Universität St.Gallen

Unemployed and their Caseworkers: Should they be Friends or Foes

Stefanie Behncke, Markus Frölich, and
Michael Lechner

November 2007 Discussion Paper no. 2007- 45

Department of Economics University of St. Gallen

Editor: Prof. Jörg Baumberger
University of St. Gallen
Department of Economics
Bodanstr. 1
CH-9000 St. Gallen
Phone +41 71 224 22 41
Fax +41 71 224 28 85
Email joerg.baumberger@unisg.ch

Publisher: Department of Economics
University of St. Gallen
Bodanstrasse 8
CH-9000 St. Gallen
Phone +41 71 224 23 25
Fax +41 71 224 22 98

Electronic Publication: http://www.vwa.unisg.ch

Unemployed and their Caseworkers:

Should they be Friends or Foes?[1]

Stefanie Behncke, Markus Frölich and Michael Lechner

Authors addresses:

Stefanie Behncke
SIAW-HSG
Bodanstrasse 8
9000 St. Gallen
Tel. +41 71 224 2304
Fax +41 71 224 2298
Email stefanie.behncke@unisg.ch

Dr. Markus Fröhlich
SIAW-HSG
Bodanstrasse 8
9000 St. Gallen
Tel. +41 71 224 2329
Fax +41 71 224 1198
Email markus.froelich@unisg.ch

Professor Dr. Michael Lechner
SIAW-HSG
Bodanstrasse 8
9000 St. Gallen
Tel. +41 71 224 2350
Fax +41 71 224 2298
Email michael.lechner@unisg.ch

[1] Markus Frölich is also affiliated with IFAU, Uppsala, and IZA, Bonn. Michael Lechner has further affiliations with ZEW, Mannheim, CEPR, London, IZA, Bonn, and PSI, London. We are very grateful in particular to Heidi Steiger and Stephan Werner for help with the data and to Stephan Hammer for fruitful cooperation in this project. We also thank Martin Grubb and Thomas Ragni as well as seminar participants in Mannheim (ZEW), Nürnberg (Federal Employment Agency), St. Gallen (COST meeting), Oslo (EALE), and Munich (German Economic Association) for helpful comments and suggestions. All remaining errors are our own. We are grateful to the research fund of the Swiss unemployment insurance system (at the seco) for providing the administrative database as well as substantial financial support for this project.

Abstract

In many countries, caseworkers in a public employment office have the dual roles of counselling and monitoring unemployed persons. These roles often conflict with each other leading to important caseworker heterogeneity: Some consider providing services to their clients and satisfying their demands as their primary task. Others may however pursue their strategies even against the will of the unemployed person. They may assign job assignments and labour market programmes without consent of the unemployed person. Based on a very detailed *linked jobseeker-caseworker* dataset, we investigate the effects of caseworkers' cooperativeness on the employment probabilities of their clients. Modified statistical matching methods reveal that caseworkers who place less emphasis on a cooperative and harmonic relationship with their clients increase their employment chances in the short and medium term.

Keywords

Public employment services, unemployment, statistical matching methods

JEL Classification

J68, C31

1 Introduction[*]

We use a very informative *linked jobseeker-caseworker* dataset augmented by detailed information on caseworker characteristics in combination with matching estimation to examine a question that is important in implementing the counselling processes of unemployment insurance systems. The question is about the impact of different levels of caseworkers' cooperativeness concerning their unemployed clients on the future employment chances of their clients. Observing the employment outcomes up to 3 years after registration, we find that a caseworker who is more demanding (i.e. less co-operative) vis-à-vis the unemployed person achieves higher re-employment probabilities.

In most countries, caseworkers are assigned the dual role of *counselling* and *monitoring* of unemployed persons. These two roles often conflict with each other: On the one hand, caseworkers need to establish a trustful and empathetic relationship with their clients for providing effective counselling. On the other hand, they have to police job search behaviour and initiate and enforce sanctions if it falls short of the requirements mandated by the unemployment insurance law. Since the legal rules typically leave some leeway to the caseworker on how to weight these two potentially conflicting roles, it is not surprising that individual caseworkers weight them differently. Some caseworkers pursue a more dominating and demanding stance vis-à-vis the unemployed, while others aim at a more cooperative relationship devoid of conflicts. Caseworkers perform this dual task by setting certain rules and initiating certain actions for their clients (e.g. sending clients to specific programmes of the active labour market policies or imposing sanctions), in addition to more personal channels such as counselling style, personal relationships, empathy or sympathy.

Substantial research interest has been devoted to the question on how to improve the public unemployment system. Various aspects or instruments of the relationship between jobseeker and employment office have been considered. The impact of explicit rules and incentives are examined in Fredriksson and Holmlund (2003), who considered 'optimal' unemployment insurance (UI) systems. They examine three different means of improving the efficiency of UI via passive labour market policy: the duration of benefit payments, monitoring in conjunction with sanctions, and workfare, and find that a system with monitoring and sanctions improves search incentives. Pavoni and Violante (2007) and Wunsch (2007) provide a theoretical economic framework to determine the 'optimal' choice between different *passive and active* labour market policy instruments. In their models, the typical optimal sequence of policies is unmonitored job search, followed by monitored job search and social assistance as an absorbing state.

So far, the role of increased *monitoring* has been examined in Meyer (1995), Gorter and Kalb (1996), and Dolton and O'Neill (1996), who found significant positive effects of monitoring in the USA, Netherlands, and the UK, respectively. Ashenfelter, Ashmore, and Deschenes (2000), and Bloom, Hill, and Riccio (2003) found no significant effects of increased monitoring in the USA, though. The effects of *sanctioning* as one of the instruments of the employment offices have been examined in Lalive, van Ours, and Zweimüller (2005), who found significant positive effects of sanctions for Swiss unemployment recipients. Van den Berg, van der Klaauw, and van Ours (2004) found positive effects of sanctions for Dutch welfare recipients, and Abbring, van den Berg and van Ours (2005) found positive effects of sanctions on Dutch unemployment benefit recipients. Svarer (2007) also finds positive effects of sanctions on the job-finding rate in Denmark. Black, Smith, Berger, and Noel (2003), and Graversen and van Ours (2006) find evidence for *threat effects* of employment and training programmes: mandatory assignment to a programme increases job finding rates before participation starts, because unemployed want to avoid time-consuming programmes.

We add to this literature in that we do not consider only the effects of one single instrument (such as monitoring or sanctioning) but rather the relationship between caseworkers and their unemployed clients as a whole.[1] In addition to explicit rules and incentives, the personal relationship between caseworker and unemployed could have an important effect on motivation, job search intensity, and job acceptance. A more demanding caseworker may use certain instruments more often but the different counselling style in itself can have important effects. These personal or behavioural factors had received less attention in the economics literature until recently.

Principal-agent theory suggests that caseworkers could increase the unemployed person's job-finding and job-taking rates through a less cooperative behaviour: Unemployed persons have an incentive to avoid costly job search or to wait for better job offers, and caseworkers are required to set appropriate incentives through compensation and supervisory schemes.[2] Caseworkers should thus demand more job search effort and treat unemployed accordingly. Confirming this line of argument, several studies found that monitoring and sanctions increase employment probabilities. On the other hand, effective counselling by the caseworker may require a trustful atmosphere. An unemployed person who expects a stiff caseworker will obviously provide distorted information about his preferences, needs, skills, aspirations, job search efforts, and the like. In such an atmosphere, counselling by caseworkers may be useless as well as any labour market training programmes. In addition, recent experimental evidence indicates that individuals may have reciprocal preferences. When being treated nicely by the caseworker, they may be more willing to behave nicely (Fehr and Schmidt, 2001). Hence, caseworkers might achieve higher employment rates by cooperating with their clients instead of potentially punishing them or by ignoring their requests. Caseworkers are

1 Using production frontier analysis Ramirez and Vassiliev (2007) have pointed out that substantial scope for improving the effectiveness of public employment services in Switzerland seems to exist, see also Ferro-Luzzi et al. (2005) and Sheldon (2003). However, those analyses are not directly instructive in pointing out how to improve effectiveness.

2 Shavell and Weiss (1979) argue that unemployment insurance lengthens unemployment duration because of its effect on job search effort and the reservation wage. However, if caseworkers could monitor job search behaviour, no such

well aware of these two opposing views of the world and their trade-offs, but each caseworker may be weighting the importance of these models differently.

This paper also helps to understand the determinants of exiting unemployment. One strand of the literature has estimated the relationship between the unemployed person's characteristics (age, gender, education, etc.) and the hazard rate for leaving unemployment, e.g. Machin and Manning (1999). Another strand of the literature has evaluated how certain instruments such as labour market programmes, monitoring or sanctions affect employment.[3] Caseworker characteristics have received less attention, though. This analysis is made possible by a unique linked jobseeker-caseworker dataset with very detailed information on both jobseekers caseworkers. (To the best of our knowledge, such data was not available before.) Focusing on the caseworker-client relationship enriches the traditional evaluation literature since the imposition of sanctions or assignment of labour market programmes could already be considered as an outcome of caseworker's behaviour.

We use semiparametric matching estimators that have been developed in the statistics literature and successfully applied in labour economics since several years.[4] To be more precise, we use statistical propensity score matching with an extension to radius matching and incorporating regression-type adjustment following ideas by Rubin (1979), Abadie and Imbens (2006), and others. In contrast to a conventional duration model analysis, which would focus only on the hazard rate out of unemployment, we estimate the employment probability at a certain point in time. This thus captures not only

problem would exist. Unemployment insurance benefits could be withheld if effort or the reservation wage was unsatisfactory.

3 See Heckman, Lalonde, and Smith (1999) for a survey of empirical findings of programme effects in the USA and Europe, or Martin and Grubb (2001) for a survey of OECD experiences, or Wunsch (2005) for a survey of programme effects in Germany. See the previous footnotes for the literature on sanctions and monitoring.

4 See e.g. Rosenbaum and Rubin (1983), Lechner (2002), Black and Smith (2004), Imbens (2004), or Ham and LaLonde (2005).

the exits from unemployment to employment but includes also re-entry in unemployment, an important aspect from a public policy perspective.

Our estimation results indicate a positive effect of reduced cooperativeness on employment probabilities of about 2 percentage points. Hence, pursuing a more demanding stance vis-à-vis unemployed persons increases employment probabilities in the short and in the medium term (up to 3 years after the beginning of unemployment) by a non-negligible amount. This increased employment is not obtained at the cost of reduced stability of jobs. The effects on stable employment are also positive and of similar magnitude. The sensitivity of these results is explored by examining several alternative specifications, in particular concerning the choice of the control variables and various definitions of the treatment variable. The results are rather stable, although often less precisely estimated.

The structure of the paper is as follows: The following section discusses the unemployment insurance system in Switzerland. Section 3 describes the data, and Section 4 the statistical methodology. Sections 5 and 6 give the main estimation results, and Section 7 concludes. Several appendices provide additional details.

2 The Swiss labour market and the role of caseworkers

2.1 Overview

Until the recession of the early 1990s, unemployment was extremely low in Switzerland, a small country with 26 different administrative regions, called *cantons*. With the recession, the unemployment rate rose rapidly to 5% (see figure below) and triggered a comprehensive revision of the federal unemployment insurance act in 1996/1997. The about 3000 municipal unemployment offices were consolidated to a smaller number of *regional employment offices*. Compared to the previous municipal offices, which were largely concerned with administering unemployment benefits, these regional offices, of which there were about one hundred operating in 2003, aimed at providing professional services with respect to counselling, placement, activation, and training. A large number of caseworkers were hired and further trained for these purposes.

Figure 1: Unemployment rate in Switzerland from January 1990 to August 2007

Note: Monthly unemployment rate, January 1990 – August 2007, Source: Swiss National Bank Monatshefte.

This was financed by the unemployment insurance. In 2003, the period we consider in our study, both employers and employees were obliged to contribute a share of 2,5% of the salary. Benefits amounted to 70-80% of the former salary depending on age, dependents and income. By July 2003 the rules for benefit entitlement were tightened for individuals younger than 56: the minimum contribution time was raised from 6 to 12 months and the maximum benefit entitlement period was reduced from 24 to 18,5 months.

2.2 Caseworkers' autonomy

Whereas the federal State Secretariat for Economic Affairs (seco) has a clear vision about the aims the employment offices and caseworkers should pursue, with a strong focus on rapid re-employment, the caseworkers generally enjoy substantial freedom in how they attempt to achieve these goals and how they treat their clients. This freedom and heterogeneity arises from two factors.

First, the 26 cantons in Switzerland generally enjoy a large autonomy in their implementation of the unemployment insurance law. Although none of them would violate clear legal provisions, such as imposing stronger benefit sanctions than legally permitted, they have substantial leeway on many other margins. The operational costs of the employment offices and their staff, as well as the costs of labour market programmes and benefits payments are fully financed by the federal unemployment insurance funds.[5] Therefore, it is tempting and without serious cost consequences for cantons to pursue their own goals and philosophies to a limited extent. For example, one goal of such a local strategy might be to avoid large numbers of people drawing on welfare benefits, which are financed by the cantons (and their municipalities).[6] A demanding stance vis-à-vis the unemployed may quickly reduce the number of registered unemployed, but it may also lead to poor job matches, instable jobs, and repeated unemployment, which could eventually lead to more people drawing on welfare as they are no longer entitled to unemployment benefits. Being more lenient and trying to satisfy the unemployed person's wishes may on the other hand lead to better job matches, and thus more sustained employment, less job separation, and less turnover in the medium term, or at least reduce the number of persons in need of welfare benefits.

The second source of caseworkers' autonomy arises within the cantons and particularly within the employment offices. Many of the employment office managers consider it important to grant substantial autonomy to caseworkers such that they can develop their own personal counselling style and react to the needs of their clients without being bound by many bureaucratic rules. This is also

[5] In addition to the unemployment benefits, this includes the costs of maintaining and operating the employment offices as well as active labour market programmes. Technically, the cantons bear the costs of the employment offices and active labour market measures, but they are refunded by the federal unemployment insurance funds up to a fixed ceiling that depends on the number of unemployed in the canton.

[6] Rules for social assistance are set at the cantonal level. They vary widely with regard to cost distribution between cantons and municipalities, as well as with regard to form and level of benefits and organisation.

confirmed by the caseworkers, who consider this freedom to be a very important aspect for their job satisfaction.[7]

2.3 The relationship between caseworker and unemployed persons

The relationship between the caseworker and his clients is characterized by the two roles of the caseworker: to help the unemployed person in searching and finding appropriate employment and to monitor whether the unemployed person searches thoroughly enough and is indeed willing to take up any job offer with acceptable pay and within acceptable commuting distance. Some caseworkers put more emphasis on their role as a counsellor and aim for a trustful relationship, whereas other caseworkers may see their policing role to be more important and may be more dominating and demanding vis-à-vis the unemployed person.

To analyse the effects of the caseworker-client relationship a written questionnaire was administered to all caseworkers and office managers in Switzerland about their aims, attitudes, behaviour, etc.[8] A key question asked the caseworker *how important he/she considers the cooperation with the unemployed person*:

Table 1: Survey question on cooperativeness of the caseworker

How important do you consider the cooperation with the jobseeker, regarding placements in jobs, and assignment of active labour market programmes?

- ☐ 1 Cooperation is very important; the wishes of the unemployed person should be satisfied.
- ☐ 2 Cooperation is important, but placements in jobs and active labour market programmes should sometimes be assigned or declined in spite of the unemployed person's wishes.
- ☐ 3 Cooperation is less important; I should assign placements in jobs and active labour market programmes independent of the wishes of the unemployed person

Note: English translation of the respective question in the questionnaire. Questionnaires were in German, French, and Italian.

---

7 See e.g. the French and German versions of the interview protocols in the appendix to Frölich et al. (2007).

8 This data was collected as part of a large evaluation project for the Swiss State Secretariat for Economic Affairs and is described in more detail later. Qualitative face-to-face interviews with the management and caseworkers were conducted beforehand in 12 employment offices. Subsequently, all managers and caseworkers were surveyed with an extensive written questionnaire. For details, see Frölich et al. (2007).

52% of the caseworkers chose option one, 39% of caseworkers chose option 2, and 9% of caseworkers chose option three. Only very few caseworkers did not respond to this question. In the main empirical analysis, we will compare those caseworkers who chose option 1 (cooperative) with those who chose option 2 or 3 (not so cooperative). In additional analyses, we will compare option 1 versus 3, leaving out those who answered with option 2.

When comparing these answers with the responses to other items of the questionnaire we observe that the less cooperative caseworkers consider control and sanctions, job assignments, and employment programmes as instruments that are more important. Counselling meetings and interim jobs (temporary wage subsidies) being less important. They also responded that they tended to assign active labour market programmes to apply pressure and to control their clients' availability for jobs and to give less emphasis to the wishes of the jobseeker.

The variation in the cooperativeness across caseworkers will be exploited to estimate the impact of cooperativeness on the employment prospects of the unemployed. The cooperativeness of the caseworker may be driven by several factors, which we have to take into account as these various characteristics may themselves have an independent effect on the employment chances of their unemployed. First, many characteristics of the caseworker himself may affect his attitude and behaviour. Important factors could be age, gender, education, and, in particular, experience in the form of tenure and participation in caseworker training programmes. Previous own experience of unemployment may also strongly affect the way the caseworker treats his unemployed clients. The caseworker's attitude may also depend a lot on the average characteristics of his clients. For example, a caseworker who is attending mainly unskilled foreigners with a poor employment history may be treating them differently than a caseworker who deals mainly with highly skilled unemployed who experienced unemployment for the first time in their life. Hence, characteristics such as gender, age, nationality, qualification, and past unemployment experience of the clients are likely to affect caseworker's behaviour. Furthermore, characteristics of the local labour market will also be relevant. If the local unemployment rate is low, a caseworker may be less lenient with unemployed than otherwise. As a last aspect, we will also consider certain aspects of the organisation of the employment office, where the caseworker is employed at, as these may also determine the caseworker's behaviour. The data needed to control for those characteristics is presented in the next section.

3 Data

3.1 Data and sample selection

The population for the microeconometric analysis are all individuals who registered as unemployed anytime during the year 2003, and their outcomes are followed up until the end of December 2006. For these individuals very detailed individual information is available from the databases of the unemployment insurance system (AVAM/ASAL) and the social security records (AHV). 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, an employability rating by the caseworker, etc. The data also contain detailed information on registration and de-registration of unemployment, benefit payments and sanctions, participation in ALMP, and the employment histories since January 1990 with monthly information on earnings and employment status (employed, unemployed, non-employed, self-employed).

The databases contain the population of all jobseekers including individuals employed but searching for a job, unemployed without benefit entitlement, and unemployed entitled to benefits. In the econometric analysis, we will focus on the last group since the first two groups are largely immune to potential threats and sanctions of the caseworker.

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. As mentioned above, we exclude jobseekers without benefit claim and individuals who applied for or claim disability insurance. Furthermore, we exclude foreigners without permanent or yearly work permit, as they are not entitled to most of the services of the unemployment insurance. For some unemployed their caseworker is undefined, which may happen if an unemployed person de-registers before being assigned to a caseworker. We also exclude a few employment offices that are not comparable to other offices in 2003. In our main analysis we focus on the prime-age group (24 to 55 years old), with a final sample size of 100,222. See Appendix A for further details.

3.2 Definition of outcomes and treatment variables

Each newly registered unemployed person in 2003 was linked to his first caseworker via the user database of the unemployment insurance system (AVAM). This database contains basic information about each caseworker, such as age and gender. 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 in December 2004, i.e. at the time the questionnaire was sent. The questionnaire contained questions about aims, strategies, processes, and organisation of the employment office and the caseworkers. 1560 caseworkers and employment office managers returned the questionnaire, which is equivalent to a rate of return of 84% of the active caseworkers.[9]

The question most relevant to our analysis was shown in Table 1. Of the 1560 individuals who returned the questionnaire, 159 office managers who did not counsel jobseekers during the year 2003

[9] Obviously, the questionnaire could not be conducted anonymously since we needed to link the answers of each caseworker with their clients. However, caseworkers were guaranteed full confidentiality.

were not asked that question. 16 caseworkers did not answer to this question. 723 answered with option 1, 540 answered with option 2, and 122 answered with option 3. In our main specification, we consider those who answered with option 1 as *cooperative* caseworkers and those who chose option 2 or 3 as less cooperative, which we also sometimes label as "non-cooperative" caseworkers. Although the cooperation attitude of a caseworker may clearly vary between his clients, we expect that a cooperative caseworker be on average more cooperative to all his clients than a less cooperative caseworker is.

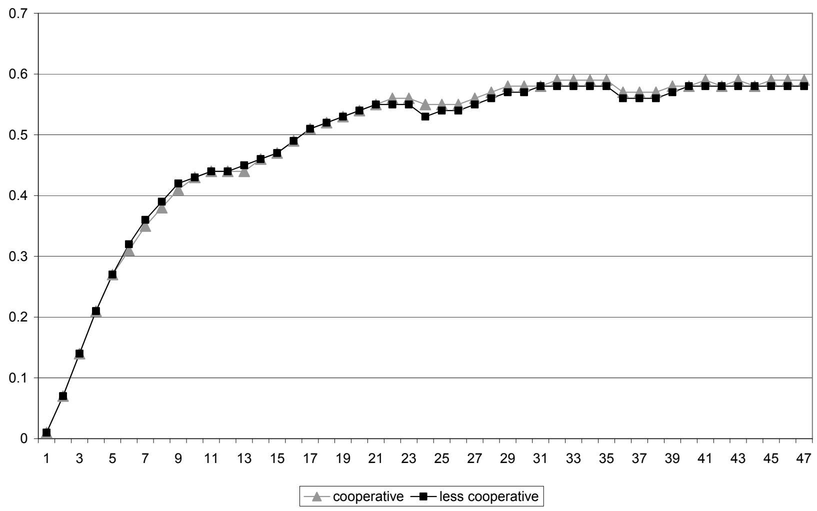
Combining options 2 and 3 to define non-cooperation is based on the presumption that both show a deviation from the full cooperation attitude that more than half of all the caseworkers display. In our robustness analysis, we will also consider other definitions. In particular, we compare option 1 directly with option 3.

We seek to estimate the effect of the cooperation attitude on the employment chances of the unemployed person. The unit of measurement for the outcome variable is thus the unemployed person. We consider an individual as employed in month t if she has de-registered with the employment office because of having found an occupation and has not yet re-registered. Thus, we solely rely on information from the employment office database to determine the employment situation. The employment status is measured with some error since a de-registered individual could have left the active labour force or could have found an occupation after de-registering without letting the employment office know. Nevertheless, a validation study for earlier years where employment data was available from two sources (the unemployment database and the social security data) shows a rather high reliability of our outcome variable, see Frölich et al. (2007).

To analyse the evolvement of the impacts of the caseworker's attitude on the employment probabilities, the employment status, $Y_{i,t_0+\tau}$, is measured until the end of 2006, relative to the time of first registration, t_0. Hence, for individuals who registered in January 2003, their employment status is followed up for 47 months, whereas only 36 months are observed for those registering in December 2003. Observing employment for at least three years allows not only the estimation of short-term effects, but also them medium-term effects of cooperation.

Figure 2 shows the employment rate for our sample relative to the time of registration at the employment office. The black line represents the employment rates for unemployed who were counselled by a less cooperative caseworker, whereas the grey line refers to cooperative caseworkers. The employment rates for both groups of unemployed are very similar. About 1% de-register one month after registering because of having found an occupation. About 7% have found a new job after two months and 44% have found employment one year after becoming unemployed.

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



Note: Average employment rates are for prime age individuals in main sample. The grey (black) line shows averages for the 51923 (48299) unemployed who were counselled by a cooperative (less cooperative) caseworker.

The employment rates in Figure 2, however, do not account for the fact that the unemployed counselled by cooperative and less cooperative caseworkers are quite distinct. Table 2 shows that these two groups of unemployed differ in many characteristics. Foreigners, less qualified unemployed, and unemployed with a poor employability rating are more likely to be counselled by less cooperative caseworkers. In this sense, the less cooperative caseworkers face the more difficult cases, which may be the reason for the small differences observed in Figure 2. A larger effect may be masked by these differences in client's characteristics.

Table 2: Selected characteristics by cooperation attitude of the caseworker

	Caseworker considers cooperation as		
	Very important (option 1)	Important, but (option 2)	Less important (option 3)
Characteristics of the unemployed person			
N (number of unemployed)	51923	39310	8989
Female	0.45	0.44	0.42
Age	36.5	36.6	36.6
Swiss	0.63	0.61	0.56
Foreigner with permanent work permit	0.24	0.25	0.29
Foreigner with yearly work permit	0.13	0.14	0.16
Qualification: unskilled	0.21	0.23	0.28
Qualification: semiskilled	0.16	0.16	0.14
Qualification: skilled without degree	0.04	0.04	0.05
Qualification: skilled with degree	0.58	0.57	0.53
Employability rating: low	0.13	0.14	0.17
Employability rating: medium	0.75	0.74	0.72
Employability rating: high	0.12	0.12	0.11
Characteristics of the caseworker			
Number of case workers	723	540	122
Female	0.42	0.41	0.40
Age	45.2	43.7	42.8
Tenure in employment office in years	5.75	6.01	5.54
Own experience of unemployment	0.65	0.61	0.62
Education: vocational training	0.30	0.34	0.41
Education: above vocational training	0.46	0.41	0.40
Education: tertiary track (university or polytechnic)	0.24	0.25	0.19
Degree in vocational training for caseworkers	0.20	0.26	0.26
Allocation of unemployed to caseworkers [a]			
At random	0.22	0.23	0.22
By alphabet	0.03	0.04	0.07
By number of clients	0.42	0.44	0.44
By industry	0.52	0.57	0.56
By occupation	0.52	0.62	0.57
By age	0.03	0.04	0.01
By employability	0.07	0.06	0.07
By region	0.12	0.11	0.17
Other	0.08	0.08	0.04
Local labour market characteristics			
German speaking employment office	0.69	0.71	0.60
French speaking employment office	0.26	0.22	0.19
Italian speaking employment office	0.05	0.07	0.20
Cantonal unemployment rate	3.70	3.75	3.79
Unemployment rate in industry	4.83	4.93	5.11

Note: The entries in the table are shares in %, means, or number of observations, by subgroup.
[a] Multiple answers to this question were permitted. Hence, the means do not sum up to 1.

Cooperative and less cooperative caseworkers also differ in their own characteristics, which may be related to their efficacy in counselling and placing unemployed. Table 2 shows, more of the non-cooperative caseworkers have participated in the vocational training programme for caseworkers, whereas more of the cooperative caseworkers have a university degree. There are also differences in the organisation of the employment offices, with the non-cooperative caseworkers being more often specialised towards counselling unemployed of a certain industry or occupational group. Lastly, there are also differences in the local labour market situations the caseworkers face, e.g. a somewhat higher local unemployment rate for the non-cooperative caseworkers. We also observe clear differences by language region. Unemployed who live in the Italian-speaking region are more often confronted with less cooperative caseworkers compared to their counterparts in the German and French-speaking regions. The language region will therefore be an important control variable, and we will use interaction terms with language regions in our later regressions throughout.

4 Methodology

As pointed out in the last section, clear differences exist between the unemployed attended by a cooperative or a less cooperative caseworker, which we need to control for. We will seek to find a parsimonious specification that captures the most important factors without introducing too much noise due to irrelevant variables. First, the semiparametric econometric methodology is described.

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] The caseworker is of a particular type with respect to his willingness to cooperate with his client. Let D_i denote the attitude of the

[10] 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.

caseworker who is counselling individual i. In most of the analyses, we will define D_i as binary, where $D_i = 1$ represents a non- or less-cooperative caseworker whereas $D_i = 0$ represents a cooperative caseworker.

We are interested in the impact of a cooperative caseworker 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 a different attitude. We base our analysis on the prototypical model of the statistical evaluation literature with a binary treatment variable D (see Neyman, 1921, Fischer, 1935, Rubin, 1974, 1979). Let

$$Y_{i,t_0+\tau}^{d} \tag{1}$$

be the potential outcome at some time τ after unemployment registration at time t_0, if the caseworker was of type d. In other words, $Y_{i,t_0+\tau}^{0}$ is the employment outcome that would have been observed had person i been counselled by a cooperative caseworker, whereas $Y_{i,t_0+\tau}^{1}$ is the employment outcome that would have been observed had person i been counselled by a non-cooperative caseworker. 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. The average treatment effect for a person who has been counselled by a non-cooperative or by a cooperative caseworker is

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

We will often refer to these parameters as the average treatment effect on the treated (ATET) and the average treatment effect on the non-treated (ATEN), respectively. The following discussion focuses on the ATET, with obvious modifications for the ATEN. Identification of these treatment effects requires further assumptions.

For being able to estimate the expected potential outcomes for different values of *d*, we need to observe variation in D_i that is exogenous with respect to the outcome variable. The observed type of the caseworker D_i might be related to many factors that also have an impact on employment chances, such that in general

$$E[Y^d] \neq E[Y^d \mid D = d]. \tag{2}$$

However if we were to condition on all variables X that determined the type of the caseworker and the potential employment chances of the unemployed person, conditional on X the potential outcomes would be identified:

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

where $\chi \subseteq Supp(X)$.[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 $D=1$ and the $D=0$ population, i.e. $\chi = Supp(X \mid D=1) \cap Supp(X \mid D=0)$. This common support restriction is discussed further below.

The most crucial aspect of the identification strategy thus relies on being able to observe all confounding variables X. To do so, the very detailed linked caseworker-client dataset, described above, is essential together with an understanding of the determinants of the cooperation attitude of the caseworker. The cooperativeness D_i of the caseworker depends on three processes: First, which

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

types of caseworkers are hired, second, how caseworkers are allocated to the unemployed, and third how their attitude develops after having been trained and gained experience on the job. Since attitudes of caseworkers could be related to their general skills of finding jobs for their clients, we include caseworker characteristics such as their age, gender, education, work experience, and experience of own unemployment as covariates. We are also able to control for the allocation process of unemployed to caseworkers since we know from the questionnaire according to which criterion (occupation, alphabet, age, employability, and the like) allocation took place. A further aspect is that caseworkers not only differ in their personalities, but they also react to the types of unemployed they counsel and the labour market environment they face. If vacancies are scarce and rapid re-employment appears difficult, caseworkers may be less demanding than in a more favourable environment. Similarly, a caseworker who counsels mainly individuals with a low employability rating may react differently than a caseworker responsible, e.g., mainly for youth. Therefore, we will include in the analysis also a large number of covariates on the unemployed person's employment history, the local labour market, etc.[12]

4.2 Semiparametric matching estimation

The estimator used is a matching estimator as implemented in Lechner, Miquel, and Wunsch (2006). The advantage of matching estimators is that they are essentially nonparametric and that they allow for arbitrary individual effect heterogeneity.[13] By the conditional independence assumption, the average treatment effect is identified as

[12] The available information is much richer than usually available in studies that rely on the conditional independence assumption (e.g. Heckman and Smith, 1999; Brodaty, Crépon and Fougère, 2001; Larsson, 2003; Dorsett, 2006).

[13] See Heckman, LaLonde, and Smith (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.

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

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, which matches observations from the one population to the other population. 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, $p(x) = \Pr(D = 1 \mid X = x)$, or by matching on *p(X)* and a subset of *X* that is suspected to be highly correlated with the outcome variable as well as with *D*. Such combinations, which they also refer to as balancing scores, can help to ensure that a misspecification of the functional form of the propensity score has only a minor impact. We therefore match on the propensity score and a number of additional covariates, where the propensity score is given a larger weight in the Mahalanobis distance calculation. (The weight is five times as large as for any of the additional covariates.) 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, Lechner, and Steiger, 2005). Moreover, it was subjected to several Monte Carlo studies (e.g. Lechner, 2002) investigating small sample problems and sensitivity issues.

In this paper we use an extension of conventional matching estimation, similar to Lechner, Miquel, and Wunsch (2006), which extends the first-nearest neighbour propensity score matching estimator in several directions: First, as mentioned above, 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.[14] Third, the matching quality is increased by exploiting 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, Ten Have, Feldman, and Kimmel, 2004). Moreover, this procedure should increase precision and may reduce small sample as well as asymptotic bias of matching estimators, see Abadie and Imbens (2006),[15] and thus increase robustness of the estimator in this dimension as well.

The motivation for radius matching is the possibility of efficiency gains without the risk of incurring too 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 (and we note the similarity to the idea of kernel matching). Here, our basic consideration is to be much more cautious with respect to additional bias that 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

[14] This is thus similar to a kernel estimator with a uniform kernel function.

[15] The results of Abadie and Imbens (2006) 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.

'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. When R is reduced the means change little, but the estimated variances increase slightly.

In addition to incorporating all control observations within a certain radius, we also reduce bias by a weighted regression. Here we note that Abadie and Imbens (2006) have shown that the usual one-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. 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 (2006), a somewhat similar procedure has been used by Rubin (1979) and Lechner (2000).

We calculate standard errors as in Lechner, Miquel, and Wunsch (2006) conditional on the weights for the comparison observations, because in Monte Carlo simulations they showed (e.g. Lechner, 2002) good performance in finite samples (their generalization to non-integer weights as used here is trivial). A difference to their implementation is that we have to take into account that the treatment variable is measured at the level of the caseworker such that unemployed persons counselled by the same caseworker are unlikely to be independent observations. We account for this by computing standard errors clustered at the caseworker level. Details are given in Appendix B.

The different steps of the estimator are described in Table B.1 in the appendix. In the first step, a probit model 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 $p(x)$. However, it will be seen in the following section that the common support is very large, and that the loss of observations due is negligible.

5 Analysis of the determinants of cooperativeness

5.1 Estimation of the propensity score

The first step of the empirical analysis consists in examining the determinants of cooperativeness. This is done by regressing the cooperativeness of the caseworker on several of his own characteristics and on characteristics of his clients, the local labour market, and the employment office. Since these estimates also serve as estimates of the propensity score for the subsequent impact analysis, a probit regression on the level of the unemployed person is used, with standard errors clustered at the level of the caseworker.

To examine the robustness of our results, we will consider four different sets of control variables *X*. For our main specification (denoted as *Xset* 1 with 65 regressors), we include a large number of characteristics of the caseworker, the unemployed, the local labour market, and the allocation process within the employment office.

Table 3 shows the estimation results for the probit model for all the regressors that are included in the main specification (*Xset* 1 with 46 regressors) and indicates those variables that are part of a more parsimonious specification denoted as *Xset 4* (see below). As mentioned before, the dependent variable is defined as being equal to one if the caseworker is less cooperative (option 2 or 3), and zero otherwise (option 1).

A first observation is that many of the coefficients are insignificant (the standard errors take the clustering at the caseworker level into account). This implies that caseworkers' attitudes and behaviour are more like a personal characteristic of the caseworker instead of merely being an adaptation to the external environment. The finding that many variables are insignificant cautions against a model with too many *X* variables as they might simply be adding noise to the propensity score matching estimator. We further observe that caseworkers who face many unskilled unemployed or who work in offices that internally specialize by occupation tend to be less cooperative, i.e. more demanding. The latter may be because specialization by occupation will lead to a better knowledge of the employment situation and vacancies in the particular industry. Another observation from Table 3 is that many of the interaction terms with the language region are significant. This may be particularly related to the language in which the written questionnaire was conducted since the translations from German to French and Italian may not have been able to pick up all the nuances of language. We therefore retain all these interaction terms as control variables as they are capturing important differences between the language regions of Switzerland.

Some goodness-of-fit statistics of the probit estimates of the propensity score (Efron's R^2 is 0.06) indicate some overall descriptive power with a substantial amount of randomness remaining.

Table 3: Probit estimates for prime age population (age 24-55, Xset 1)

Binary dependent variable: being a less cooperative caseworker				
N = 100222		coefficient	std error	in Xset 4
Constant		-0.24	0.36	э
French speaking employment office	*	1.39	0.73	э
Italian speaking employment office	***	4.75	1.28	э
Allocation of unemployed to caseworkers (reference: at random)				
By industry		0.14	0.10	
x French speaking region		-0.06	0.20	
x Italian speaking region		-0.45	0.36	
By occupation	**	0.24	0.10	э
x French speaking region		0.16	0.21	
x Italian speaking region		-0.03	0.33	
By age		0.12	0.22	
By employability		-0.09	0.17	
By region		0.06	0.13	
Other		-0.05	0.15	
Characteristics of the caseworker				
Age		-0.01	0.01	э
x French speaking region	*	-0.02	0.01	э
x Italian speaking region	***	-0.06	0.02	э
Female		-0.04	0.10	э
x French speaking region		0.07	0.20	
x Italian speaking region		0.02	0.35	
Experience in employment office (tenure in years)		0.02	0.02	э
x French speaking region		-0.03	0.03	
x Italian speaking region		-0.07	0.05	
Own experience of unemployment		-0.04	0.10	
x French speaking region		-0.15	0.21	
x Italian speaking region		0.03	0.38	
Indicator for missing caseworker characteristics		-0.10	0.25	
Education: above vocational training	*	-0.20	0.11	э
x French speaking region		0.35	0.25	
x Italian speaking region		-0.39	0.41	
Education: tertiary track (university or polytechnic)		-0.20	0.14	
x French speaking region		0.32	0.27	э
x Italian speaking region		-0.35	0.48	
Special vocational training of caseworker		0.09	0.12	э
x French speaking region		0.29	0.36	
x Italian speaking region		0.42	0.35	

Table 3 to be continued.

Table 3 continued

		Coefficient	std error	in Xset 4
Characteristics of the unemployed person				
Female		-0.04	0.03	϶
x French speaking region		-0.10	0.07	
x Italian speaking region		0.03	0.08	
Mother tongue other than German, French, Italian		-0.03	0.04	϶
x French speaking region	*	0.10	0.06	
x Italian speaking region		0.05	0.07	
Qualification: unskilled	**	0.10	0.04	϶
x French speaking region		-0.13	0.08	
x Italian speaking region		-0.04	0.09	
Qualification: semiskilled		0.04	0.05	϶
x French speaking region		0.00	0.08	
x Italian speaking region		-0.07	0.17	
Qualification: skilled without degree		0.02	0.05	϶
x French speaking region	**	0.19	0.09	
x Italian speaking region	*	-0.28	0.17	
Number of unemployment spells in last two years		0.01	0.01	
x French speaking region		-0.01	0.02	
x Italian speaking region	**	0.05	0.02	
Fraction of time employed in last years		0.00	0.03	
x French speaking region	**	-0.13	0.06	
x Italian speaking region		0.03	0.09	
Employability low		0.02	0.11	
x French speaking region		0.15	0.17	
x Italian speaking region		0.15	0.20	
Employability medium		0.00	0.10	
x French speaking region		0.02	0.14	
x Italian speaking region		0.04	0.19	
Local labour market characteristics				
Unemployment rate in canton		0.06	0.06	
x French speaking region		-0.18	0.12	
x Italian speaking region	**	-0.27	0.14	

Note: Standard errors are clustered at the caseworker level. Most variables are interacted with French and Italian language region. (German is the reference language region.) The last column indicates variables included in Xset 4 with ϶.

A crucial aspect of our identification strategy is the conditional independence assumption and thus the selection of the set of control variables. In the main specification, *Xset* 1, we included a large number of caseworker and jobseeker characteristics as well as some indicators of the local labour market and of the employment office, which we deemed important after several interviews with caseworkers and employment office managers. A concern might be that *Xset* 1 contained too few covariates to make the conditional independence assumption plausible. Furthermore, additional variables that are related to the outcome variable could increase precision. On the other hand, including too many variables also runs the risk of including endogenous control variables (i.e. those already been affected by the treatment variable) and/or reducing the common support region. They may also introduce more noise into the estimation of the propensity score.

In *Xset* 2 (= 232 regressors) we added a large number of additional covariates. The additional variables of the unemployed person are age, civil status, children, and earnings in the last job. Furthermore, there are three dummies for education, three dummies for foreign language knowledge, and two dummies for the types of foreigners' work permit. To approximate the unemployed person's labour market history, it contains variables capturing the duration of unemployment in the last two years, the average wage in last ten years, the total number of employment spells in last ten years, the number of employment spells in last five years, an indicator of having been out of labour force in last five years, the fraction of time being employed and unemployed in last ten years, and a dummy for having a zero contribution time to the unemployment insurance. Furthermore, it contains 16 occupation dummies, six industry dummies, and a dummy for looking for a part-time job. With regard to local labour market characteristics, additional variables are municipality size, and the cantonal unemployment rate. All these variables are interacted with the French and Italian language regions. The pension data also indicate the first month of contribution (since 1990), if ever contributed. We also include this variable together with interaction terms with being young/old, and foreigner/Swiss. These interaction terms roughly pick up in which year a foreigner migrated to Switzerland.

Most of these variables turned out to be insignificant in the estimation of the propensity score. Despite of being insignificant, they still affect the calculation of the propensity score and can thus introduce a lot of noise into the matching estimator. By sequentially deleting insignificant variables in the probit model, we generated another *Xset* 3 (= 94 regressors), in an attempt to reduce noise due to insignificant variables. In a general to specific approach, we eliminated covariates whose F-test did not suggest any explanatory power at the 5% level. (One should note that this sequential statistical variable selection is subject to the pre-test problem such that the size of these repeated tests is not exactly 5%. It should rather be considered as an algorithm for selecting the probably most important variables.) However, we retained all variables of *Xset* 1 here, even if insignificant. Hence, *Xset* 1 is a strict subset of *Xset* 3. Further eliminating sequentially all variables with insignificant F-test leads us to *Xset* 4 with 46 regressors, which is our most parsimonious specification.

Table 4 shows various goodness-of-fit statistics of the probit regression for these different sets of *X* variables. The two parsimonious sets *Xset 1* (obtained by deliberate choice) and *Xset 4* (obtained by statistical variable choice) appear to be hardly worse than the two most complex specifications. Comparing *Xset 2* with *Xset 4*, adding almost 200 regressors increases Efron's R^2 by less than 2 percentage points and reduces the number of wrong predictions by less than 800 from more than 40,000. The additional variables thus mainly introduce noise.

Table 4: Goodness of fit measures for different Xsets for prime age population (age 24-55)

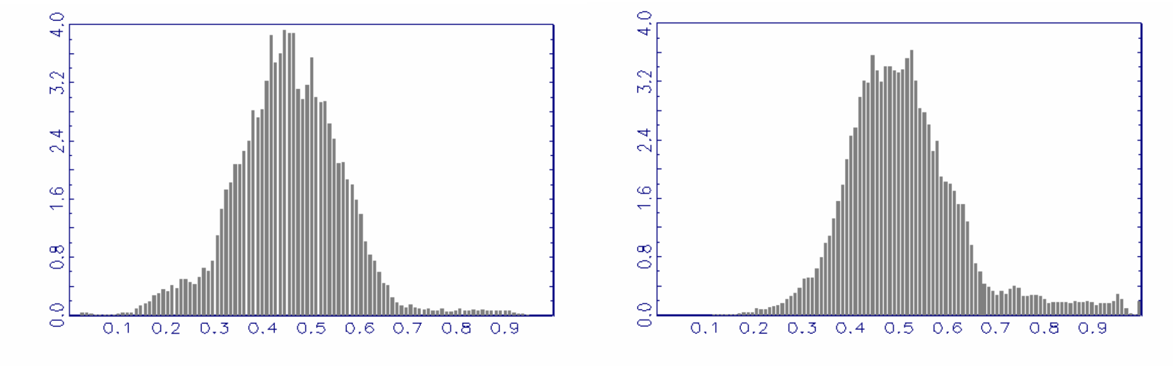
Regressors	Number of covariates	Log-Likelihood	Efron's R^2	NWP	SSR	WSSR
Xset 1	65	-66303	0.058	41496	23560.19	100167.38
Xset 2	232	-65408	0.074	40097	23167.07	100098.15
Xset 3	94	-65807	0.067	41031	23346.20	100183.13
Xset 4	46	-66478	0.056	40832	23626.93	100745.01

Note: The number of observations for each *Xset* is 100222. Efron's R^2 (Efron, 1978) is a measure for residual variation. NWP is the number of wrong predictions, SSR is the sum of squared residuals, and WSSR is the sum of weighted squared residuals.

5.2 Common support

The nonparametric identification strategy relies on estimating the expected counterfactual outcome $E[Y^0 \mid X]$ for every *D=1* observation. This is possible only if for every value of *X* that is observed in the *D=1* population also at least one individual with very similar values of *X* can be found in the *D=0* population. For values of *X* where $\Pr(D=1 \mid X)$ is equal to one this is impossible by definition, and it is very difficult do find comparison observations if $\Pr(D=1|X)$ is very large. Figure 3 shows histograms of the estimated propensity scores for the *D=1* and *D=0* subsamples.

Figure 3: Distribution and common support of the propensity score



Note: Left graph histogram of the estimated propensity scores in the *D=0* sample. Right graph histogram of the estimated propensity scores in the *D=1* sample. Propensity score with Xset 1.

Partly due to availability of a very large sample, the region of common support appears to be very large as well: Even very large values of the propensity score are observed in the *D=0* sample and also very small values are observed in the *D=1* sample. Observations that appear to be outside of the common support are deleted for the matching estimator. For estimating ATET, all *D=1* observations with a propensity score larger than the largest propensity score among all *D=0* observations are deleted. For estimating ATEN, all *D=0* observations with a propensity score smaller than the smallest propensity score among all *D=1* observations are deleted. This leads to a loss of 312 treated observations (= 0.003%) for estimating ATET in our main specification with Xset 1. When estimating ATEN we lose 57 control observations (= 0.0006 %).

5.3 Matching quality

An advantage of matching compared to conventional regression is that one can model the propensity score before examining the outcome variable *Y*. Hence, the propensity score model can be respecified until a reasonable fit is obtained without having the researchers' decisions being affected by the resulting estimates of the treatment effects. In this sense, this approach is immune to the respecification and pre-testing problem of conventional regression. In addition, the region of common support of the *X* regressors can also be examined before the outcome variable *Y* is incorporated. As suggested by Rosenbaum and Rubin (1983), matching on the propensity score leads to a balancing of the covariates *X* in the $D=1$ and $D=0$ population. Hence, after matching treated and control, the joint distribution of *X* and thereby the marginal distributions of *X* should be identical in both matched subsamples. Thus, a simple way to validate the specification is to test for equality of means in the two subsamples. However, since we use a more complex estimation procedure than simple matching involving radius matching and regression, a more appropriate balancing test is to the estimate the effect of the treatment on the covariates. If the model is correctly specified, this effect must be zero asymptotically and should not be significantly different from zero in finite samples.

For both groups, Table 5 shows the estimated means, their difference, and t-stats for the hypothesis that their means are equal. We find that the matching quality with respect to almost all covariates is very good. The only exception is the number of unemployment spells in the past two years: the weighting procedure over-adjusts for this variable by raising the mean in the comparison group from 0.56 to 0.68. In one of our later specifications, we therefore include this variable as an additional covariate on which matching is conducted.

Table 5: Matching quality Xset 1, prime age population (age 24-55)

Control variables used in propensity score	Predicted mean for control	Mean of treated	Predicted mean difference (treated-control)	t-value for test that difference is zero
Observations (after imposition of common support)	51923	47987		
French speaking employment office	0.21	0.21	0.00	0.00
Italian speaking employment office	0.09	0.09	0.00	0.00
Allocation of unemployed to caseworkers (reference: at random)				
By industry	0.57	0.57	0.00	-0.08
x French speaking region	0.09	0.08	0.00	0.02
x Italian speaking region	0.04	0.04	0.00	0.01
By occupation	0.60	0.61	0.01	0.36
x French speaking region	0.16	0.16	0.00	0.03
x Italian speaking region	0.06	0.06	0.00	0.01
By age	0.03	0.03	0.00	0.12
By employability	0.06	0.06	0.00	0.23
By region	0.11	0.12	0.01	0.44
Other	0.08	0.07	0.00	-0.17
Characteristics of the caseworker				
Age	43.46	43.63	0.17	0.22
x French speaking region	9.25	9.30	0.05	0.04
x Italian speaking region	3.71	3.75	0.04	0.05
Female	0.42	0.41	-0.01	-0.37
x French speaking region	0.08	0.08	0.00	0.00
x Italian speaking region	0.04	0.03	-0.01	-0.88
Tenure in employment office (in years)	5.98	5.91	-0.07	-0.29
x French speaking region	1.35	1.34	-0.02	-0.09
x Italian speaking region	0.67	0.65	-0.02	-0.16
Own experience of unemployment	0.61	0.61	0.00	0.01
x French speaking region	0.14	0.14	0.00	0.05
x Italian speaking region	0.05	0.05	0.01	0.65
Indicator for missing caseworker characteristics	0.05	0.04	0.00	-0.06
Education: above vocational training	0.43	0.41	-0.02	-0.52
x French speaking region	0.09	0.09	0.00	0.02
x Italian speaking region	0.04	0.03	-0.01	0.01
Education: tertiary track (university or polytechnic)	0.23	0.24	0.01	0.44
x French speaking region	0.08	0.09	0.00	0.02
x Italian speaking region	0.01	0.02	0.01	0.01
Special vocational training of caseworker	0.26	0.25	-0.01	-0.30
x French speaking region	0.01	0.02	0.00	0.01
x Italian speaking region	0.06	0.05	-0.01	0.01

Table 5 to be continued.

Table 5 continued

	Predicted mean for control	Mean of treated	Predicted mean difference (treated-control)	t-value for test that difference is zero
Characteristics of the unemployed person				
Female	0.44	0.43	0.00	-0.25
x French speaking region	0.09	0.09	0.00	-0.03
x Italian speaking region	0.04	0.04	0.00	0.36
Mother tongue other than German, French, Italian	0.34	0.32	-0.02	-1.17
x French speaking region	0.07	0.07	0.07	0.07
x Italian speaking region	0.03	0.03	0.03	0.03
Qualification: unskilled	0.26	0.26	0.26	0.26
x French speaking region	0.05	0.05	0.05	0.05
x Italian speaking region	0.03	0.03	0.03	0.03
Qualification: semiskilled	0.16	0.16	0.16	0.16
x French speaking region	0.04	0.04	0.04	0.04
x Italian speaking region	0.01	0.01	0.01	0.01
Qualification: skilled without degree	0.05	0.05	0.05	0.05
x French speaking region	0.02	0.02	0.02	0.02
x Italian speaking region	0.01	0.01	0.01	0.01
Number of unemployment spells in last two years	0.68	0.59	-0.09	-2.34
x French speaking region	0.16	0.16	0.16	0.16
x Italian speaking region	0.09	0.09	0.09	0.09
Fraction of time employed in last years	0.78	0.78	0.78	0.78
x French speaking region	0.16	0.16	0.16	0.16
x Italian speaking region	0.07	0.07	0.07	0.07
Employability low	0.16	0.16	0.16	0.16
x French speaking region	0.02	0.02	0.02	0.02
x Italian speaking region	0.02	0.02	0.02	0.02
Employability medium	0.72	0.72	0.72	0.72
x French speaking region	0.16	0.16	0.16	0.16
x Italian speaking region	0.05	0.05	0.05	0.05
Local labour market characteristics				
Unemployment rate in canton	3.76	3.75	0.00	-0.06
x French speaking region	0.88	0.89	0.00	0.02
x Italian speaking region	0.38	0.38	0.00	-0.05

Note: Matching quality for estimation of ATET based on full estimator and after imposition of common support.

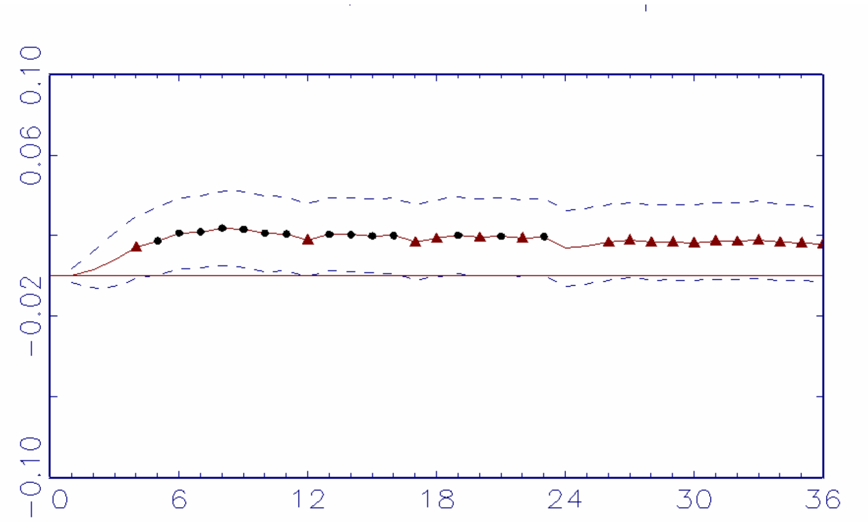
6 Estimated treatment effects

Section 6.1 gives the main empirical results of the propensity score matching. To examine the robustness of our results in Section 6.2, we consider four different sets of control variables *X*, different outcome variables, as well as different definitions of the treatment variable *D*. For each of these different combinations, we re-estimated the propensity score and applied the common support restrictions.

6.1 Impact of a less cooperative caseworker

The following figures show the matching estimates when the propensity score is estimated with *Xset* 1 and treatment is defined as *cooperation not very important* versus *very important*. That is, *D* is defined as one if the caseworker selected option 2 or option 3, and is defined as zero if the caseworker selected option 1 (see Table 1). In this specification, we isolate those caseworkers who place very much emphasis on cooperation versus all the others.

Figure 4: Impact of having a less cooperative caseworker on employment in %-points



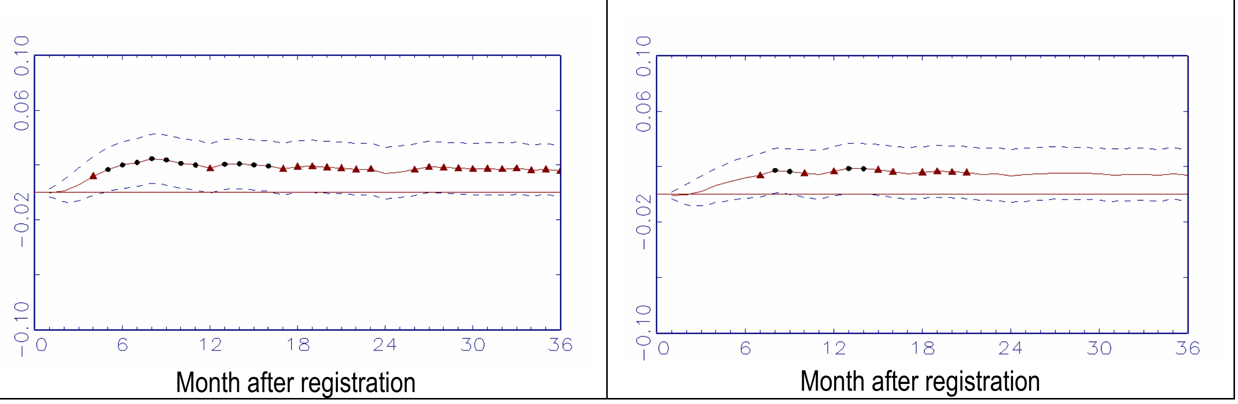
Note: Average treatment effect on the treated (ATET) on employment, in percentage points. Prime age unemployed (24 to 55 years). Abscissa: Month after registration of unemployment. Ordinate: Treatment effect on employment in %-points. Dots (triangles) indicate significance at the 5% (10%) level. The dashed line represents the pointwise 95% confidence interval.

Figure 4 gives the estimates for the outcome variable *employment* for the subsequent 36 months after registration. (Triangles indicate point-wise significance at the 10% level, dots at the 5% level.) These results indicate that having a less cooperative caseworker increases the employment probability by about 2%-points. The effect sets in about five months after registration and is relatively stable until month 36. From month 24 onwards, it is significant only at the 10% level, though.

An increase in employment probability by 2%-points is a non-negligible effect, given that it only requires a change in the caseworkers' attitudes and behaviour towards their clients. At the same time, it is also likely to lead to additional cost savings for the unemployment insurance system since more demanding caseworkers may also often impose more sanctions in the form of suspension of benefits. Unfortunately, reliable data about potential costs of such a policy shift are not available.

Figure 5: Impact of having a less cooperative caseworker on stable employment

Figure 5.1: Six months stability | *Figure 5.2: Twelve months stability*



Note: Average treatment effect on the treated (ATET) on *stable* employment, in %-points. In left graph, employment is only considered as stable if the employment spell has a duration of at least 6 months. In right graph, a duration of at least 12 months is required. Prime age unemployed (24 to 55 years). Abscissa: Month after registration of unemployment. Ordinate: Treatment effect on employment. Dots (triangles) indicate significance at the 5% (10%) level. Dashed line represents pointwise 95% confidence interval.

We also examined the average treatment effects separately for four subgroups: qualified, unqualified, older than 55 years, and younger than 24 years. Most of these results, however, turned out to be insignificant, mainly due to the smaller sample size. (This was already expected from Figure 4 where statistical precision was at the margin of detecting a significant effect.) However, estimates from other specifications (available on request from the authors) suggest that the qualified, the unqualified, and the older unemployed tend to benefit from having a less cooperative caseworker, while the effects for the young are always insignificant.

A general concern with tougher caseworkers is that they might push jobseekers into precarious or unstable jobs, which due to the poor match quality might lead to higher separation rates soon after.

To examine the stability of jobs we define an individual as being in *stable employment* in a given month if the employment spell is of at least *six* months duration. Alternatively, we require at least 12 months duration. Figure 5 gives the treatment effects on stable employment (analogously to Figure 4), which are positive throughout and only slightly smaller than in Figure 4. Although the statistical precision is insufficient to draw very strong conclusions, it does not appear that non-cooperativeness of the caseworkers would lead to unstable jobs.

6.2 Sensitivity analysis

The previous section showed a positive impact of non-cooperativeness of the caseworker on employment outcomes. Albeit being statistically significantly different from zero, the confidence bands were nevertheless rather wide. In this section, we examine alternative specifications, which all point towards a positive impact of non-cooperativeness, although with different degrees of statistical confidence.

Table 6 shows the estimation results for various alternative specifications. For conciseness, we do not show the entire graphs but display the effects only for 6, 12, 18, and 30 months after registration. The first three rows refer to alternative definitions of employment and correspond to Figures 4 and 5. They show that the positive impact does not seem to have been arisen at the cost of instability of jobs. The magnitude of the effects tends to decrease when we look at stable employment but these differences are not significant. The following rows show the results for various subgroups, where the results are mostly positive but very imprecise throughout.

The subsequent rows examine alternative specifications of the estimator and of the regressor set. First, we reduce the radius from 0.9 to 0.1 in the propensity score matching estimator. This is similar to a reduction of the number of neighbours in k-nearest neighbour matching. Coefficients and standard errors are not much affected.

As an alternative specification, we exclude the regressor 'employability rating of the unemployed', which might potentially be endogenous as it is a subjective assessment made by the caseworker. For instance, more demanding caseworkers might consider the same type of client as easier to place than the more lenient caseworkers might. The results do not change substantially. Second, we add the number of clients that caseworkers report to counsel on average to Xset 1, because one may want to control for caseworker's workload if more (or less) cooperative caseworkers are more successful in placing clients and hence more clients are assigned to them, which might negatively affect their efficiency. Again, the results are hardly affected. Third, we include a dummy for registering as unemployed in the second semester of 2003, since rules for benefit entitlement were tightened in July 2003. Although it has no significant impact on cooperation behaviour, it reduces standard errors of employment effects by increasing estimation efficiency. Finally, we include the number of unemployment spells in the past two years as an additional covariate on which to match exactly, because the matching quality was imperfect in Table 5 with respect to this variable. The estimated effects increase for month 6, but slightly decrease for the later months and become less significant.

As discussed above, the choice of control variables is an important aspect for the credibility of the conditional independence assumption. Table 6 shows estimates of propensity score matching with *Xsets* 2, 3, and 4, respectively, which all show positive effects but with different degrees of precision. It seems that the two very large regressor sets 2 and 3, with many insignificant variables, lead to noisy estimates, whereas the results with Xset 4, where the insignificant variables have been purged, are much more precise. Hence, including too many control variables that are not related to the treatment variable can introduce substantial noise into propensity score matching.

As a further robustness check, we examined the logit estimator as a parametric alternative to propensity score matching. The effects remain positive but smaller compared to the matching estimates. This could be due to the functional form assumption imposed.

Finally, we estimate the ATEN to complement the estimates of the ATET. The effects remain positive but much smaller and clearly insignificant. A possible interpretation of this finding is that those caseworkers who decided or happened to be more demanding were right in doing so, whereas the gains from being more demanding are smaller or even zero for those caseworkers who decided against this strategy. This is what we would expect if caseworkers adapt to their environment. This also explains the previously mentioned small effects of the parametric logit estimator because the logit model permits only very limited effect heterogeneity and, more or less, measures some kind of average between ATET and ATEN.

In some further analysis, we also consider alternative definitions of the treatment variable. First, we discard caseworkers of the intermediate type and only consider an attitude as being less cooperative if the caseworker has explicitly chosen option 3, see Table 1. (In other words, we eliminate all caseworkers who chose option 2.) In our main specification, estimates are still positive, but less precise due to the smaller sample size. When using the large Xset 2, estimates remain positive but less precise. The estimates of the parametric logit model are now similar to the matching estimates. Hence, all these estimates remain positive, but tend to decrease over time. The results for ATEN are partly negative, on the other hand, although not statistically significantly so. We therefore restrict ourselves to interpreting the ATET estimates as overall positive, whereas we cannot say much about ATEN.

Table 6: The impact of non-cooperativeness on employment, robustness analysis

	Obs.	Month 6		Month 12		Month 18		Month 30	
		Effect at month after registration							
		Coeff.	t-Stat	Coeff.	t-Stat	Coeff.	t-Stat	Coeff	t-Stat
Non-cooperativeness (option 2 and 3) versus Cooperativeness (option 1)									
Alternative definitions of the outcome variables (Pscore matching with Xset 1, age 24-55)									
Employment	100222	0.021	2.360	0.018	1.955	0.019	1.917	0.017	1.725
Six-months stable employment	100222	0.020	2.316	0.018	1.908	0.019	1.959	0.017	1.785
Twelve-months stable employment	100222	0.012	1.614	0.017	1.825	0.016	1.656	0.015	1.532
Effects on employment for subgroups (Pscore matching with Xset 1, age 24-55)									
Qualified, age 24-55	61191	0.007	0.669	0.008	0.710	0.011	0.994	0.007	0.637
Unqualified, age 24-55	39031	0.017	1.384	0.007	0.552	0.003	0.229	0.000	-0.033
Older than 55 years	8580	-0.010	-0.626	0.012	0.690	-0.002	-0.115	0.009	0.437
Younger than 24 years	28980	-0.006	-0.408	0.005	0.310	0.016	1.091	0.009	0.615
Alternative specifications (age 24-55)									
PSM with radius 0.1 (Xset 1)	100222	0.022	2.333	0.021	2.128	0.020	1.978	0.018	1.819
PSM without employability (Xset 1)	100222	0.018	2.047	0.020	2.247	0.016	1.658	0.016	1.614
PSM with number of clients (Xset 1)	100222	0.020	2.319	0.018	1.944	0.022	2.271	0.014	1.475
PSM with dummy for 2nd semester (Xset 1)	100222	0.023	2.557	0.021	2.224	0.025	2.466	0.022	2.280
PSM exact on number of unemployment spells (Xset1)	100222	0.023	2.611	0.016	1.784	0.018	1.922	0.014	1.514
Pscore matching (Xset 2)	100222	0.011	1.229	0.001	0.151	0.003	0.325	0.001	0.059
Pscore matching (Xset 3)	100222	0.010	1.137	0.001	0.083	0.004	0.368	-0.001	-0.056
Pscore matching (Xset 4)	100222	0.032	3.474	0.023	2.508	0.026	2.696	0.020	1.996
Logit estimates (Xset 1)	100222	0.008	1.251	0.005	1.098	0.002	0.388	-0.002	-0.322
ATEN using PSM (Xset 1)	100222	0.002	0.028	0.002	0.209	0.008	0.813	0.016	1.680
Non-cooperativeness (option 3) versus Cooperativeness (option 1), eliminating caseworkers with option 2									
Pscore matching (Xset 1, age 24-55)	60912	0.036	1.852	0.001	0.044	0.010	0.507	0.011	0.530
Qualified Age 24-55	37346	0.028	1.226	0.015	0.670	0.024	1.058	0.014	0.623
Unqualified Age 24-55	23566	0.056	2.171	0.003	0.124	0.021	0.755	0.011	0.397
Old (> 55 years)	5201	0.003	0.096	0.001	0.042	0.016	0.413	0.008	0.195
Young (< 24 years)	18059	-0.017	-0.670	0.009	0.342	-0.003	-0.114	-0.007	-0.270
Pscore matching (Xset 2, age 24-55)	60912	0.032	1.534	0.004	0.232	0.009	0.438	0.011	0.530
Logit estimates (Xset 1, age 24-55)	60912	0.028	2.583	0.008	1.182	0.012	1.290	0.010	1.010
ATEN using PSM (Xset 1, age 24-55)	60912	-0.038	-1.529	-0.012	-0.609	0.003	0.129	0.004	0.197
Intermediate cooperativeness (option 2) versus no or full cooperativeness (option 1 or 3)									
Pscore matching (Xset 1, age 24-55)	100222	0.010	1.159	0.009	0.971	0.009	0.873	0.004	0.442

Note: Standard errors are clustered at the caseworker level. Pscore matching and PSM are abbreviations for propensity score matching.

In the last row of Table 6, we tested for possible non-linearities in the treatment variable. One could imagine that a caseworker with intermediate cooperation behaviour might perform better compared to a caseworker with very low or high cooperativeness. Therefore, as a final check we define *D=1* if the caseworker chose option 2 and *D=0* if the caseworker chose option 1 or 3. The estimates are close to zero and insignificant, thereby not confirming this hypothesis.

7 Conclusions

In most countries, caseworkers have substantial autonomy in the extent to which they cooperate with their clients. Some place more emphasis on counselling, whereas others also consider monitoring of job search as a very important part of their work. Using large and informative administrative data on unemployed persons merged with data on caseworkers and their employment offices, obtained from a detailed questionnaire, we investigate which attitude towards unemployed is more successful for their subsequent employment chances. These data allow us to control for potential selection bias by semiparametric matching estimators and to account for treatment effect heterogeneity. Estimates are obtained up to the first three years after unemployment registration.

More than half of the caseworkers responded that they considered cooperation with unemployed as very important and that the wishes of the unemployed are of key importance for their decisions. However, the estimates suggest that the employment probabilities of those unemployed persons who were counselled by less cooperative caseworkers were higher because of their less cooperative attitude. Such unemployed persons had about 2 percentage point higher employment probabilities during the first three years after registration than similar unemployed persons who were counselled by (somewhat) less demanding caseworkers. The most plausible explanation for our finding is that caseworkers indeed influence their clients' behaviour to search for jobs and accept job offers. In an extensive sensitivity analysis, almost all results confirmed the sign of the effect, but in several cases, the effect was insignificant.

A Data Appendix

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 239004 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.[16] 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, which 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 which 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, 219540 persons remain who registered in one of the 103 offices. For 215251 persons the first caseworker was well defined, whereas for the other 4289 no caseworker was (yet) assigned. The reason for this is that it may often take several weeks until the first counselling meeting with a caseworker takes place, e.g. after having participated in a one-day course that explains the duties and rights of an unemployed person. In total, 1891 different caseworkers were identified in the data.

[16] 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. Finally, we lose about 25% of observations whose caseworker had either not responded to the questionnaire in general or to the cooperativeness question in particular. Comparing the samples before and after dropping these observations, we do not find any differences neither in their characteristics, X, nor in their observed outcomes, Y.

Table A.1: Sample selection criteria for empirical analysis

	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 unemployed whose caseworker did not respond to the questionnaire	-31'469	105,137
Exclude unemployed whose caseworker did not respond to the cooperativeness question	-4'915	100,222

B Further details on the estimator

B.1 Matching protocol

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 $E[Y^0 \mid D=1]$ 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. The modifications for ATEN are obvious. $\tilde{x}$ includes the two dummy variables French speaking and Italian speaking employment office. $\tilde{x}$ is included to ensure a high match quality with respect to these critical variables.

B.2 Standard errors for clustered matching

Lechner (2001) suggested an estimator of the asymptotic standard errors for ATET conditional on the estimated weights. Since the treatment variable $D \in \{0,1\}$ is measured at the level of the caseworker but the outcome variable is measured at the level of the jobseeker, for the computation of the standard errors we have to take into account that the outcomes across the jobseekers counselled by the same caseworker may be correlated. The calculation of the clustered standard errors is described in the following.

The matching estimator of the potential outcome has the general form:

$$\hat{Y}^l = \sum_{i=1}^{N} \underline{1}(D_i = l) w_i^l y_i$$

where $i = 1, \ldots, N$ indexes the unemployed persons and where the sum of the weights is one:

$$\sum_{i=1}^{N} \underline{1}(D_i = l) w_i^l = 1 .$$

To introduce the cluster structure we can re-write the matching estimator using a double sum

$$\hat{Y}^l = \sum_{j=1}^{J} \sum_{i=1}^{N} \underline{1}(D_i = l) \underline{1}(C_i = j) w_i^l y_i ,$$

where i indexes the unemployed persons and $j = 1, \ldots, J$ indexes the J caseworkers. The variable $C_i \in \{1,..,J\}$ indicates the caseworker who is in charge of the unemployed i. The number of clients of caseworker j in the $D=l$ group and weighted by w_i^l is thus given by

$$N^j := \sum_{i=1}^{N} \underline{1}(D_i = l) \underline{1}(C_i = j) w_i^l .$$

We can compute the variance allowing that the outcomes across unemployed persons counselled by the same caseworker are dependent, but assume that observations across caseworkers are independent:

$$Var(\hat{Y}^l) = \sum_{j=1}^{J} Var\left[\sum_{i=1}^{N} \underline{1}(D_i = l)\underline{1}(C_i = j) w_i^l y_i\right]$$
$$= \sum_{j=1}^{J} N^{j2}\, Var\left[\frac{1}{N^j}\sum_{i=1}^{N} \underline{1}(D_i = l)\underline{1}(C_i = j) w_i^l y_i\right]$$

Hence, the variance is obtained by summing over the caseworkers the variance of the expression A_j which is defined as

$$A_j = \frac{1}{N^j}\sum_{i=1}^{N} \underline{1}(D_i = l)\underline{1}(C_i = j) w_i^l y_i \,.$$

Since the A_j are independent across the caseworkers, we can estimate $Var\left(A_j\right)$ as

$$\widehat{Var}\left(A\right) = \frac{1}{J}\sum_{j=1}^{J}\left[A_j - \frac{1}{J}\sum_{j=1}^{J} A_j\right]^2,$$

which we now plug into the formula for $Var(\hat{Y}^l)$.

In the implementation, we ignore the regression step in the matching estimator. The justification is given by Abadie and Imbens (2006) showing that a nonparametric regression step after the matching does remove the bias in the asymptotic distribution without affecting its variance. Although our estimator differs in some respects from the fixed-number-of-neighbours estimator they consider, the general set-up is very similar. It may be conjectured that since we use a parametric instead of a nonparametric regression the variance is indeed reduced that would lead to conservative inference.

References

Abadie, A., and G. W. Imbens (2006): "Large Sample Properties of Matching Estimators for Average Treatment Effects", *Econometrica*, 74, 235-267.

Abbring, J., G. van den Berg, and J. van Ours (2005): "The Effects of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment", *Economic Journal*, 115, 602-630.

Ashenfelter, O., D. Ashmore, and O. Deschenes (2000): "Do Unemployment Insurance Recipients Actively Seek Work? Evidence From Randomized Trials in Four U.S. States", *Journal of Econometrics*, 125, 53-75.

Black D. A., and J. A. Smith (2004): "How Robust is the Evidence on the Effects of College Quality? Evidence from Matching", *Journal of Econometrics*, 121, 99-124.

Black D. A., J. A. Smith, M. Berger, and B.J. Noel (2003): "Is the threat of reemployment services more effective than the services themselves? Evidence from random assignment in the UI system," *American Economic Review*, 93, 1313-1327.

Bloom, H., C. Hill, and J. Riccio (2003): "Linking program implementation and effectiveness: lessons from a pooled sample of welfare-to-work experiments", *Journal of Policy Analysis and Management*, 22, 551-575.

Brodaty T., B. Crépon, and D. Fougère (2001): "Using Kernel Matching Estimators to Evaluate Alternative Youth Employment Programs: Evidence from France, 1986-1988", in M. Lechner and F. Pfeiffer (eds.): *Econometric Evaluations of Labour Market Policies*, Heidelberg: Physica Verlag, 85-124.

Dolton, P., and D. O'Neill (1996): "Unemployment duration and the restart effect: some experimental evidence", *Economic Journal*, 106, 387-400.

Dorsett, R. (2006): "The New Deal for Young People: Effect on the Labour Market Status of Young Men", *Labour Economics*, 13, 405-422.

Efron, B. (1978): "Regression and ANOVA with Zero-One Data: Measures of Residual Variation"*, Journal of the American Statistical Association*, 73, 113-212.

Fehr, E., and K. Schmidt (2001): "Theories of Fairness and Reciprocity - Evidence and Economic Applications", *Working Paper, Institute of Empirical Research in Economics,* University of Zurich.

Ferro-Luzzi, G., Y. Flückiger, J. Ramirez, and A. Vassiliev (2005): "Swiss unemployment policy: An evaluation of the public employment service", *Swiss Journal of Sociology*, 30, 319-337.

Fisher, R. A. (1935), *Design of Experiments*, Oliver and Boyd: Edinburgh.

Fredriksson, P., and B. Holmlund (2003): "Improving incentives in unemployment insurance: A review of recent research", *Discussion Paper, IFAU*.

Frölich, M., M. Lechner, S. Behncke, S. Hammer, N. Schmidt, S. Menegale, A. Lehmann, and R. Iten (2007), *Einfluss der Rav auf die Wiedereingliederung von Stellensuchenden,* Schweizerisches Staatssekretariat für Wirtschaft (seco),

SECO Publikation, Arbeitsmarktpolitik No 20, http://www.seco.admin.ch/dokumentation/publikation/00008/02015/index.html?lang=de.

Gerfin M., and M. Lechner (2002): "Microeconometric Evaluation of the Active Labour Market Policy in Switzerland", *Economic Journal*, 112, 854-893.

Gerfin, M., M. Lechner, and H. Steiger (2005): "Does subsidised temporary employment get the unemployed back to work? An econometric analysis of two different schemes", *Labour Economics*, 12, 807-835.

Gorter, C., and G. R. J. Kalb (1996): "Estimating the effect of counselling and monitoring the unemployed using a job search model", *Journal of Human Resources*, 31, 590-610.

Graversen, B., and J. van Ours (2006): "How to help unemployed find jobs quickly; experimental evidence from a mandatory activation program, *Discussion Paper, Center*, Tilburg University.

Ham J., and R. LaLonde (2005): "Special Issue on Experimental and Non-Experimental Evaluation of Economic Policy and Models (Introduction)", *Journal of Econometrics*, 125, 1-13.

Heckman J., H. Ichimura, J. Smith, and P. Todd (1998): "Characterizing Selection Bias Using Experimental Data", *Econometrica*, 66, 1017-1098.

Heckman, J., R. LaLonde, and J. Smith (1999): "The Economics and Econometrics of Active Labor Market programs," in O. Ashenfelter and D. Card (eds.), *Handbook of Labour Economics*, Vol. 3, 1865-2097.

Heckman, J., and J. Smith (1999): "The Pre-Program Earnings Dip and the Determinants of Participation in a Social Program: Implications for Simple Program Evaluation Strategies", *Economic Journal*, 109, 313-348.

Imbens, G. W. (2000): "The Role of the Propensity Score in Estimating Dose-Response Functions", *Biometrika*, 87, 706-710.

Imbens, G. W. (2004): "Nonparametric Estimation of Average Treatment Effects under Exogeneity: A Review", *Review of Economics and Statistics*, 86, 4-29.

Joffe, M., T. R. Ten Have, H. I. Feldman, and S. Kimmel (2004): "Model Selection, Confounder Control, and Marginal Structural Models", *American Statistician*, 58-4, 272-279.

Larsson, L. (2003): "Evaluation of Swedish Youth Labor Market Programs", *Journal of Human Resources* 38(4), 891-927.

Lalive, R., J. van Ours, and J. Zweimüller (2005): "The Effect of Benefit Sanctions on the Duration of Unemployment", *Journal of European Economic Association*, 3, 1386-1407.

Lechner, M. (2000): "An Evaluation of Public Sector Sponsored Continuous Vocational Training Programs in East Germany", *Journal of Human Resources*, 35, 347-375.

Lechner, M. (2001): "Identification and Estimation of Causal Effects of Multiple Treatments under the Conditional Independence Assumption", in: M. Lechner and F. Pfeiffer (eds.), *Econometric Evaluation of Active Labour Market Policies*, 43-58, Heidelberg: Physica.

Lechner M. (2002): "Some Practical Issues in the Evaluation of Heterogeneous Labour Market Programmes by Matching Methods", *Journal of the Royal Statistical Society, Series A*, 165, 59-82.

Lechner, M., R. Miquel, and C. Wunsch (2006): "Long-run effects of Public Sector Sponsored Training in West Germany", revised version of *Discussion Paper 2004-19*, *Department of Economics*, University of St. Gallen.

Machin, S., and A. Manning (1999): "The causes and consequences of long-term unemployment in Europe," in: O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, Vol. 3c, 3085-3139, Amsterdam: North-Holland.

Martin, J., and D. Grubb (2001): "What Works and for Whom: A Review of OECD countries' experiences with active labour market policies," *Swedish Economic Policy Review*, 8, 9-56.

Meyer, B. D. (1995): "Lessons from the U.S. unemployment insurance experiments", *Journal of Economic Literature*, 33, 91-131

Neyman, J. (1923): "On the Application of Probability Theory to Agricultural Experiments. Essay on Principles", *Statistical Science*, Reprint, 5, 463-480.

Pavoni, N., and G. Violante (2007): "Optimal Welfare-to-Work Programs," *Review of Economic Studies*, 74, 283-318.

Ramirez, J., and A. Vassiliev (2007): "An efficiency comparison of regional employment offices operating under different exogenous conditions", *Swiss Journal of Economics and Statistics*, 2007, 31-48.

Rosenbaum P., and D. Rubin (1983): "The Central Role of the Propensity Score in Observational Studies for Causal Effects", *Biometrika*, 70, 41-55.

Rubin, D. (1974): "Estimating Causal Effects of Treatments in randomized and nonrandomized Studies," *Journal of Educational Psychology*, 66, 688-701.

Rubin, D. (1979): "Using Multivariate Matched Sampling and Regression Adjustment to Control Bias in Observational Studies", *Journal of the American Statistical Association*, 74, 318-328.

Shavell, S., and L. Weiss (1979): "The optimal payment of unemployment insurance benefits over time", *Journal of Political Economy*, 87, 1347-1362.

Sheldon, G. (2003): "The efficiency of public employment services: a nonparametric matching function analysis for Switzerland", *Journal of Productivity Analysis*, 20, 49-70.

Svarer, M. (2007): "The effect of sanctions on the job finding rate: evidence from Denmark", IZA discussion paper 3015.

Van den Berg, G. J., B. van der Klaauw, and J. C. van Ours (2004): "Punitive sanctions and the transition rate from welfare to work", *Journal of Labor Economics*, 22, 211-241.

Wunsch, C. (2005): "Labour Market Policy in Germany: Institutions, Instruments and Reforms since Unification," *Discussion Paper, Department of Economics*, University of St. Gallen.

Wunsch, C. (2007): "Optimal Use of Labour Market Policies," *Discussion Paper, Department of Economics*, University of St. Gallen.