value
1) Caseworker fixed effects	2244	401	psm	0.041	-	0.033	-	0.023	-	0.047	-	0.022	-
			logit	0.075	<0.001	0.053	0.044	0.049	0.029	0.049	0.077	0.047	0.016
2) More caseworker covariates	37165	1455	psm	0.041	0.012	0.034	0.014	0.035	0.007	0.021	0.115	0.032	0.010
			logit	0.025	0.053	0.036	0.009	0.036	0.010	0.031	0.028	0.035	0.006
3) Employment office fixed effects	37165	1455	psm	0.019	-	0.012	-	0.013	-	0.007	-	0.022	-
			logit	0.022	0.061	0.035	0.008	0.035	0.008	0.031	0.025	0.035	0.005
4) Allocation not at random	30377	1136	psm	0.014	0.347	0.016	0.538	0.034	0.031	0.027	0.127	0.035	0.025
			logit	0.019	0.187	0.041	0.012	0.039	0.018	0.040	0.015	0.041	0.005
5) Allocation at random	6788	319	psm	0.083	0.021	0.071	0.101	0.054	0.160	0.021	0.662	0.034	0.359
			logit	0.040	0.147	0.026	0.289	0.023	0.362	-0.003	0.915	0.018	0.502
6) More covariates of the unemployed	37165	1455	psm	0.031	@@	0.020	@@	0.014		0.024		0.036	
			logit	0.024	0.057	0.035	0.010	0.035	0.010	0.030	0.032	0.034	0.008
7) With employability rating	37165	1455	psm	0.045	0.013	0.045	0.002	0.042	0.012	0.024	0.067	0.034	0.009
			logit	0.024	0.068	0.034	0.012	0.035	0.013	0.029	0.041	0.033	0.009
8) With caseload of caseworker	37165	1455	psm	0.027	0.034	0.045	0.005	0.025	0.077	0.025	0.066	0.033	0.012
			logit	0.022	0.076	0.035	0.011	0.034	0.013	0.03	0.034	0.034	0.007
9) Similar if unemployment caseworker	37804	816	psm	0.042	0.027	0.035	0.096	0.028	0.166	0.018	0.409	0.043	0.020
			logit	0.028	0.081	0.051	0.005	0.041	0.018	0.041	0.031	0.042	0.017

See note below Table 2. 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).[26] 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 (and slightly larger) to those of Section 5.1.

6 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 to 4 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. In addition to propensity score matching, Maximum Likelihood logistic regressions gave very similar, often somewhat larger, effects that where estimated more precisely. 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 unemployment insurance funds. The reductions in unemployment benefits payments are not at the expense of increases in other costs.

[26] 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).

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. This may be easier to achieve in larger employment offices, i.e. when smaller offices are merged, or when employment offices specialise on certain types of clients *and* caseworkers.

Beside the obvious policy implication, 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.

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