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Stefanie Behncke
Markus Frölich
Michael Lechner

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Stefanie Behncke
SEW, University of St. Gallen

Markus Frölich
University of Mannheim,
SEW, IFAU and IZA

Michael Lechner
SEW, University of St. Gallen,
CEPR, ZEW, PSI, IAB and IZA

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

A Caseworker Like Me: Does the Similarity between Unemployed and Caseworker Increase Job Placements?*

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 4 percentage points if caseworker and unemployed 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.

JEL Classification: J64, J68, C31

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

Corresponding author:

Michael Lechner
SEW
University of St. Gallen
Varnbühlstr. 14
CH-9000 St. Gallen
Switzerland
E-mail: Michael.Lechner@unisg.ch

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1 Introduction

Most research on the determinants of unemployment durations has focussed 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 or her 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 reemployment probabilities. We find a positive employment effect of about 4 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. Hence, similarity in several dimensions is needed for obtaining positive employment effects.

The effects of social identity, social distance, and social interactions have been examined in various disciplines, including economics, psychology, pedagogy, and sociology. While a lot of this research examines peer group choice or endogenous group formation, we focus on the *similarity* between caseworker and the unemployed person with respect to various characteristics, in the context of an arguably exogenous group formation: A person becomes unemployed, registers at the nearest employment office, and is assigned to a caseworker. Several different models of *social identity* have been developed to explain how similarity might influence behaviour and thus employment outcomes (Tajfel and Turner, 1979, Akerlof and Kranton, 2000).

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). 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 or her job search activities and outcomes 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, for example, often difficult for the caseworker to prove factually that an unemployed person displayed insufficient search effort.

For the reasons mentioned above, belonging to the same social group 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. It is conceivable that it would also improve the screening of vacancies and that it increases the intensity and effectiveness of job placement activities.

So far, the effects of similarity between 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, education, and previous unemployment experience 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 covariates and to measure the placement of the unemployed in the job market.

In our empirical analysis, we define a caseworker to be similar to his client, if he/she has the same gender, the same educational level, and a similar age. It is also desirable to ensure homogeneity with respect to cultural background: In most of our analysis, we only consider the subsample of Swiss caseworkers and Swiss unemployed whose mother tongue is the main cantonal language. Using matching estimators to control for many other socio-economic differences that are available in this rich data, we find positive employment effects of 4 percentage points when having a caseworker who is similar in all these dimensions. Similarity in fewer characteristics leads to smaller (and often insignificant) effects. With similarity in only two dimensions, (smaller) positive effects remain for having the same education and age, and for having the same education and gender. However, gender and age alone lead to mostly insignificant effects. Finally, similarity with respect to only one characteristic gives very small and insignificant 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. Similarity on several dimensions is needed. This also seems to be in line with social identity theories where people may identify themselves with a social group according to a multidimensional characteristic vector.

The robustness of our results is confirmed in a broader sample that includes foreigners (adding nationality and language as further dimensions of 'similarity'). Furthermore, the results are also robust when adding a further dimension to the definition of similarity by taking into account whether caseworkers share a history of unemployment with their clients.

Regarding policy conclusions, these findings suggest that a targeted allocation of unemployed to caseworkers could enhance employment outcomes. The relationship between unemployed and caseworker matters and a similar social background can enhance it. Such a targeted allocation would be easier to achieve in larger units, i.e. when smaller offices are merged, or when employment offices specialize in certain types of clients *and* caseworkers.

In Section 2, we review the literature on social identity and social interaction from various disciplines. In Section 3, the key features of the public employment system in Switzerland are presented. Section 4 describes the databases and provides a descriptive analysis. Section 5 discusses the econometric identification strategy and the estimation methodology. Section 6 presents the results and Section 7 concludes. Sample selection issues and further details on the matching estimator are described in the Appendix. An Internet Appendix provides further details on sensitivity checks and robustness (available on http://www.alexandria.unisg.ch/Export/dl/44220.pdf).

2 Social groups, social identity and social interactions

In this section, we examine various models on social groups and social identification from different disciplines. Psychology devoted substantial interest to concepts of social identity and social interactions. Educational science paid special attention to effects of teacher's gender or race on schooling outcomes and to peer effects between students. In economics, concepts like peer effects, trust, and social preferences recently received considerable interest. By including social identity into the utility function, Akerlof and Kranton (2000) laid down an economic foundation that gives insights in many economic problems. Let us consider the different fields in more detail:

Psychology: Already Freud (1921) argued that investigating the psychology of groups is crucial for understanding individual behaviour. Group phenomena may already arise in groups of two persons. The Ego perceives an important analogy to the other and identifies to the group by affiliating attributes to the own Ego. Belonging to a group provides feelings of security and power. It results in a tendency to be guided by affections in the group. Subsequently, social psychologists investigated the impacts of groups on individual behaviour in more detail by implementing experiments (e.g., Sherif et al., 1961, Tajfel, 1970, and Brewer, 1979, for a review). Accordingly, the mere perception of belonging to two 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. Based on these experiments, Tajfel and Turner (1979) developed the social identity theory. According to it, individuals identify to social groups and tend to behave as part of groups. They have an inbuilt tendency to categorize themselves into one or more groups and to develop part of their own identity based on being member of that group. In addition to being themselves member of that group, they also tend to delineate boundaries to other groups.

Education: Research in the education sciences and pedagogy has devoted substantial attention to the possible interaction effect between teachers' and students' ethnicity or gender. Several explanations have been put forward why we might expect effects of race and/or gender interactions. Among them, there are *role model effects*: Students with the same gender or ethnicity as their teacher more easily identify with their teacher. As a result, they are more encouraged and perform better. Many argue that the absence of female or black teachers is partly responsible for explaining gaps in test scores (AAUW, 1992, King, 1993). *Stereotype threat* is another possibility for effects of teachers' race or gender: students' achievement may be impaired by the fear that their behaviour will confirm an existing stereotype of the group with which they identify (e.g. see Steele, 1997, and Spencer et al., 1999). Teachers may also react differently to students' race and gender: prior expectations of students' abilities on grounds of their gender might work as self-fulfilling expectations (Ehrenberg et al., 1995, Ferguson, 1998, Lindahl, 2007).

With respect to ethnicity, there is evidence for positive effects. Using a randomised experiment, Dee (2004) finds positive effects of own-race teacher on test scores for black and white students in Tennessee. Similarly, Lindahl (2007) finds positive effects on Mathematics performance for ethnic minority students in Sweden when the share of ethnic minority teachers increases.

Regarding gender, there is mixed evidence. With respect to secondary education, Dee (2007) finds that assignment to a same gender teacher significantly improves the achievement of both girls and boys as well as teachers' perceptions of student performance and student engagement with teachers' subject. Lindahl (2007) finds positive same gender effects for Mathematics tests scores in Sweden, while she does not find significant effects for test scores in Swedish and English. In contrast, Holmlund and Sund (2005) find no significant effects on grade performance for secondary students in Sweden. Regarding tertiary education, Neumark and Gardecki (1998) find that female Economics PhD students with female mentors spent less time in graduate school, while their initial job placement is not affected. In contrast, Hilmer and Hilmer (2007) do not find evidence of statistical differences between female PhD students working with female advisors compared to working with male advisors. Hoffmann and Oreopoulos (2007) find that teacher gender plays little or no role for college students' achievement or field of study choice, while Bettinger and Long (2005) find that female instructors have a positive effect on course selection.

In a related literature, the effects of other students' characteristics on own outcomes are examined. Again, evidence is mixed, and ranges from zero to relatively large effects (see Ammermueller and Pischke, 2006, for a summary). Frölich and Michaelowa (2005) find positive peer effects on learning resulting from textbooks owned by classmates.

Economics: Quite recently, economists dedicated their interest to the impact of social groups on individual behaviour. Indeed, there is evidence that peer effects matter e.g. for dropping out of high school, schooling outcomes, teenage pregnancy, crime, drug, tobacco, alcohol use, as well as productivity, employment and unemployment (Crane, 1991, Case and Katz, 1991, Glaeser et al., 1996, Topa, 2001, Brock and Durlauf, 2001, Ammermueller and Pischke, 2006, Araujo et al., 2004, Falk and Ichino, 2006, Mas and Moretti, 2006, Cutler and Glaeser, 2007). Typically, peer effects arise in groups of *similar* individuals, such as classmates, colleagues, or neighbours.

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) found 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. It is frequently observed that people initiate and maintain personal relationships based on fairness, trust, and gift-exchange. This phenomenon was not only studied in numerous laboratory experiments, but also in real-life settings (Falk, 2007). People are offering and repaying gifts without external obligations.[1] An interesting experiment in this respect is Tyran and Feld (2006), who analyse ‘compliance when legal sanctions are non-deterrent’. They find evidence that people tend to comply with the law and social norms if they expect others to do so as well. Unemployed jobseekers are required by law to invest sufficient job search effort and to accept any offered job, provided it satisfies certain minimum requirements. Otherwise, legal sanctions in the form of suspension of benefits apply. For the caseworker, however, it is often difficult to prove that his client displayed insufficient search effort or sabotaged a

[1] See Akerlof (1982), Falk et al. (2003), Feld and Frey (2002), Fehr and Gächter (2000a, b), Fehr et al. (1997), Fehr et al. (1993), Fischbacher et al. (2001), Frey and Meier (2004), Gächter and Falk (2002), Gächter et al. (2004), Kahneman et al. (1986).

potential but disliked job offer, e.g. by displaying anti-social behaviour during the job talk. To some extent, the caseworker therefore has to appeal to and elicit the commitment of the jobseeker.

Akerlof and Kranton (2000) provide theoretical support for the above outlined empirical evidence on the explanatory power of group interactions on individual decisions. They incorporate the concept of identity in the utility function as a motivation for behaviour. Identity is associated with different social categories (such as man and woman), and how people in these categories should behave. Violating these prescriptions evokes discomfort in oneself and in others. Furthermore, they argue that much conflict occurs because people with different prescriptions or identities come into contact.

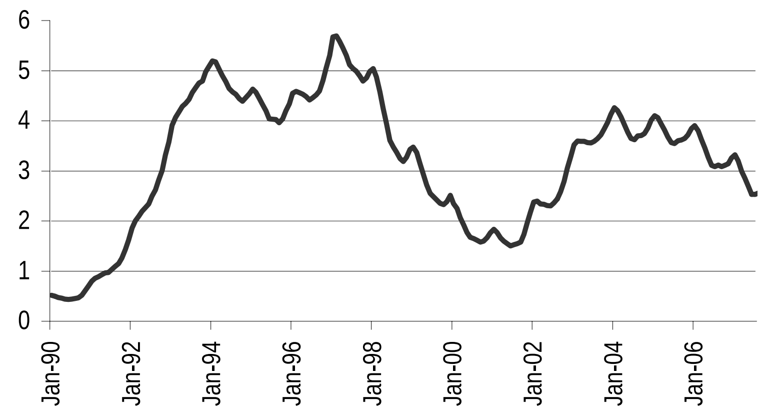
3 The public employment services in Switzerland

3.1 General

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, about 3000 in number, were consolidated to around 100 regional employment offices (REO). Compared to the previous municipal offices, which were largely concerned with administering unemployment benefits, these regional offices aimed at providing professional services with respect to counselling, placement, activation, and training.

With the exception of Geneva (and one particular employment office in the canton of Solothurn), all regional employment offices were geographically organized 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. In contrast, the employment offices in Geneva were organized according to skills and professions. We will use only the geographically organized offices for our empirical analysis.

Figure 1: Unemployment rate in Switzerland (January 1990 - December 2007)



Note: Monthly unemployment rate in %, January 1990 - December 2007, Source: Swiss National Bank Monatshefte.

3.2 Allocation of unemployed to caseworkers by gender, age, and education

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. The unemployed persons are assigned to caseworkers based on various criteria, mostly caseload or industry of previous occupation. In the survey, described in Section 4, caseworkers and office managers were asked about the criteria used for the allocation of unemployed persons to caseworkers. Table 1 shows the answers given by the caseworkers. The most important criteria are caseload, industry sector, and occupation group. Region and employability are of much less importance, and age is hardly ever mentioned.

With the option "other", caseworkers could also give fill-in answers.[2] These survey answers, indicating the predominance of caseload, industry sector and occupation group, are helpful when we discuss the determinants of the allocation process of unemployed to caseworkers further below.

Table 1: Criteria used for the allocation of unemployed to caseworkers

Criteria	Mentioned by X% of caseworkers
randomly	24
alphabetically	4
by industry sector	50
by occupation group	55
by caseload	43
by age of the unemployed	3
by employability	7
by region	10
other	10

Note: Caseworkers' answers to the question "According to which criteria where unemployed persons allocated to you". The answers sum up to more than 100% since multiple answers were permitted.

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.

[2] They mentioned "health status", "disability status", "expired benefit claim", "youth", "youth looking for apprenticeship", "school leavers", "university graduates", "higher management/academic persons", "self employed", "unemployed without qualifications", etc.

4 Data

4.1 Data and sample selection

The population for the microeconometric 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. 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.

In total, 239,004 persons registered as new unemployed during the year 2003. Notice that we consider only the first registration in 2003 for each person and subsume any further registrations in the outcome variables. For some persons no caseworker is defined in the data because the person de-registered before the first counselling meeting took place. For 215,251 persons the first caseworker is well defined. We then restrict our empirical analysis to employment offices that are comparable to other employment offices in 2003 (for the definition of 'comparable', see Appendix A). To analyse the effects for a more homogenous population we apply several further sample selection rules. We exclude jobseekers without benefit claim and individuals who applied for, or claim disability insurance, as these are probably less or no longer attached to the labour market. We exclude foreigners without yearly or permanent work permit because they do not have access to many of the employment offices' services and are likely to leave the country such that they could not be followed up.[3] We further restrict the sample to the prime-age population (24 to 55 years old) to avoid additional heterogeneity due to early retirement or re-entry into education. The remaining sample size is 136,606. In our main analysis, we focus on Swiss caseworkers and Swiss unemployed, thereby further reducing the effective sample size. See Section 4.3 and Appendix A for more details.

4.2 Definition of outcomes 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 the subsequent 47 months, whereas only 36 months are observed for those registering in December 2003. Observing the employment state for at least three years allows us to estimate the effects of similarity not only in the short term, but also in the medium term.

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 There is substantial temporary and seasonal employment in the hotel, restaurant, and tourism sectors in Switzerland.

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.

4.3 Definition of similarity

To identify the effects of similarity for a relatively homogenous population, we focus mainly 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.

For our main analysis 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. More precisely, caseworker and unemployed are considered to be of "similar age" if the absolute difference between their age is less or equal than 4 years.

Educational background is classified into four categories: Primary education (i.e. no degree from secondary education), lower secondary education, and 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. One should mention here that educational background is missing for quite a number of jobseekers in the administrative database system due to administrative reasons. The empirical results, however, are robust towards this missing data problem (see the Internet Appendix for details).

In addition to analysing the effects of similarity in all three dimensions gender, age, *and* education, we also examine the impact of similarity on only two or one of these dimensions. For example, the

[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.

definition "same gender and education" means that gender and education are identical for caseworker and unemployed whereas age may or may not be. Naturally, the number of observations with D_i=1 increases substantially when the definition of similarity is relaxed, as Table 2 shows.

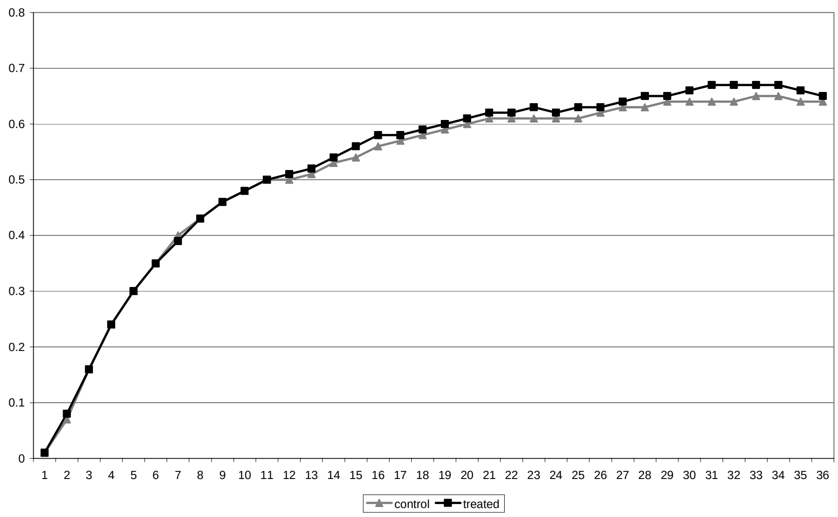
Table 2: Number of observations with similar and dissimilar caseworker

Definition of similarity	Unemployed with D_i=0	Unemployed with D_i=1
Same gender and age and education	37,165	1,455
Same gender and age	34,062	4,558
Same gender and education	31,079	7,541
Same age and education	36,119	2,501
Same gender	15,950	22,670
Same age	30,805	7,815
Same education	25,778	12,842

4.4 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.

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 1455 unemployed who are counselled by a caseworker with the same gender, age, and education. The grey line shows the employment rate for the 37165 individuals whose caseworker is different in at least one of the three characteristics.

Table 3 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 determinant for the propensity score estimation, as will be discussed in the next section. Since unemployed persons counselled by the same caseworker may be treated similarly by the caseworker, the standard errors are clustered at the caseworker level.[5] The last two columns show the means of selected variables for 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 are on average older than the unemployed. The difference in the employment rates in the previous Figure 2 is therefore probably downward-biased because the higher age in the D=1 group would lead - everything else equal - to relatively lower employment chances.

Table 3: Probit estimates of the determinants of similarity and sample averages for selected variables

		Probit estimates		Sample average	
		Coefficient	t-statistic	Different age, gender and/or education (D=0)	Same age, gender and education (D=1)
Constant	***	-2.93	12		
Characteristics of the unemployed clients					
Age (divided by 100)	***	.26	10	.36	.41
Female		-.05	.9	.45	.43
Education: primary education		-.06	.9	.15	.14
lower secondary education and apprenticeship		.00	.0	.61	.63
higher secondary education		.10	1.3	.03	.04
graduate from university/college/polytechnic		-	-	.20	.19
Qualification: unskilled		.01	.2	.10	.10
semiskilled		.02	.4	.13	.14
skilled without degree	***	-.35	3.5	.03	.01
skilled		-	-	.75	.75
Employability: low		-.10	1.6	.12	.11
medium		.04	.7	.74	.77
high		-	-	.13	.12
Looking for part-time job		.05	1.1	.11	.13
Industry of previous job: agriculture and forestry		.22	1.6	.01	.01
construction		.15	1.3	.06	.07
processing industry		.04	.5	.15	.14
tourism		-.06	.5	.07	.07
services		.11	1.3	.49	.52
public	*	.16	1.9	.16	.17
other		-	-	.05	.03

Table 3 to be continued.

[5] The standard errors are irrelevant for the propensity score matching or the common support analysis, but helpful for the interpretation of the determinants of similarity.

Table 3: Continued …

		Probit estimates		Sample average	
		Coefficient	t-statistic	Different age, gender and/or education (D=0)	Same age, gender and education (D=1)
Local labour market characteristics					
Language of employment office: French	***	-.22	3.0	.23	.16
Italian	*	.17	1.7	.07	.09
German		-	-	.71	.75
Registering in second half 2003 (dummy)		.01	.4	.56	.58
Size of municipality ≥200000 inhabitants		-	-	.08	.09
≥150000		.08	.8	.09	.10
≥75000		-.18	1.3	.05	.04
≥40000		-.11	.8	.04	.02
≥25000		.03	.4	.05	.05
≥15000		-.03	.4	.15	.15
≥8000		-.05	.5	.13	.13
≥3000		.04	.4	.20	.23
≥2000		-.01	.1	.10	.10
<2000		-.04	.3	.11	.11
Unemployment rate of canton	**	.07	2.1	3.75	3.83
Unemployment rate in industry (divided by 10)	**	.23	2.3	.46	.46
Characteristics of their caseworkers					
Age in years		-	-	46	40
Female		-	-	.42	.43
Tenure in employment office (in years)	***	-.03	3.4	5.80	5.28
Previous experience in municipality office (dummy)	*	.20	1.7	.10	.13
Previous experience in private placement office (dummy)	**	.13	2.3	.23	.30
Own experience of unemployment (dummy)	**	-.13	2.4	.62	.56
Education: primary education		-	-	.01	.00
lower secondary education and apprenticeship		-	-	.31	.76
higher secondary education		-	-	.46	.16
graduate from university/college/polytechnic		-	-	.23	.07
Special vocational training of caseworker (Eidg. Fachaus.)		-.04	.6	.25	.23
Allocation of unemployed to caseworker					
by industry	**	-.11	2.2	.52	.48
by occupation		-.02	.4	.54	.52
by age		-.22	1.4	.03	.02
by employability	**	-.23	2.0	.06	.04
by region	***	-.21	2.6	.12	.08
other		-.06	.6	.07	.06
at random		-	-	.22	.25
by alphabet		-	-	.04	.03
by caseload		-	-	.41	.40

Note: 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 38577, log Likelihood -5868, sum of squared residuals 1383, Efron's (1978) R-square is 0.012.

With respect to gender and educational attainment, both groups are quite similar. We do 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] In addition, when looking at the subjective employability rating of the caseworker, or whether the unemployed person looks for a part-time job or the industries in which they used to work before losing their job, we again find that both groups appear to be quite similar.

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. This implies a further potential downward bias of their average employment rate in Figure 2. 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.[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 differ-

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

[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.

ence 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 happen to be 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 relatively less likely to be assigned according to industry, age or employability compared to the reference group of a purely random, alphabetically or caseload dependent allocation.

The most striking finding of the Probit regression, however, is that most of the coefficients are small and insignificant with a Pseudo R^2 close to zero (only 0.012). We interpret this as an indication that there is no selection rule based on these observable characteristics, which is in line with the findings from Table 1. 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.

In the next section, we explain why the Probit estimates play a crucial role for our estimation strategy.

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

5 Identification and estimation of treatment effects

5.1 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 jobseeker, 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 the 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 $D = 0$. We base our analysis on the prototypical model of the statistical evaluation literature with a binary treatment variable *D* (see Neyman, 1921, Fisher, 1935, Rubin, 1974, 1979). Let

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

be the potential outcome at some time τ after unemployment registration at time t_0, if the similarity index was *d*.

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, we can define average treatment effect for the treated (ATET) as

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

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

The ATET is the treatment effect for an individual randomly drawn from the population of unemployed who were counselled by a caseworker of the same gender.[10]

For being able to estimate the ATET, 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 various other characteristics as well. Although Table 3 did not reveal very many differences, some of those are clearly related to the employment chances of the unemployed, e.g. whether the person lives in the German or the French-speaking part of Switzerland.

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

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

where $\chi \subseteq Supp(X \mid D = 1)$.[11] This assumption is referred to as the conditional independence assumption (CIA) in the following. It is also called unconfoundedness in the statistical literature (e.g. Rubin, 1974). We assume the CIA to hold for every value of x that lies in the support of X in the $D=1$ population, i.e. $\chi = Supp(X \mid D = 1) \subseteq Supp(X \mid D = 0)$. This common support restriction is discussed further below.

[10] We focus on the ATET, 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.

[11] *Supp(A)* denotes the support of the random variable *A*.

5.2 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 3, 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 do *not* want to control for age, gender and education of the *caseworker* as this would determine *D* with probability one such that identification of the counterfactual would be impossible. We also mention that in supplementary analyses we examined the treatments "age", "gender" and "education" of the caseworkers in separate estimations and did not find any significant effects on employment. Hence, we are confident, that not including these caseworker variables in *X* does not bias our results, in the sense that they capture the effects of similarity and not effects of caseworker characteristics alone. To be on the safe side, we nevertheless control for other caseworker characteristics that reflect caseworker quality, as discussed below.

Now consider two unemployed with identical age, gender and education, but different value 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 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.

One could further imagine that the office manager assigns difficult unemployed to caseworkers that are more similar in several respects. We include a measure of employability of the unemployed as control variable. We also control for the criteria used for allocation as discussed in Table 1.

Given two individuals who 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 municipal employment office, previous experience in private placement agency, own experience of unemployment, and participation in special caseworker training. These variables capture what is known to the labour office at the time of the decision to allocate a specific casework to a specific unemployed client.

Overall, Table 3 suggested that there is no clear selection rule, which assigns unemployed to caseworker with similar characteristics. Most of the coefficients are small and insignificant with a Pseudo R^2 of only 0.012. We interpret this as an indication that the similarity indicator D_i is more or less randomly allocated. Although these estimates do not rule out selection-on-unobservables, it seems highly implausible that this would be of a big concern. If the indicator D_i was driven by selection-on-unobservables, we would expect D to be correlated with at least a reasonable number of observed characteristics. This is particularly the case since some of the X variables included in the regression are unobserved in many other datasets. Most of the characteristics of the unemployed jobseeker are insignificant in Table 3. A notable exception is age of the unemployed person, which is because the average age of the caseworkers is larger than the average age of the unemployed jobseekers. 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.

5.3 Semiparametric matching estimation

In the empirical analysis, we use a matching estimator as implemented in Lechner et al. (2006). The advantage of matching estimators is that they are semiparametric and that they allow for arbitrary individual effect heterogeneity.[12] By the conditional independence assumption (3), the ATET is identified as

$$\begin{aligned} E[Y^1 - Y^0 \mid D=1] &= E[Y \mid D=1] - E[Y^0 \mid D=1] \\ &= E[Y \mid D=1] - E[E[Y^0 \mid X, D=1] \mid D=1] \\ &= E[Y \mid D=1] - E[E[Y \mid X, D=0] \mid D=1], \end{aligned}$$

where the first term can be estimated by the sample mean in the *D=1* population and the second term by

$$\frac{\sum_i \hat{m}_0(X_i) \cdot D_i}{\sum_i D_i},$$

where $\hat{m}_0(x)$ is a nonparametric estimator of $E[Y \mid X = x, D = 0]$, e.g. a first-nearest-neighbour estimator. As we search for each individual of the *D=1* population for the nearest neighbour in the *D=0* population, this is usually referred to as a "matching" estimator, i.e. it matches observations from one subsample to the other subsample. Rosenbaum and Rubin (1983) have shown that instead of matching on the high-dimensional vector *X*, consistent estimates are also obtained by matching on the one-dimensional propensity score $p(x) = \Pr(D = 1 \mid X = x)$. The propensity score was estimated by Probit as shown in Table 3. The small sample properties of matching estimators have been well explored and appeared to be quite robust in different practical applications (e.g. Larsson, 2003; Gerfin et al., 2005).

[12] See Heckman et al. (1999), for matching with a binary treatment, and Imbens (2000), Lechner (2001), and Gerfin and Lechner (2002) for multiple treatments. Imbens (2004) provides an excellent survey of the recent advances in this field.

In this paper we use an extension of conventional matching estimation, similar to Lechner et al. (2006), which extends the first-nearest neighbour propensity score matching estimator in several directions: First, matching does not only proceed with respect to the propensity score but also incorporates additionally some other covariates. Second, instead of using first-nearest neighbour matching, all neighbours within a pre-specified radius are used. Third, the matching quality is increased by implementing a weighted regression based bias removal procedure on top of matching.

We do not only match on the propensity score, but also on several additional covariates that are suspected to be potentially highly correlated with the outcome variable Y^0 as well as with D. Such combinations, which are also referred to as balancing scores, can help to ensure that a misspecification of the functional form of the propensity score has only a minor impact on the estimation of ATET. We therefore match on $p(x)$ and a subset of X, where the propensity score is given a larger weight (five times higher) in the Mahalanobis distance calculation.

The motivation for radius matching is the possibility of efficiency gains without the risk of incurring much additional bias. The matching algorithm in Gerfin and Lechner (2002) used the first nearest control observation for each treated. However, when there are other comparison observations that are similar to the matched comparison observation, there are straightforward efficiency gains (without paying a high price in terms of additional bias) by considering these additional 'very close' neighbours and forming an 'averaged matched comparison' observation. Of course, there are many ways to do this in practice. We suggest being more cautious with respect to additional bias than with respect to additional variance because the variance of the estimator is visible after the estimation, whereas the bias generally is not. To be conservative, we consider only observations that have a distance to 'their' treated observation of no more than 90% (denoted by R in the following) of the worst match that we had obtained by one-to-one matching (after enforcing common support; $R=0$ is the case of one-to-one matching; R corresponds to a bandwidth choice in kernel weighting). To be even more conservative, we weight the observations proportionally to their distance from the treated (corresponding to a triangular kernel). The results are not very sensitive to the exact way the weighting is implemented.

In addition to incorporating all control observations within a certain radius, we also exploit the fact that appropriately weighted regressions that use the sampling weights from matching have the so-called double robustness property. This property implies that the estimator remains consistent if the matching step is based on a correctly specified selection model *or* the regression model is correctly specified (e.g. Rubin, 1979; Joffe et al., 2004). Moreover, this procedure should increase precision and may reduce small sample as well as asymptotic bias of matching estimators and thus increase robustness of the estimator in this dimension as well. Note that Abadie and Imbens (2006a) have shown that the usual *1-to-K* matching estimators, where K is a fixed number, may exhibit an asymptotic bias, because matches are not exact. Our weighted radius matching estimator does not necessary imply a fixed K and is thus probably less subject to this problem.[13] Nevertheless, we follow their proposal and implement a weighted regression based bias removal procedure on top of the matching. The regression is done in the comparison sample only. Outcomes are predicted for the attributes observed in treated and control samples. Specifically, the outcome variable is regressed on the propensity score and the additional variables with weights coming from the matching step (see Imbens, 2004). The difference between the mean of the predicted outcomes using the observed X of the treated and the weighted X of the comparison observations gives an estimate of the bias (see Table B.1 for the exact implementation). Without the theoretical justification given by Abadie and Imbens (2006a), a somewhat similar procedure has been used by Rubin (1979) and Lechner (2000).

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[13] The results of Abadie and Imbens (2006a) do not apply directly to propensity score matching, but since we also match on additional variables there are some similarities with the estimators they consider.

The different steps of the estimator are described in Table B.1 in the Appendix. In the first step, a Probit model (Table 3) is used to estimate the propensity score. Step 2 ensures that we estimate only effects in the region of common support. For observations of the *D=1* sample with propensity score *p(x)* very close to one we would not be able to find a corresponding observation in the *D=0* sample with characteristics leading to similar values of *p(x)*. Given the large number of *D=0* observations and the weak predictive power of the *X* variables, it turned out that we do not lose any observations due to this restriction, though.

Inference for this entire estimation step is based on the bootstrap.[14] It is implemented following MacKinnon (2006) by bootstrapping the p-values of the t-statistic directly based on symmetric confidence intervals (rejection regions). Bootstrapping the p-values directly, compared to bootstrapping the distribution of the effects or the standard errors, leads to asymptotic refinements because the t-statistics on which the p-values are based are asymptotically pivotal in contrast to the standard errors or coefficient estimates.

6 Empirical results

6.1 General remarks

The presentation of the empirical results proceeds as follows. We first discuss the results for our main population of Swiss caseworkers and Swiss unemployed (whose mother tongue is identical to the cantonal language) of age 24 to 55. This population contains 38,620 unemployed persons, of which 1,455 observations have D_i=1 and the remaining 37,165 have D_i=0. As mentioned before, this population already shares nationality and language with their caseworkers. We first examine

[14] Although Abadie and Imbens (2006b) showed that the conventional matching estimator is not smooth enough and that, therefore, the bootstrap does not lead to valid inference, the version of the estimator implemented here is by construction much smoother than the estimator studied by Abadie and Imbens (2006b), such that the results of Abadie and Imbens do not apply.

the impact of sharing additionally *gender*, *age*, and *education* with the caseworker. In further analyses, we then examine the effects when being identical in only one or two of these three characteristics. Then we present an extensive sensitivity analysis that confirms our main findings.

For all specifications, two different estimators have been used: propensity score matching with additional matching and regression on *age* and *gender* and *three education dummies* (as discussed in the previous section), and Maximum Likelihood logistic regression. In the following, we show only the results for propensity score matching and give the Maximum Likelihood results in the Internet Appendix. Overall, the Logit results were very similar, often somewhat larger, and as expected less noisy.[15] The confidence intervals are obtained by the nonparametric bootstrap via re-sampling caseworkers (together with all their clients) to account for possible dependence among the unemployed counselled by the same caseworker.

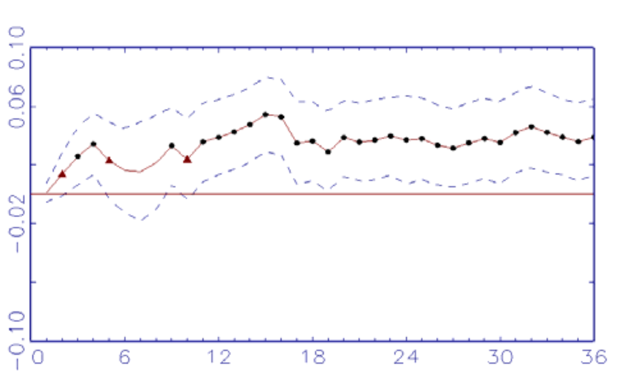
6.2 Estimation results for main population

In this section, we present the effects of similarity on employment probabilities for the population of *Swiss caseworkers and Swiss unemployed whose mother tongue is the cantonal language*. In particular, we use the Probit from Table 3 to estimate the propensity score. The following Figure 3 shows the estimated effects on employment 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 4-percentage points. The estimates are smaller and less precise in the early months, which is natural as we expect *similarity* to improve the job search process rather than leading to immediate re-employment. Compared to effects of active labour market programmes, these effects are remarkably high. (Note also that they are conservative, as there are individuals in the control group who are similar in two or one characteristic with their caseworker. When selecting

[15] We also examined propensity score matching with alternative sets of additional regressors $\tilde{X}$, i.e. regressors that are used in the regression step of the matching estimator. Since the results were similar, we do not report these estimates.

only individuals whose age and gender and education is not similar to their caseworker for the control group, we find treatment effects that are about half a percentage point larger.)

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

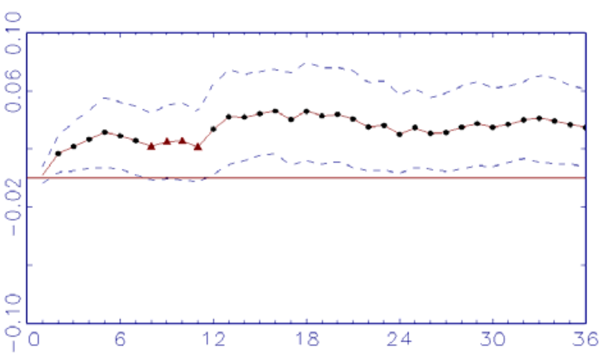


Note: Treatment effect of similarity in age, gender and education, estimated by propensity score matching with *age*, *gender* and *education* of the unemployed as additional variables. Treatment effect on employment probability in month t after registering as unemployed. Dots indicate significance at the 5% level, triangles at the 10% level. The dashed lines represent pointwise 95% confidence intervals.

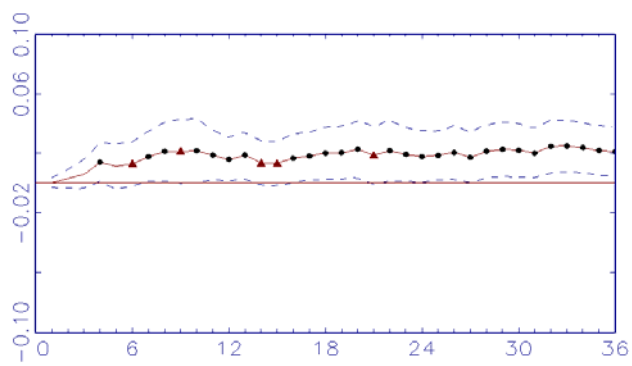
Figure 3 showed the effect when similarity was defined as one only if gender, age *and* education coincided between unemployed and caseworker. The three graphs in Figure 4 show the estimation results when similarity is defined less strictly. Here coincidence in two of these three characteristics suffices for *D* to be defined as one. The results show that similarity in age and education still leads to a significant positive effect of about 2 to 3 percentage points. Similarity in gender and education leads to a significant positive effect of 2 percentage points, whereas the effects for similarity in age and gender appear to be close to zero.

Figure 4: Effects of similarity in two characteristics on employment

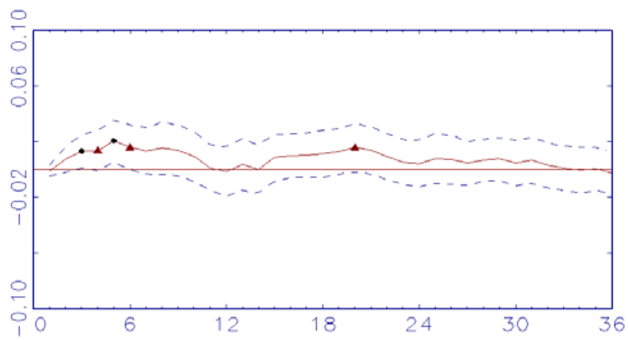
Similarity in age and education



Similarity in gender and education



Similarity in age and gender

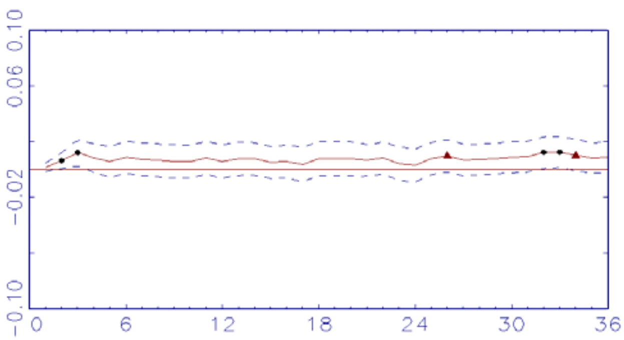


Note: See note below Figure 3.

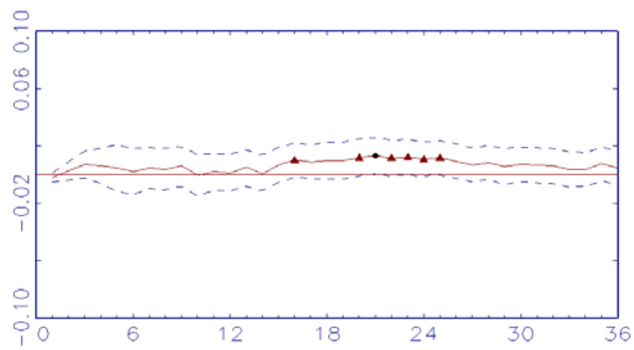
The graphs contained in Figure 5 show the estimates when *D* is defined with respect to only one characteristic: All effects are close to zero. The simplest interpretation of these results is that having a caseworker of the same gender or of the same age is not sufficient to reap positive effects. Caseworker and unemployed have to be similar on several dimensions for these effects to materialize. This interpretation seems to be in line with the social identity hypothesis referred to in Section 2. There is no strong evidence that women interact better with women and men interact better with men, or vice versa. Rather, an effective relationship requires a more clearly delineated social group.

Figure 5: Effects of similarity in one characteristic on employment

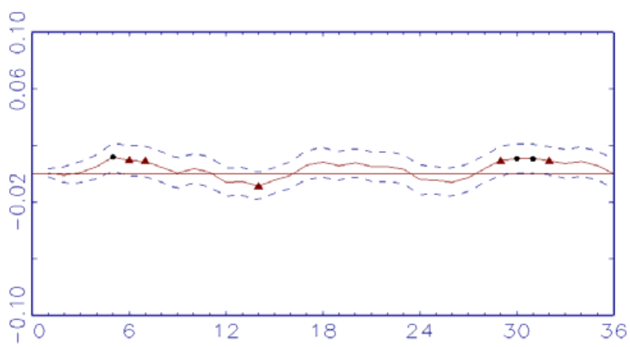
Similarity in education



Similarity in age



Similarity in gender



Note: See note below Figure 3.

Admittedly, the similarity in terms of age, gender, and educational background does not define an extremely narrow social group, but it is worthwhile to remember that we retained in our sample only Swiss caseworkers and Swiss jobseekers who share the same local language. Given that about 35 to 40% of the unemployed are foreigners, this provides a further element of identity. Unfortunately, we do not have information on the profession or industry where the caseworker had been employed (if any) before entering the public employment services, but even if we had, the number of observations with similarity on this additional dimension would drop to very low numbers. Nevertheless, in Section 6.3.2 we examine various other populations and find that the estimates generally tend to support the above interpretation that the caseworker-to-jobseeker relationship is enhanced the more similar they are. Before we examine those alternative definitions of our population of interest, we examine first potential concerns about selection-on-unobservables in the next section.

6.3 Sensitivity analysis

6.3.1 Selection on unobservables

A central element to our identification strategy is the conditional independence assumption (3). In other words, we assume that D is not assigned based on unobserved characteristics that are at the same time related to the labour market chances Y^0 of the unemployed person. In this section, we discuss potential violations of (3) in more detail.

A reasonable concern is that there are employment office specific idiosyncratic selection rules, which we cannot observe, and which are 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 jobseekers to caseworkers according to their characteristics whereas such strategies might not be used in other offices. Including a dummy variable for every employment office in the Probit estimation ensures that we effectively measure the effect of similarity only *within* each employment office. Although variability increases, the employment office dummies do not change the results much (see Figure IA.4 in the Internet Appendix).

Another potential problem could be that unemployed jobseekers with poor labour market chances Y^0 might deliberately be assigned by the employment office managers to caseworkers that seem to fit best. We already control for subjective assessment of employability in X, but this perhaps may not capture everything important.) As mentioned before, all employment offices included in our analysis are responsible for a certain geographic area. They apply within-office specialization only to a limited degree, e.g. some caseworkers dealing mainly with jobseekers from manufacturing and crafts, while others tend to specialize in office jobs. As already mentioned in Section 3, caseworkers as well as employment office managers were asked which criteria were used to allocate jobseekers to caseworkers. Whereas allocation according to industry was mentioned frequently, gender or education was hardly ever mentioned in the spontaneous answers section to the question, while only a minor fraction mentioned age as an allocation criterion. In addition, in several interviews we conducted with caseworkers and office managers such criteria were not mentioned.

To address further concerns about missing selection variables in the Probit estimation of the propensity score, we examined specifications with extended regressor sets (see the Internet Appendix for the respective tables): First, 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. Since the office management first wants to ensure a similar caseload across its caseworkers, there is much more scope in large offices than in small offices for active matching of caseworkers and jobseekers beyond one or two simple criteria such as occupation group and region. We would therefore expect the size of the office to have a positive effect on the probability that D=1. However, the coefficient is negative and insignificant in Table IA.1 in the Internet Appendix. In other words, we do not find evidence for the above hypothesis.

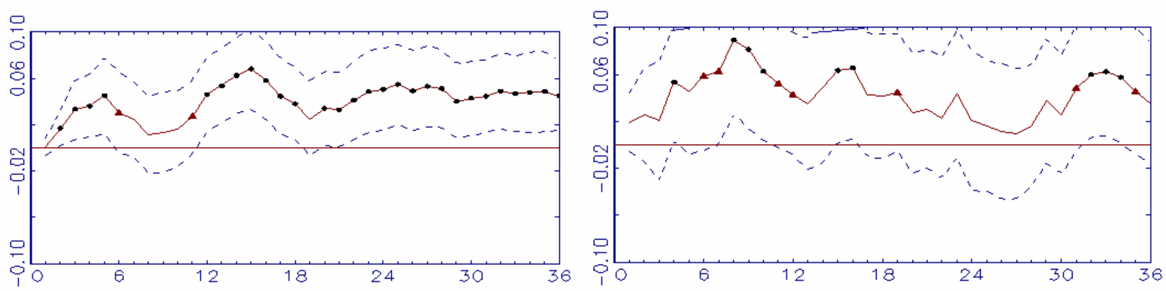
Second, in a further specification, we included a larger number of additional characteristics of the unemployed jobseeker in X, in addition to those already shown in Table 3. These additional variables are 16 dummies for the occupation group of the last job of the unemployed, family size, number of unemployment spells in the last 2 years, average yearly earnings in the last 10 years, and the total number of months in employment in the last 10 years. Even with this larger set of regressors, which capture long-term employment and earnings histories, the Pseudo R^2 increases only to 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.

As another, very clean approach to examine the potential degree of "selection-on-unobservables", we split the sample into two parts, according to the answers given by their caseworkers to the survey question on the criteria used for the allocation of unemployed to caseworkers. The first subsample consists of those unemployed whose caseworker mentioned any of the items "allocation by industry sector/industry", "by occupation group", "by age of unemployed", "by employability", "other" or gave no answer at all to this question (see Table 1). This group contains caseworkers who received (at least partly) unemployed clients based on an active selection rule. The second subsample consists of caseworkers who had not mentioned any of the above items, in other words, who 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.[16] 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. Figure 6 shows the estimated treatment effects of similarity in age, gender and education, on the left for Group 1 (allocation according to criteria) and on the right for Group 2 (allocation at random). Since the estimation results appear to be quite similar for both groups, despite the fact that Group 2 contains only 319 observations with D=1, potential selection-on-unobservables might overall not be a big concern. If any difference exists at all, the effects seem to

[16] 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.

be rather larger than smaller in the “random allocation” Group 2. Unfortunately, due the drastically reduced sample size for Group 2, the test lacks power.

Figure 6: Effects of similarity in age, gender, and education on employment, by allocation criteria



Note: On the left, Group 1 (allocation according to criteria) with 30,377 observations with *D*=0 and 1,136 observations with *D*=1. On the right, Group 2 (allocation at random) with 6,788 observations with *D*=0 and 319 observations with *D*=1. See also note below Figure 3.

6.3.2 Variation of the population

As a further robustness check, we estimated treatment effects also for the subpopulation of 39 to 55 year old unemployed persons to consider a population of unemployed that is on average more similar to the age distribution of the caseworkers, whose average age is about 45 years. With the main population of 24 to 55 year old unemployed it turned out that in the matching estimator the average age of the unemployed in the *D*=0 and the *D*=1 population could not be perfectly balanced. In other words, for a 24-year-old unemployed it is very unlikely to be counselled by a caseworker of a similar age, whereas for a 45-year-old unemployed this is much more likely. This may not be so much of a concern because the matching estimator contains an additional regression step to eliminate average bias due to unbalanced covariates, including age. To be on the safe side, however, we also considered subpopulations of unemployed where we discarded the very young unemployed. With the population of 39 to 55 year old it turned out that average age could be exactly balanced in the *D*=0 and *D*=1 population.

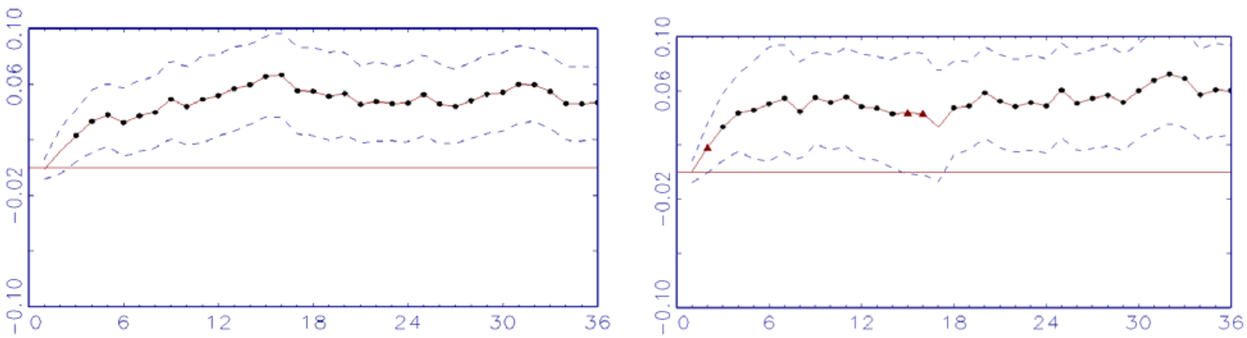
The estimated treatment effects for this subpopulation are about 3 to 4 percentage points and thus similar to those of Figure 3. Since the number of *D*=1 observations fell nearly by half, the estimates are however less precise (see Figure IA.5 in the Internet Appendix for details).

6.3.3 Variation of the treatment

This section contains an additional sensitivity analysis to examine the stability of the previous findings with respect to the definition of similarity. First, we examine the effect of same age, gender, education, *nationality,* and *mother tongue* explicitly. *D* is now defined as one only if age, gender, education, and nationality (coded binary as Swiss/non-Swiss)[17] coincide, and if the mother tongue of the unemployed equals the main language of the canton. Otherwise, *D* is zero. We estimate the effect of this stricter definition of similarity in the main population, but naturally include also the foreign caseworkers and unemployed, giving a sample size of 60,194. The number of observations with *D*=1 is 1,480 and with *D*=0 is 58,714. The estimated effects are given on the left hand side of Figure 7. They are now somewhat larger than those of Figure 3, and significant, clearly confirming the previous findings.

[17] We code nationality only as binary because we do not know the exact nationality of the foreign caseworkers (only their names) and because there would be only very few coincidences of nationality for a foreign caseworker and a foreign unemployed. Furthermore, we do not know the exact mother tongue of the caseworker. We presume that it corresponds to the language used by his employer, i.e. the cantonal administration. At least, he has to be very fluent in that language.

Figure 7: Effects of similarity in age, gender, education, nationality, mother tongue, and unemployment



Note: In left graph, treatment is similarity in age, gender, education, nationality, and mother tongue. In the right graph, own experience of unemployment is included as additional criterion to define the treatment. See also note below Figure 3.

The right graph of Figure 7 shows the results when we add even another dimension to the similarity definition. As part of our survey, we also asked caseworkers whether they had ever been unemployed themselves. About two thirds of the caseworkers made this experience and we include this in the definition of similarity. *D* is now only different from zero if also the caseworker shares the experience of unemployment with his clients. The number of observations with *D*=1 is 837 and with *D*=0 is 59,357. Again, the estimated effects in the right graph of Figure 7 are similar to those of Figure 3. This shows that similarity is relevant even in a wider context.

6.3.4 Missing values in the education variable

A further concern might be the large number of unemployed with missing information on their education. As mentioned before, this seems largely be due to the fact that information on school education was previously not elicited by the information system of the employment offices as it was only concerned with labour market experiences and job qualifications. As educational variables were added to the administrative data, some of the caseworkers were initially reluctant to accept the additional administrative burden of entering this requested information (on education and other variables) into the computer. However, as discussed in the Internet Appendix, missing information on education does not seem to be a concern for our results.

7 Conclusions

In this paper, we examined the impact of similarity between caseworker and the unemployed job-seekers on their chances to find a job. A positive employment effect of about 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, parametric Maximum Likelihood logistic regressions gave very similar, often somewhat larger, effects that where estimated more precisely.

An interesting finding is that 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-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 would be worth implementing. A reallocation of unemployed to caseworkers could thus enhance reemployment outcomes. This may be easier to achieve in larger employment offices, i.e. when smaller offices are merged, or when employment offices specialize 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.

Appendix A: Data

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.[18] We do not include the canton Geneva in our study since in this canton the employment offices are functionally specialized 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-organized during the year 2003, one employment office that specialized on the difficult cases in Solothurn, and the tiny employment office in Appenzell-Innerrhoden, which did not participate in the survey.

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.

[18] 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.

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 for the main analyses restrict the sample to the prime-age population.

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 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

Appendix B: Further details on the matching estimator

Table B.1: A matching protocol for the estimation of ATET

Step 1	Estimate a Probit model to obtain the choice probabilities: $\hat{p}_i = \Pr(D=1 \mid X = X_i)$
Step 2	Restrict sample to common support: Delete all D=1 observations with $\hat{p}_i$ larger than the largest estimated propensity score among the D=0 observations.
Step 3	*Estimate the counterfactual expectation of the outcome variable* $E[Y^0 \mid D=1]$

Standard propensity score matching step (binary treatment)

a-1) Choose one observation from the *D = 1* subsample and delete it from that pool.

b-1) Find an observation from the *D = 0* subsample that is as close as possible to the one chosen in step a-1) in terms of $\left[\hat{P}(x), \tilde{x}\right]$, with respect to the Mahalanobis distance. Do not remove that observation, so that it can be used again.

c-1) Repeat a-1) and b-1) until no participant in *D = 1* is left.

Exploit thick support of X to increase efficiency (radius matching step)

d-1) Compute the maximum distance (δ) obtained for any comparison between treated and matched comparison observations.

a-2) Repeat a-1).

b-2) Repeat b-1). If possible, find other observations of the *D = 0* subsample that are at least as close as *R x* δ to the one chosen in step a-2); *R* is fixed to 90% in this application but different values are examined in the sensitivity analysis. Do not remove these observations, so that they can be used again. Compute weights for all chosen comparisons observations such that these weights are proportional to their distance (calculated in b-1). Normalise the weights such that they add to one.

c-2) Repeat a-2) and b-2) until no participant in *D = 1* is left.

d-2) For every *D=0* observation, add the weights obtained in b-2).

Exploit double robustness property to adjust small mismatches by regression

e) Using the weights $w(x_i)$ obtained in d-2), run a weighted linear regression of the outcome variable on the variables used to define the distance (and an intercept).

f-1) Predict the potential outcome $y^0(x_i)$ of every observation in *D = 0* and *D = 1* using the coefficients of this regression: $\hat{y}^0(x_i)$.

f-2) Estimate the bias of the matching estimator for $E[Y^0 | D=1]$ as:

$$\frac{1}{N^1}\sum_{i=1}^{N}\underline{1}(D_i=1)\hat{y}^0(x_i) - \sum_{i=1}^{N}\underline{1}(D_i=0)w(x_i)\hat{y}^0(x_i).$$

g) Using the weights obtained by weighted matching in d-2), compute a weighted mean of the outcome variables in *D = 0*. Subtract the bias from this estimate.

Final estimate

h) Compute the treatment effect by subtracting the weighted mean of the outcomes in the comparison group (*D = 0)* from the mean in the treatment group (*D = 1*).

Note: The table refers to the estimation of ATET. $\tilde{x}$ includes gender, age and three education dummies. $\tilde{x}$ is included to ensure a high match quality with respect to these critical variables.

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