

Effects of contracting out employment services: Evidence from a randomized experiment^a

by

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January, 2010

Abstract

In many countries welfare services that traditionally have been provided by the public sector are increasingly being contracted out to private providers. But are private contractors better at providing these services? We use a randomized experiment to empirically assess the effectiveness of contracting out employment services to private placement agencies. Our results show that unemployed at private placement agencies have a much closer interaction with their placement worker than unemployed at the Public Employment Service (PES). In particular, unemployed at private agencies receive more assistance in improving their job search technology. We do not find any overall difference in the probability of employment between private placement agencies and the PES, but this hides important heterogeneities across different types of unemployed. We find evidence that private providers are better at providing employment services to immigrants, and also indications that they may be worse for adolescents. Any effects tend to fade away over time.

Keywords: Job placement, Contracting out
JEL-codes: J68

^a We have benefited for comments and suggestions from Peter Fredriksson, Hans Grönqvist, Gabriella Sjögren Lindquist, Andreas Madestam, Oskar Nordström Skans, Peter Skogman Thoursie, Johan Vikström and seminar participants at the Institute for Labour Market Policy Evaluation (IFAU), the Public Employment Service in Sweden, the Swedish Fiscal Policy Council and the Swedish Ministry of Employment. We are also grateful to Rolf Adolfsson and Lil Ljunggren Lönnberg at the Public Employment Service in Sweden for enabling us to generate experimental data, and to Staffan Brantingson at Statistics Sweden.

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1 Introduction

Are private contractors better at providing welfare services than are public providers? In many OECD countries welfare services (schools, health care, social assistance, and employment services) that traditionally have been provided by the public sector are increasingly being contracted out to private providers. The motivation for contracting out is that private entrepreneurs—having residual rights of the asset—have stronger incentives to invest in cost saving technologies and quality improving innovations, as discussed in the framework of Grossman and Hart (1986), Hart and Moore (1990) and Hart (1995). But private contractors may have too strong incentives to reduce costs, which can impair on the quality of the services provided (Shleifer 1998, Hart, Shleifer and Vishney 1997). Specifically, the scope for private provision is larger if opportunities to save costs by deteriorating non-contractible quality are limited; if innovations are a salient feature of the industry; and if there is a substantial reputation building and competition among producers that force them to uphold quality.

A contrasting line of arguments suggest that many public sector activities can be viewed as mission-oriented where employees are highly motivated and subscribing to the mission; it may thus be less costly to provide incentives in the public domain (see Besley and Ghatak 2005). All in all, the ex-ante case for contracting out thus differ across services and have to be assessed empirically ex-post.

In this paper we empirically assess the case for contacting out job placement. Even if private placement services are present in many countries¹, evidence of its effectiveness is still scarce. Winterhager (2006) and Bernard and Wolff (2008) find small negative, or no, general effects of contracting out placement services to private providers in Germany, whereas Winterhager, Heinze and Spermann (2006) find positive effects for individuals utilizing job placement vouchers at private placement agencies in Germany. Evaluations of the British Employment zones indicate positive effects from contracting out reintegration services for long-term unemployed in certain deprived areas (Hasluck, Elias and Green 2003). A potential worry in these observational studies is that there

¹ Australia and the Netherlands have gone as far as privatizing employment services, while private placement agencies provide services alongside the PES in Britain, Germany and Denmark (See for example Struyven and Steurs 2005; Bruttel 2005; Jahn and Ochel 2007; Bredgaard and Larsen 2007; Finn 2008; and Wright 2008).

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may be remaining differences across individuals at different providers of employment services.

We add to existing research by using experimental data to estimate if private providers are more efficient at placing unemployed; if private providers use different technologies; and if private providers generate a higher satisfaction among their clients.

By providing empirical evidence in a setting with strong incentive based contracts and a well defined service, where the extent of non-contractible quality is limited; where there is a substantial competition for contracts; and where performance may have consequences for future procurement, we also contribute to the more general discussion on when a government should provide a service in-house and when it should contract out provision (See for example Dewenter and Malatesta 2001, Duggan 2004 and Aizer, Currie and Moretti 2007, Lindqvist 2008 and Bloom et al 2006 for work on private versus public in other settings)

In 2007 the Swedish centre-right government gave the Public Employment Service (PES) instructions to more actively use private contractors as an alternative to in-house provision (Regeringen 2007). As a result the PES, in July 2007, launched a scheme where private contractors were commissioned to match *hard-to-place* unemployed to jobs on the regular labor market. Target groups were immigrants, adolescents and disabled, and the trial ran for 3 or 6 months.² The trial was setup as an experimental intervention where we randomly assigned almost 5,000 unemployed to either a private placement agency or to the PES. Specifically, those assigned to a private placement agency were given an option to switch from the PES to the private provider during the trial period. The contracted private placement agencies faced high-powered incentives as they were remunerated largely based on successful placements (60 percent of the full payment), as compared with no similar financial motivation for the PES. Even if the PES are benchmarked internally based on certain indicators (e.g. placement rates and customer satisfaction) their incentives are arguably weaker.

Our results indicate that unemployed at private placement agencies have a much closer interaction and are more content with their placement worker than are the

² The intervention was 6 months for disabled and foreign born individuals and 3 months for adolescents.

unemployed at PES. In particular, unemployed at private agencies receive more assistance in improving their job search technology. We do not find any overall difference in the probability of employment between private placement agencies and the PES. Our results show that private providers are better at providing employment services to immigrants; in fact, immigrants get significantly higher wage earnings up to 12 months after randomization. There are also indications that private contractors are worse at providing such services to adolescents.

The rest of the paper is organized as follows. In section two we describe the institutional setting of the trial with private placement services and give a brief background to employment services on the Swedish labor market. Section three describes the experimental design for allocating individuals to private providers and our estimation strategies. Section four present the results and section five concludes.

2 Institutional setting

2.1 Job placement in Sweden

The PES in Sweden plays a central role for Swedish labor market policy. In addition to matching jobseekers and employers and general labor market counseling to jobseekers, case workers also assigns unemployed to labor market programs and administer labor market related rehabilitation for those with reduced work capacity (e.g. disabled). The PES also plays an important role for the control function in the unemployment insurance by monitoring that claimants fulfill the requirements in the insurance of actively searching for jobs. This control function gives the PES some leverage towards the unemployed to be active in their job search, and to accept program assignments and job offers (Sibbmark 2008).

The role for private providers in implementing Swedish labor market policy has traditionally been limited, and restricted to labor market training where private providers have been used since the late 1980's (Olofsson and Wadensjö, 2009 p57). In fact, the PES had a monopoly on employment services on the Swedish labor market until 1992 when commercial temping and recruiting agencies were allowed to operate. These commercial agencies are still regulated and, for example, not allowed to charge jobseekers for matching services.

2.2 Trial with private placement agencies

In 2007, the centre-right government gave the PES instructions to more actively use private contractors to place unemployed in order to improve the matching between job seekers and employers (Regeringen 2007). The idea was that private placement agencies could utilize improved technologies and offer more personalized services. As a consequence of this instruction the PES launched a trial scheme with private placement agencies in July 2007. Within the trial, the covered target groups were randomly assigned either to a private placement agency, to fill contracted slots, or to the PES (in section 3.1 we describe the random assignment in more detail).

Commission

In Early 2007, the PES posted a call for tenders to procure placement services from private contractors. The procurement included contracts in three different regional labor markets (Malmö metropolitan area, Norrköping and Sundsvall/Timrå/Härnösand) for three specific target groups with difficulties to reintegrate into the labor market. The groups covered by the procurement were:

- 1 *Disabled with impaired working capacity.*
- 2 *Immigrants with an unemployment spell of at least six months (excluding individuals under age 25); and*
- 3 *Adolescents under 25 years with an unemployment spell of at least three months;*

The call for tender encompassed placement services for *matchable* individuals during a period of six months for disabled and immigrants, and three months for adolescents. The individuals covered by the procurement were matchable in that they had professions, educations and experience that were in demand on the labor market. That is, they were assessed not to be in need of any labor market program to find employment. Individuals in these groups were thus judged to be ready for the labor market, but suffering from difficulties in marketing their skill profiles.

In the procurement, commissioned private placement agencies were contracted to find the assigned job seekers a full-time employment—or employment to the assessed level of work capacity for disabled—on the regular labor market with a duration of at least three months. The private providers were essentially allowed to choose their own

technology to place the unemployed, but did not get paid for hiring them in-house and operating as a temping firm. Also, unemployed assigned to private placement agencies did not have access to the set of regular labor market programs during the contracted period.³ Contractors could not refuse anyone assigned to them, so there was no room for contractors to cherry pick easy cases.

The contracted providers also took over parts of the control function in the unemployment insurance from the PES; that is, if an individual did not fulfill the requirements in the insurance of being actively searching for jobs, the provider had to report this violation to the PES who would then initiate sanctions in the unemployment insurance. In this respect, the private providers had the same type of leverage towards the unemployed as the PES.

Assignment of contracted slots

Unemployed covered by the procurement were randomly allocated to the contracted private placement agencies in six waves: starting on July 10, 2007, and with the last wave in January 28, 2008. This means that the unemployed in the last wave were serviced by private placement agencies until July 2008, had they not yet transited into employment. As this trial scheme was cast in the second half 2007, with fairly low unemployment, it turned out to be difficult to fill the procured slots. Starting with the second wave of assignment (August 15, 2007) the required length of the unemployment spell for adolescents and foreign born was therefore reduced to 30 days.⁴

In total 669 unemployed individuals—within the three target groups—were allocated to a private placement agency. *Table 1* describes how individuals are allocated across the regional trial sites and across target groups. Adolescents are the largest group with

³ There are some exceptions to this: Disabled could make use of programs involving technical aids and personal assistance at the workplace if the private provider and the PES agreed on this (Stöd till hjälpmedel på arbetsplatsen, Stöd till personligt biträde på arbetsplatsen); All groups could get a certain wage subsidy if they were eligible to this program (Nystartsjobb); Individuals for whom the unemployment insurance was exhausted were transferred into a different benefit scheme (Jobb- och utvecklingsgarantin).

⁴ In the last wave of assignment on January 28, 2008, the required length of unemployment spell in order to be covered was raised to 50 days

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around 50 percent of the contracted slots, whereas 30 and 20 percent of the slots were assigned to immigrants and disabled, respectively.⁵

Table 1. Recruitment to private placement by site and target group

	Regional trial sites			Total
	Malmö	Norrköping	Sundsvall	
Adolescents	102	113	128	343
Immigrants		67	139	206
Disabled		49	71	120
Total	102	229	338	669

Competition for contracts

At each regional site two competing providers were procured for each contracted target group. The procurement procedure encompassed to stages. In the first stage, bidders had to document their proficiency (e.g. their experience from similar assignments and the competence of their personnel) and firms fulfilling certain quality criteria were invited to submit a full tender. In the second stage, the tender had to include a detailed description of working methods as well as a price. The quality of firms submitting the full tender was then rated according to the general quality of their working methods; their focus towards employers; time interacting with job seekers; and their degree of innovation. The bids were finally selected on both quality (60 percent) and the price (40 percent).

In the first round 38 firms submitted bids, of which eight to ten (depending on target group) were invited to submit a full tender. This two stage procedure was constructed in order to ensure a substantial degree of competition both along the dimensions of price and quality.

The providers awarded a contract did not have any prior experience in the exact services procured, since these were previously provided by the PES. Still, the awarded firms did have experience in job placement services, reintegration services, rehabilitation, and labor market training; for example provided at large lay-offs, firm closures or for individuals being on long-term sick leave as covered by—and financed

⁵ The reason for the large share of adolescents is that the procurement for disabled and immigrants in Malmö was appealed by a firm not receiving any contract, and subsequently withdrawn from the procurement by the PES.

through—collective agreements between trade unions and employers. Their experience of the groups covered by the procurement may, however, have been limited.

Incentives from contracts

The private placement agencies were remunerated largely based on successful placements. The contracts stated a price per unemployed which was decided through the procurement; this price differed both across target groups and across providers.⁶ The contractors were paid 40 percent of this sum when an unemployed got assigned to them; 30 percent was payable when the unemployed had signed an employment contract for full time employment with a duration of at least three months and had started the employment; and the last 30 percent was payable when the job seeker had stayed at his employment for three months.

While the private contractors faced strong economic incentives to find successful matches, the PES did not meet such financial incentives. Still, each branch of the PES is benchmarked based on a number of key indicators, including measures of customer satisfaction (both jobseekers and employers); measures of placement rates; indicators of wellbeing among personnel; and budgetary indicators. Although the PES is benchmarked internally their incentives are arguably weaker than those of the private providers.

A general worry when contracting out services is that the incentives faced by the agent—as induced by the contract—does not correspond to the intentions of the principal. However, in the present setting the contracted outcome—full-time employment on the regular labor market with a duration of at least three months—is well defined (observable and verifiable) and encapsulates many aspects of quality. Hence, even if there are additional aspects of the service such as the quality of the match, there is a limited scope for providers to reduce costs in a way that deteriorates the quality of the service.

The trial with private placement agencies in 2007 was the first episode of competition in employment services in Sweden. Even if this particular trial was in itself

⁶ For disabled/Adolescents/Immigrants the contracted price ranges between 30,000/12,200/20,280 SEK and 42,000/25,000/35,000 SEK.

limited in time and scope, it could be viewed, at the time, as a platform for larger scale privatization of employment services. In fact, the centre-right government expressed a political will when instructing the PES in 2007 to more actively use private entrepreneurs when providing employment services (Regeringen 2007).⁷ The trial scheme could therefore be seen as a storefront, and thereby giving the contractors additional incentives to provide high quality and efficient services in the hope of being awarded future contracts. Now, the trial may also have given the PES incentives to prove their efficiency, when facing the treat of a larger scale contracting out of their services.

2.3 What is the treatment?

Even if our evaluation will capture the net impact the receiving placement services from a private agency, as compared to the PES, it can be instructive to consider what components this net effect consists of.

First of all, there could be an effect from changing from a public to a private provider, since ownership in itself can provide motivation.⁸ A related issue is that private and public providers within this trial scheme have different incentives; where private placement agencies face substantial financial incentives. It can be conceptually difficult to separate the effects of ownership from those of monetary incentives induced by contracts. An additional incentive effect may also come from private providers hoping to be awarded additional contracts in future procurement.

The procedure with a two stage procurement process, ensuring competition both in quality and price, can also have an effect on outcomes. As noted by Winterhager (2006), if the first stage in a procurement is used to screen for a minimum level of quality, and tenders in the second stage are only selected on the lowest price, there is a considerable risk that firms awarded commission are those – that combine a low price with low

⁷ The instructions to the PES in 2008 (Regeringen 2008) expressed an even clearer political will by requiring the PES to use private providers as an *integral part* of its operations, and by setting up ambitious quantitative goals on the market penetration of private placement agencies.

⁸ In the framework of Grossman and Hart (1986), Hart and Moore (1990) and Hart (1995) this motivation comes from private providers having residual control rights of the asset.

quality.⁹ This was, indeed, not the case in the present setting as both quality and price were explicitly taken into account. But with a different procurement strategy different types of providers may have been awarded contracts.

We will also capture differences that are due to the fact the PES have a long experience in reintegrating these particular groups of unemployed, whereas the private contractors have less experience in traditional job placement services. It worth noting, however, that private contractors do have experience in providing similar services, albeit in other contexts and to different groups of unemployed.

All in all, the estimated treatment effects will capture all technology differences between public and private providers generated by these differences in ownership, incentives, procurement procedure and experience.

3 Empirical strategy

To assess the effects of private placement services we utilize a randomized experiment. In this section we describe the experiment, the data collection and the econometric strategy used to estimate the effectiveness of private provision.

3.1 Experimental design

The problem when assessing the effect of an intervention is that individuals who are being assigned to, or self-selecting into, a program may be different from those not affected by the intervention; e.g. by having a different capacity to benefit from the program or having different general prospects on the labor market. Importantly, they are typically different in dimensions that are unobservable to the researcher. The ideal way to identify the effects of an intervention is to utilize an experimental approach where the random assignment balances individual characteristics across those treated and non-treated.

The introduction of the trial scheme with private placement service gave us an opportunity to set up an experiment—together with the central administration of the PES—in order to evaluate the effectiveness of contracting out placement services. In the

⁹ Winterhager (2006) argues that in the German setting with private job placement services he is studying, providers with low quality and price were awarded contracts.

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experimental intervention, unemployed were randomized into an experimental group and a control group; those assigned to the experimental group were then given a choice to switch from the PES to a private placement agency.

Randomization procedure

The experiment was staged in six experimental waves: starting on July 10, 2007, and with the last wave in January 28, 2008. At each of the six experimental waves, the individuals in the stock of unemployed—within the target groups at each regional site—were either randomized to a specific private provider, or to the control group at the PES. The experimental variation generated is thus within each target group-region-wave cluster; in total we have 33 sub experiments to be used in the analysis.¹⁰

The number of individuals subjected to the randomization at each wave was determined by the available stock of unemployed within each target group (at each regional site) and the number of available slots at the private placement agencies.¹¹ Those unemployed who were subjected to randomization in a specific wave (either assigned as controls or to the experimental group) did not belong to the sampling frame in subsequent waves. In total, the experimental intervention include 4,804 individuals, of whom 2,410 were randomized to the experimental group.

Table 2 shows how individuals subjected to the intervention are distributed across regional sites, targets groups, as well as the relative size of the experimental waves. The largest regional trial site was Norrköping followed by Sundsvall/Timrå/Härnösand and Malmö, and the largest target group in the randomization was adolescents. The experiment was initiated in July 2007, but the second wave in August 2007 was largest, essentially sampling the whole stock of available unemployed—52 percent of the individuals included in the experiment. During the fall 2007 and early 2008 four additional randomizations took place to fill the remainder of the procured slots.

¹⁰ Note that the randomization did not cover all target groups at all regional sites for all six waves of the experiment.

¹¹ It should be noted that local caseworkers at the PES had no opportunity to manipulate who got assigned to the private providers. The randomization of individuals was carried out by the central administration of the PES at each wave of the experiment, and was based on random numbers generated from birth dates which were provided by us. After each randomization the central administration of the PES contacted the local branch and gave the list on whom to call for information meetings.

Table 2. Sample description

	Experimental group	Control group
Regional trial sites		
Malmö	0.251	0.249
Sundsvall	0.293	0.294
Norrköping	0.456	0.457
	<i>1.000</i>	<i>1.000</i>
Target group		
Adolescents	0.606	0.606
Immigrants	0.266	0.267
Disabled	0.128	0.127
	<i>1.000</i>	<i>1.000</i>
Experimental wave		
July 10, 2007	0.103	0.104
August 15, 2007	0.515	0.518
September 17, 2007	0.077	0.078
October 15, 2007	0.192	0.191
November 26, 2007	0.038	0.038
January 28, 2008	0.075	0.072
	<i>1.000</i>	<i>1.000</i>
Observations	2,410	2,394

After each randomization all individuals assigned to the experimental group were contacted by postal mail, where they were informed that they had an opportunity to switch to a private placement agency. In the letter they were also called to an information meeting at the PES (sometimes the meeting was located at the private placement agency). At this meeting the PES gave general information about the trial scheme; including rules and rights, and the private provider informed about their philosophy and working methods.

At the end of the meeting individuals had to decide on whether to take the opportunity to switch from the PES and instead receive job placement services from the private provider for a period of 6 months (3 months for adolescents). Participation in the trial was voluntary, but individuals declining the offer had to state a reason. Those who took the option could however not discontinue their participation during the 6 (3) month intervention, and private providers could not refuse anyone assigned to them.

Outcome of the randomization

To check if our random assignment was successful in balancing the experimental and control group, we compare them with respect to an array of observable and pre-determined background characteristics; see *Table 3*. We find that the experimental and control groups are similar with respect to gender, age, non-Nordic citizenship and the

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length of their unemployment spell; they are on average around 29 years with three and a half months of unemployment, and only a quarter are long-term unemployed. Long-term unemployment is defined as 3 months for those less than 25 year of age and as 6 months for those at, or over, 25. In both groups 39 percent carry benefits from the unemployment insurance, and they have similar job search profiles with respect to full-time work and geographical search areas. The groups are also similar with respect to educational attainment; around 62 percent have high school education, while only 12 to 13 percent carry a university degree. Turning to income the year before the intervention, we find that the share with a zero income is similar in the experimental and control groups, but the pre-study income is 4,700 SEK higher in the experimental group (To be revised....).

Even if the income difference is significant, the overall picture is that the experimental intervention has generated a good balance between the groups. In the analysis we will also control for all these background characteristics. It should be noted, however, that the level of pre-study income is not significant when included as a control in the analysis (see column 4 in *Table A2* in the Appendix) (To be revised...)

Table 3. Balance of the experiment (se till att karaktäristika matchar x-variablerna; ändra pre-study income). Skriv att andra kovariater (12) i skattning istf Pre-study income.

	Experimental group	Control group	Difference
Male	0.520	0.533	-0.013
Age	29.1	29.1	0.000
Unemployed (months)	3.5	3.6	-0.100
Long term unemployed	0.254	0.258	-0.004
Education compulsory	0.241	0.257	-0.016
Education upper sec	0.626	0.623	0.003
Education University	0.133	0.120	0.013
Pre-study income	47547	44539	3008
Pre-study income>0	0.701	0.697	0.004
Non-Nordic citizen	0.133	0.133	0.000
Unemployment insurance	0.393	0.386	0.007
Searching full time employment	0.961	0.960	0.001
Extended search area	0.380	0.370	0.010
Observations	2410	2394	

Compliance

The compliance in the experiment turned out to be relatively low; only 28 of those randomized to the experimental group chose to switch from the PES to a private

placement agency. As seen in *Table 4* the low compliance rate is the result of a selection process in two subsequent stages; only half of the unemployed who were called to the information meeting actually attended the meeting (51 percent), and amongst those present only about a half took the opportunity to switch to a private provider (54 percent).

The low attendance at the meetings may in part be due to the PES's unemployment register not being fully updated for all individuals who have recently left unemployment,¹² and in part due to a relatively low share of unemployed being eligible for benefits from the unemployment insurance in the target groups. Individuals on unemployment insurance could get sanctions in their unemployment benefits for not attending the meeting, but as only 39 percent of the individuals in the experiment received unemployment benefits this leverage was only partially binding. This is particularly true for adolescents—where only 23 percent are covered by the unemployment insurance—who had the lowest attendance at the information meeting.

Of individuals attending the meeting, adolescents were most inclined to participate (58 percent) whereas immigrants were most likely to decline the opportunity. In the control group, on the other hand, compliance was 100 percent.

Table 4. Compliance in the experimental group

	Experimental group	Attended meeting	Participated
Adolescents	1460	587 (0.40)	343 (0.39) FEL
Immigrants	642	424 (0.66)	206 (0.32)
Disabled	308	223 (0.72)	120 (0.23) FEL
Total	2410	1234 (0.51)	669 (0.28)

The compliers did not fully represent the population of the target groups. Older individuals and individuals with a longer unemployment spell are more likely to take the opportunity to switch from the PES to a private job placement agency, and so are also women. All in all, we do not find any strong selection on observable characteristics into treatment among those randomized into the experimental group (See the first stage regressions in *Table A2* column 2 in the Appendix).

¹² For a discussion on misclassification in Swedish unemployment registers see Bring and Carling (2000) and Benmarker et al. (2000).

3.2 Data

Our analysis is based on a combination of four different sources of data: administrative data from the PES, billing data from the private providers; earnings data from the tax authorities and data from two surveys.

For each wave of the experiment we first collect information from the PES's unemployment register, at the time of the randomization, for all individuals subjected to the randomization. This includes information on region; target group; whether the individual is a control or belongs to the experimental group; as well as the background characteristics described in *Table 3* (above). In addition, we have collected information on participation in the trial scheme—i.e. whether the individual switched to a private provider—directly from the billing of the assignment fee. We expect this billing data to be of high quality and exhaustive as the private providers have strong incentives to make sure they receive their payments.

We use two different data sources for outcomes; both survey information and also wage income data that employers are mandated to report to the tax authorities.¹³ Specifically, employers have to report the annual wage sum paid to each employee, and the months for which the wage is paid for income tax declaration purposes. For every individual in the experiment we have thus been able to collect yearly wage earnings 2006 to 2008 paid by each employer, and in addition the first and last month every year that the employer pays wage to the individual. Using this information we calculate an average monthly wage for each individual, as well as an employment indicator month-by-month.¹⁴

We have also administered two surveys to the individuals included in the experiment.¹⁵ The first survey was collected either one or three months after individuals

¹³ We do not use information on employment status from the PES's unemployment register. The reason is that we do not trust the unemployment register for outcomes in the (post) intervention period since private providers have much stronger incentives to report employment (than have the PES).

¹⁴ The employment indicator is defined as having a monthly wage earning larger than 5,000 SEK. We have used a cut-off larger than zero in order to reduce noise caused by for example delayed holiday payments or over time compensation. REVIDERA!!!!

¹⁵ For the first survey there was an administrative error making it impossible for us to link some survey responses to individuals in the experiment; the same survey identification number was used twice, to an individual in the experimental group and to a control. Fortunately the error was random, but it effectively reduced the response rate to 60 percent.

were subjected to randomization.¹⁶ The first survey is mainly focused at intermediate outcomes capturing differences in working methods, also collects information on short run employment outcomes.

The second survey was administered three months after the longest *potential* treatment at a private placement agency; which means nine (six) months after randomization for immigrants and disabled (adolescents). This survey collects information on employment outcomes and general life satisfaction.

The response rate is over 60 percent in the first survey and over 70 percent in the second, with the same response rate in both the experimental and the control groups. Similarly we do not find any systematic differences in observable characteristics between responders in the experimental and the control groups. Descriptive statistics of all outcome variables are available in *Table A1* in the Appendix

3.3 Estimation method

In the experiment, compliance to treatment was voluntary for individuals who were randomized into the experimental group; that is, some unemployed individuals were randomly given an offer to switch from the PES to a private provider. Any differences in outcomes between the experimental group and the control group therefore reflect the intention-to-treat effect. (*Table A1* in the Appendix displays the unconditional intention-to-treat effects.)

To estimate the treatment effects of receiving job placement services from a private contractor (rather than the intention-to-treat effect of being offered such services) we use the random assignment as an instrument for going to a private provider. In essence, we scale the reduced form estimates of being offered private placement services with the share of unemployed who accepted the offer (compliers). Our identifying assumptions are that the random assignment of offers is really random and that there is no direct effect of being offered treatment on the outcome, other than through its effect on the probability to take the treatment; i.e. that the assignment is ignorable and monotonic (see for example Angrist, Imbens and Rubin 1996).

¹⁶ Adolescents are surveyed after one month, whereas half of the immigrants and disabled were randomly surveyed after one and three months respectively. To increase power in our analysis we have disregarded the timing of the first survey and use the information as composite measures of the first part of the intervention period.

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That the treatment offers were really randomly assigned is indicated in Table 3 showing the balance of the experiment¹⁷, whereas the strength of the instrument is indicated by the fact that the compliance is 28 percent in the experimental group and 100 percent in the control group (See also the first stage regressions in *Table A2* column 2 in the Appendix). Since no-one in the control group received treatment, the monotonicity assumption, i.e. that no-one counter act their assignment, is fulfilled.

We therefore estimate the following IV-model capturing the treatment effect of private placement services, for unemployed choosing to switch a private job placement agency when given the opportunity,

$$Y_{ij} = \alpha + \delta Private_{ij} + \mathbf{X}_{ij}\boldsymbol{\beta} + \lambda_j + \varepsilon_{ij}$$
$$Private_{ij} = \alpha + \phi Z_{ij} + \mathbf{X}_{ij}\boldsymbol{\beta} + \lambda_j + e_{ij},$$

where Y_{ij} is the outcome of individual i in sub-experiment j in the outcome equation. $Private_{ij}$ is the indicator of being treated by private placement agencies, which is endogenous due to non-compliance. In order to ensure balance among treated and non-treated we instrument treatment status with the initial random assignment of the option of getting treatment, Z_{ij} . To further ensure balance and to reduce residual variance we control for a vector, \mathbf{X}_{ij} , of background characteristics described in Table 3.¹⁸ For similar reasons we also utilize the within sub-experiment variation by including fixed effects, λ_j , for each *region-target group-wave* cluster j .¹⁹ Hence, δ captures the effect of

¹⁷ A possible concern is that sanctions in the unemployment insurance for those not attending the information meeting could have had direct effects on the outcome. First, with only 39 percent of the unemployed in the target groups having qualified for unemployment insurance, the threat of sanctions were only partially binding. Second, only 2-3 percent of the claimants received sanctions in the unemployment insurance during the 2007-2008 period (Riksrevisionen, 2009). Therefore we do not believe potential sanctions to have had any direct effects on unemployment.

¹⁸ Column 4 of *Table A2* in the Appendix display employment effects of private placement services 6/9 months after randomization with, and without, a vector, \mathbf{X}_{ij} , of background characteristics. The estimated effect becomes slightly larger in size, but is essentially the same, when including the covariates.

¹⁹ In the econometric specifications we use 39 fixed effects (To be revised..), rather than 33 as in the number of sub-experiments. The reason for this is that from the second wave of the experiment the requirement on the length of the unemployment spell to be included in the sampling frame was reduced. This had the consequence that some individuals who were not in the sampling frame in the first wave of the randomization were randomized in the later waves. In the randomization procedure used by PES everyone with random numbers below a certain threshold were allocated to the experimental group, and everyone above to the control group, but with problems in filling the contracted slots this threshold number was increased. From the second wave of the experiment, individuals subject to randomization consisted of both newly unemployed individuals with short unemployment spells flowing into the target groups and individuals with longer unemployment spells who were unemployed at an earlier wave of the randomization but not within the sampling frame at that point. As a consequence of the changed threshold number in the randomization, individuals with longer unemployment spells were overrepresented in the experimental group. In

private placement services for individuals choosing to participate when being randomly assigned an offer.

Benefits of experimental variation

The benefits of using an experimental approach—rather than relying on selection on observables—can be appreciated from *Table A2* in the Appendix. Here we assess the effects of private placement services on having employment 6/9 months after randomization (3 months after the end of the intervention). Looking first at the OLS estimates in column 1 (lower panel), where we compare unemployed who are under treatment at private placement agencies with all those at the PES—the control group plus the non-compliers—and controlling for a rich set of covariates, \mathbf{X}_{ij} , we find a positive and significant estimate. This would indicate that being under treatment at a private job placement agency increases the probability of having a job 6/9 months after randomization with 4.5 percentage points. Comparing the lower panel estimates where we control for \mathbf{X}_{ij} with the upper panel without covariates, we see that the point estimate is slightly reduced but stay essentially the same, thus indicating that selection on observables to private providers would not be a problem. Still we see from the first-stage estimates of the IV (column 2 of *Table A2*) that individuals selecting to go to private providers are in fact not fully representative. (To be revised, new: Y & A2)

When we instead exploit the experimental variation to identify the effect of private employment services, we find negative and insignificant effects (See the IV-estimates in column 4). In our setting with full compliance in the control group, both the IV-estimate and the OLS-estimate should be equivalent to the average effect of treatment on the treated (Angrist and Imbens 1992).] Hence, the benefits of our identification strategy are obvious; had we tried to identify effects by conditioning on observables (e.g. a matching approach) instead of running an experiment, we would have risked drawing erroneous conclusions about the effectiveness of private employment services.

order to handle this problem we have defined separate experiments (clusters) for individuals being “stock” and “flow” sampled from the second wave of the randomization and onwards.

4 Results

The motivation for contracting out job placement services to private providers is that private providers may prove more effective in matching unemployed to vacancies. These benefits may emanate from private contractors having an opportunity to be more flexible in methods; not having to adhere to regularities in the routines of the PES, or from them having different skill sets stemming from other types of experiences. In addition, they may also have stronger innate incentives to innovate the market, being claimants of residual profits.

In this section we will first analyze differences in the technology of delivering jobs to unemployed. That is, using survey information—collected one or three months after randomization—we describe differences between private job placement agencies and the PES in how the unemployed spend their time, how they search for jobs and how they interact with their case worker.

We will also present estimates on the effects of private placement services on various labour market outcomes and on personal well-being.

4.1 Differences in working methods

The general picture, when assessing differences in delivering placement services, is that private job placement agencies appear to use a more labor intense technology. Unemployed at private providers spend more time with their case officer where they get more information on vacancies and receive more help in improving their job search strategies. They are also more satisfied with their case worker. In addition, they are more frequent at attending job fairs. Adolescents, in particular, appear to have a higher job search intensity when being at private providers—initiating more contacts with employers, applying for more jobs and attending more job interviews—while adolescents at the PES spend more time in job training at employers.

In *Figure 1* we describe the number hours spent in different activities during a typical week as a job seeker at either a private job placement agency or the PES. For the PES the figure displays values for the control group. For private placement services the figure displays IV-estimates of the effects of going to a private provider added to the

values for the control group. For all individuals surveyed the number of hours per week adds up to 40, representing activities during a normal (8 hour) working day.

In the first pair of bars we see that unemployed at the private placement agencies spent, on average, 1 hour and 40 minutes per week with their case worker, as compared to only about 20 minutes for those at the PES; the difference around 1 hour and 20 minutes being statistically significant. This implies that private placement agencies are substantially more labor intense in delivering placement services.

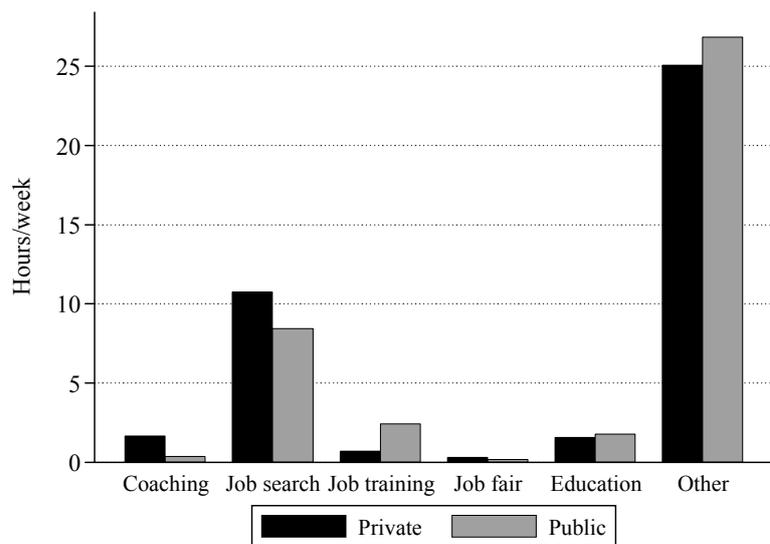


Figure 1. Hours spent in different activities last week (ännu ej reviderat -skattning)

Note: For the public employment services the figure displays values for the control group. For private placement services the figure displays IV-estimates of the effects of going to a private provider added to the values for the control group. The figure is based on survey information on the number hours spent in different activities last week. (Note är möjligen överflödig)

Figure 1 also indicates that unemployed at private providers spend more time searching for job every week. This includes getting instructions on how to search a job effectively (e.g. writing a CV and preparing for interviews). The unemployed at private providers spend almost 11 hours searching jobs, while those at the PES spend around eight and a half hours a week searching for jobs. Another striking difference is that the PES uses job training at employers and internships to a larger extent. While these differences in weekly hours spent on searching jobs, and in job training, are suggestive they do not reach statistical significance. What is notable in *Figure 1* though is that all

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unemployed—both at private providers and at the PES—devote most of their time to something else; i.e. activities not related to finding a job.

As seen in *Figure 1* unemployed at private providers spent substantially more time with their case officer every week. The results in *Table 5* corroborate this finding. When we ask if the unemployed met their case worker last week, we find that those at a private provider had a 48 percentage point higher probability of meeting the case worker. This high interaction with the case worker is present for all three target groups included in the experiment. As only 35 percent of the unemployed at the PES on average meets with their case worker in a given week; this difference amount to an increase of 140 percent. An important issue for the question of the efficiency of private job placement agencies is the content and quality of these meetings. Columns 2 and 3 show that such meetings allegedly helps unemployed at private providers to improve their job search strategy and provide them with information with (on?) available vacancies. This is particularly true for immigrants; a group with potentially weaker connection to norms and networks on the Swedish labor market.

Table 5. Effects of private placement services on contacts with case worker (revised)

In contact with case worker last week	Case worker helped to improve job search	Case worker provided information of vacancies	Sufficient help from case workers to find a job
Panel A: All			
0.482*** (0.0586)	0.335*** (0.0373)	0.274*** (0.0443)	0.341*** (0.0613)
Panel B: Disabled			
0.397*** (0.150)	0.303*** (0.0881)	0.196** (0.0977)	0.541*** (0.167)
Panel C: Immigrants			
0.486*** (0.0948)	0.504*** (0.0689)	0.421*** (0.0792)	0.322*** (0.0987)
Panel D: Adolescents			
0.490*** (0.0884)	0.222*** (0.0532)	0.184*** (0.0661)	0.251*** (0.0910)

Note: The table shows the IV-estimates of the effect of private job placement services (as compared to the public employment services). Each cell show the effect from a separate regression, with different outcomes across columns and different (sub)samples across lines. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*. Pre-study income was controlled for with using 12 variables measuring monthly income one year prior to randomization. Robust standard errors are in parentheses, */**/** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence.

Unemployed at private providers are also much more satisfied with the service received, as seen in column 4 of *Table 5*. On average, the share that says that they have

received sufficient help to find a job is 0.34 higher; which amounts to an increase of 83 percent. It is in particular disabled, a group that is possibly furthest away from the labor market, who state that they have receive sufficient help. ,

Figure 1 suggests that unemployed at private job placement agencies spend less time, during a normal week, on job training or internships than the unemployed at the PES. In *Table 6* this is supported by survey questions asking on job search activities during the last month. However, the lower probability of participating in job training for those at the private providers emanates entirely from adolescents. Immigrants and disabled are not less likely to attend job training. One potential explanation for this result is that the PES have positive experience from such activities for adolescents, and therefore have specific programs, or other types of working methods, for adolescents geared at providing job training.

Rather than using job training, as a way for unemployed to interact with employers and demonstrate their skills, private providers use activities like job fairs and job markets. *Table 6* shows that immigrants and disabled at private job placement agencies are much more likely to attend such events. It is notable, though, that adolescents at private providers did not visit job fairs more often than those at the PES.

Consistent with *Figure 1*, we also find that unemployed at private providers were more frequent in participating in various types of job search training. For example, this can be workshops where the unemployed receives instructions on writing application letters or are subjected to mock job interviews. The average difference of 35 percentage points is large, (43 percent), and effects are present for all subgroups.

Table 6. Effects of private placement services on job search activities the last 30 days

Job search training	Job training	Job fair
	Panel A: All	
0.347*** (0.0545)	-0.0467 (0.0378)	0.103** (0.0469)
	Panel B: Disabled	
0.557*** (0.132)	0.0440 (0.0912)	0.171 (0.108)
	Panel C: Immigrants	
0.461*** (0.0907)	0.0488 (0.0554)	0.228*** (0.0858)
	Panel D: Adolescents	
0.221*** (0.0826)	-0.128** (0.0607)	-0.0268 (0.0678)

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Note: The table shows the IV-estimates of the effect of private job placement services (as compared to the public employment services). Each cell show the effect from a separate regression, with different outcomes across columns and different (sub)samples across lines. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*. Robust standard errors are in parentheses, */**/** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence.

While unemployed at private providers have a more frequent interaction with their case worker—where the emphasis is on improving job search techniques and creating contact surfaces with employers—we also look at whether this resulted in a higher job search intensity. When we ask about job search intensity during the previous month, *Table 7*, we find that being exposed to the working methods of a (?) private provider may cause adolescents to become more motivated in their job search. In particular, adolescents at private job placement agencies initiated more contacts with prospective employers, applied for more jobs, and were called to more interviews, than had they been treated at the PES. This effect is however only present for adolescents; We find no strong evidence that immigrants or the disabled at private providers applied for more jobs or initiated more contacts with prospective employer, as effects are not statistically significant. The point estimates, though, suggests that also immigrants and disabled may have been more active in initiating contacts with employers.

Table 7. Effects of private placement services on job search intensity the last 30 days

Number of self initiated contacts with employers	Number of jobs applied	Number of unannounced job applied	Number of jobs interviews
Panel A: All			
1.576** (0.690)	1.999 (1.303)	0.753 (0.704)	0.534*** (0.176)
Panel B: Disabled			
1.493 (1.868)	0.931 (3.208)	1.367 (1.476)	0.639 (0.463)
Panel C: Immigrants			
0.686 (0.899)	0.158 (2.172)	-0.295 (1.186)	-0.0645 (0.349)
Panel D: Adolescents			
2.220* (1.204)	4.258** (2.079)	1.402 (1.118)	0.983*** (0.243)

Note: The table shows the IV-estimates of the effect of private job placement services (as compared to the public employment services). Each cell show the effect from a separate regression, with different outcomes across columns and different (sub)samples across lines. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*. Robust standard errors are in parentheses, */**/** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence.

4.2 Effects on labour market outcomes

Private job placement agencies rely in part on different working methods than the PES; as private providers have a stronger emphasis on improving job search technology, trying motivating job seekers to search more intensively. The crucial question is whether private providers also improve the labor market prospects of unemployed relative to the PES. There are essentially two margins that can be affected. Private providers may influence both the chances of finding a job—reducing the time to employment—and how well the job fits the person’s skill profile, i.e. the quality of the match. Since the private contractors are remunerated largely based on successful placements, a potential worry is that they may save effort by placing unemployed at jobs where the matching quality is lower.

We will assess effects both on the prospects of finding a job and on three proxy measures of matching quality; hours worked; job satisfaction, and monthly wage earnings. Our results do not indicate any overall effect of private employment services on the prospects of finding a job. There are however important heterogeneities across the target groups; in particular, immigrants at private providers worked more during the first six first months after randomization. When using wage earnings as a broader measure of labor market prospects—capturing employment as well as hours worked and matching quality—we find that immigrants at private providers have higher earnings up to 12 months after randomization. We also find some support for private providers having a negative effect on earnings and hours worked for adolescents.

Employment

In *Table 8* we present effects of receiving job placement services from private providers on employment. As an indicator of employment status we use the incidence of having earnings. Specifically, we use taxation data from employers with monthly information on whether the individual has received income. We estimate the employment effect at different points in time after randomization. To be more exact, we measure the effect as the number of months with earnings exceeding SEK 5,000 (To be revised..) within 3/6/9/12 months after randomization. The first column shows the effect of private job placement on employment prospects 1 to 3 months after randomization; on average unemployed at private provides have worked 0.075 months more during the three first

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months after randomization, than had they been at the PES. This effect is not significant, and neither are the effects for any of the subgroups. (To be revised..) Three months may be too early to expect any employment effect.

The second column displays employment effects during the first six months after randomization, which was the longest period an unemployed under treatment could spend at a private provider (3 months for adolescents). The results show that the average difference between public and private employment services is small; unemployed at a private job placement agency had on average worked 0.14 months more during the six first months, but this effect is not statistically significant. However, these small average effects hide interesting differences across subgroups. While the point estimate for adolescents is negative, the effect is in fact positive and marginally significant for immigrants. Immigrants at private providers worked 0.7 months more the first half year after randomization. This pattern is similar also nine months after randomization. There is still an indication that adolescents at private providers are doing worse, and the effect for immigrants is still large and positive but does no longer reach statistical significance ($p=0.103$). In the longer run, up to a year after randomization, any differences in employment effects across public and private employment services tend to decline in size. (To be revised..)

Table 8. Employment effects of private placement services summed over different numbers of months after randomization (cutoff $D=9700$, $I=9400$, $A=5700$)

Employment 1-3 months	Employment 1-6 months	Employment 1-9 months	Employment 1-12 months
Panel A: All			
0.0481 (0.104)	0.142 (0.202)	0.155 (0.308)	0.108 (0.428)
Panel B: Disabled			
0.132 (0.170)	0.317 (0.349)	0.418 (0.556)	0.268 (0.773)
Panel C: Immigrants			
0.419*** (0.146)	0.963*** (0.299)	1.241*** (0.470)	1.312** (0.653)
Panel D: Adolescents			
-0.209 (0.171)	-0.418 (0.328)	-0.591 (0.493)	-0.862 (0.710)

Note: The table shows the IV-estimates of the effect of private job placement services (as compared to the public employment services). The outcome variable is the number of months with a wage earning above 9,700 (9,400; 5700) SEK for the disabled (Immigrants; Adolescents). These numbers correspond to the median monthly income, excluding zeros, one year after randomization. Each cell show the effect from a separate regression, with different outcomes across columns and different (sub)samples across lines. All models include fixed effects for each sub experiment and controls for background characteristics

described in *Table 3*. Robust standard errors are in parentheses, */**/** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence.

Alternativ. Employment effects of private placement services summed over different numbers of months after randomization (cutoff=5000 SEK)

Employment 1-3 months	Employment 1-6 months	Employment 1-9 months	Employment 1-12 months
Panel A: All			
0.0896 (0.112)	0.165 (0.214)	0.148 (0.322)	0.0938 (0.445)
Panel B: Disabled			
0.307 (0.216)	0.547 (0.420)	0.711 (0.639)	0.566 (0.861)
Panel C: Immigrants			
0.369** (0.167)	0.811** (0.332)	1.047** (0.511)	1.067 (0.699)
Panel D: Adolescents			
-0.160 (0.175)	-0.356 (0.335)	-0.579 (0.500)	-0.844 (0.719)

In order to be more elaborate on how employment effects evolve over time, and whether there are any important differences in patterns across the target groups, we presents also detailed results graphically. In *Figure 2* we display the employment effect—month-by-month—of obtaining job placement services at a private provider instead of the PES. Effects are displayed for the period 1 month before randomization until 13 months after the randomization, where month 0 represents the month of the randomization. (Detta ska revideras p.g.a. att vi numera kontrollerar för inkomst fram till en månad före randomisering) The solid line represents the probability of finding a job in a specific month, and the dotted lines indicate the 95percent confidence interval. Panel A displays the overall employment effects of placement services at private providers; we see here that effects are small and insignificant throughout the whole follow-up period 13 months from randomization.

Panels B-D show the effects for the different target groups separately. For disabled we do not find any employment effect from being at a private provider. As seen in Panel B, one should, however, be careful in interpreting results for disabled as point estimates are positive and substantial in size but fairly imprecisely estimated.

For immigrants there appears to be a positive employment effect from being at a private provider peaking around six months after the randomization; in fact, the estimate at month 6 after randomization is significant. Over a longer follow-up period the size of

the estimated effects declines. The pattern with potentially negative employment effects for adolescents is visible in Panel D, where point estimates show a consistent negative pattern between the second and the eighth month after randomization. These effects never reach statistical significance though.

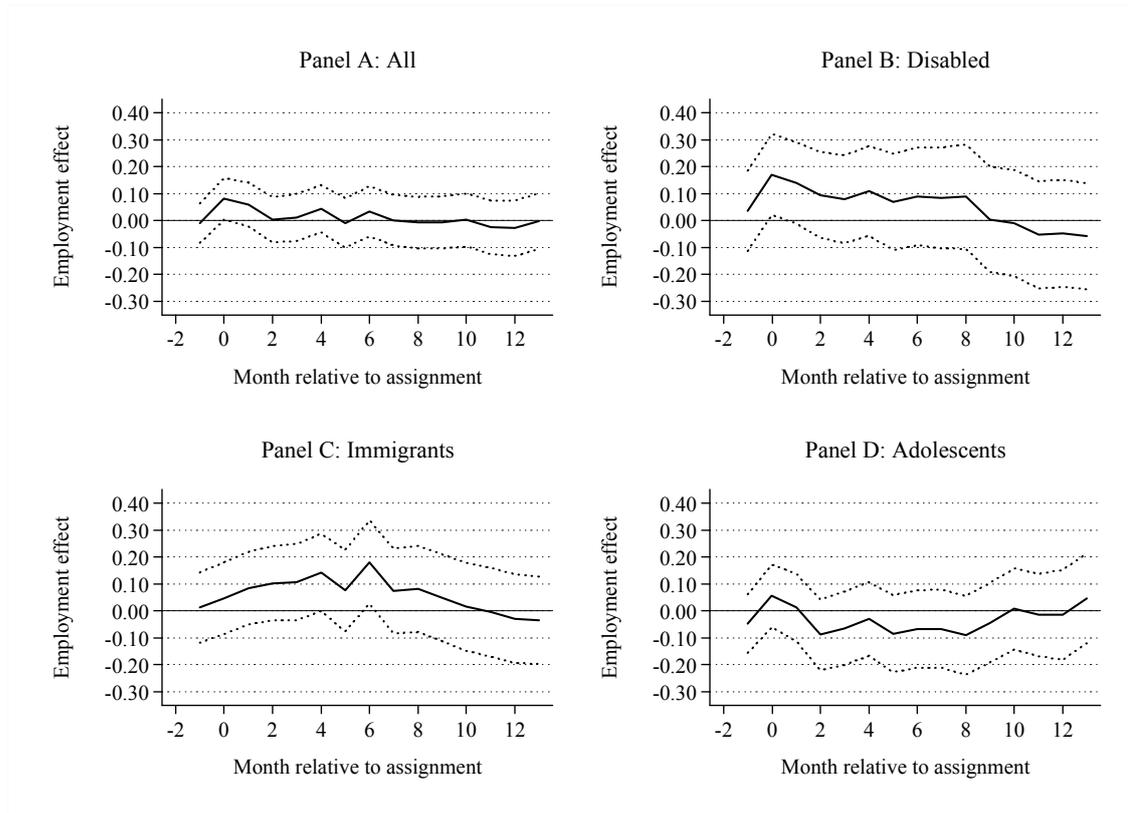


Figure 2. Employment effects of private placement services at different months after randomization (to be revised due to new x-variables and possibly new Y-variable)

Note: The solid line shows IV-estimates of the month-by-month effect of private job placement services (as compared to the public employment services), while the dotted line show the 95 percent confidence intervals using robust standard errors. The outcome variable is positive wage earnings. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*.

In the analyses above employment status is defined as having wage earnings exceeding 5,000 SEK. (Maybe revise) As a corroborating support for this information, we have also estimated employment effects based on survey information; see *Table A4* in the Appendix. In the two surveys, respondents were asked about their employment status at 1/3 months and at 6/9 months after randomization. Although these estimates are insignificant, the point estimates are in line with the results in *Figure 2*.

Wage earnings, hours worked and job satisfaction

It is natural to evaluate the relative effectiveness of public and private employment services based on “employment” capturing the extensive margin of labour market participation; especially since private providers are remunerated based on successful placements. But to focus on employment as an outcome may also be restrictive in the sense that there are many other aspects of the matching between employers and job seekers that we think are important. We have therefore estimated effects on wage earnings, since we believe that earnings is a broader composite measure of labour market participation capturing also the quality of the match; both hours worked and whether the individual is at a job where his particular set of skills is more productive, thus generating a higher wage.

In *figure 3* we display the earnings effect—month-by-month—of being under treatment from a private job placement agency rather than the PES. The solid lines represent the earnings in SEK gained/lost a specific month caused by being at a private provider. Similar to the employment effect, Panel A shows that the overall earnings effect from being at a private provider is negligible; both small in size and insignificant up to 13 months after randomization. This is in line with the results on the employment effect.

For disabled (Panel B) we find positive and quite substantial estimates on wage earnings throughout the intervention period, but none of the effects are statistically significant. It is therefore difficult to make any conclusion on the effectiveness of private employment services for disabled.

The earnings effects for immigrants show a positive and increasing pattern throughout the intervention period; the pattern is similar to the employment effects, but is here even more pronounced. Panel C shows significant effects on wage earnings, for immigrants at private job placement agencies, already from the third month after randomization, with a peak at the end of the intervention period six months after randomization. Over time, however, the positive effect on wage earnings levels out and declines. (Maybe revise...)

Also for adolescents the wage earnings effect resembles the employment effect. In Panel D we see that the effects on earnings become strikingly negative five through

twelve months after randomization. It should be noted that for adolescents the intervention with private job placement agencies had only a duration of three months. Hence, while at the private provider there are no differences, but after the intervention has ended there may be detrimental effects for adolescents from having been at a private provider; in fact, the effect is significant eight months after randomization. (revise?)

It is notable that any effect on immigrants and adolescents declines over time; a year after the randomization any effect appears to have died off.

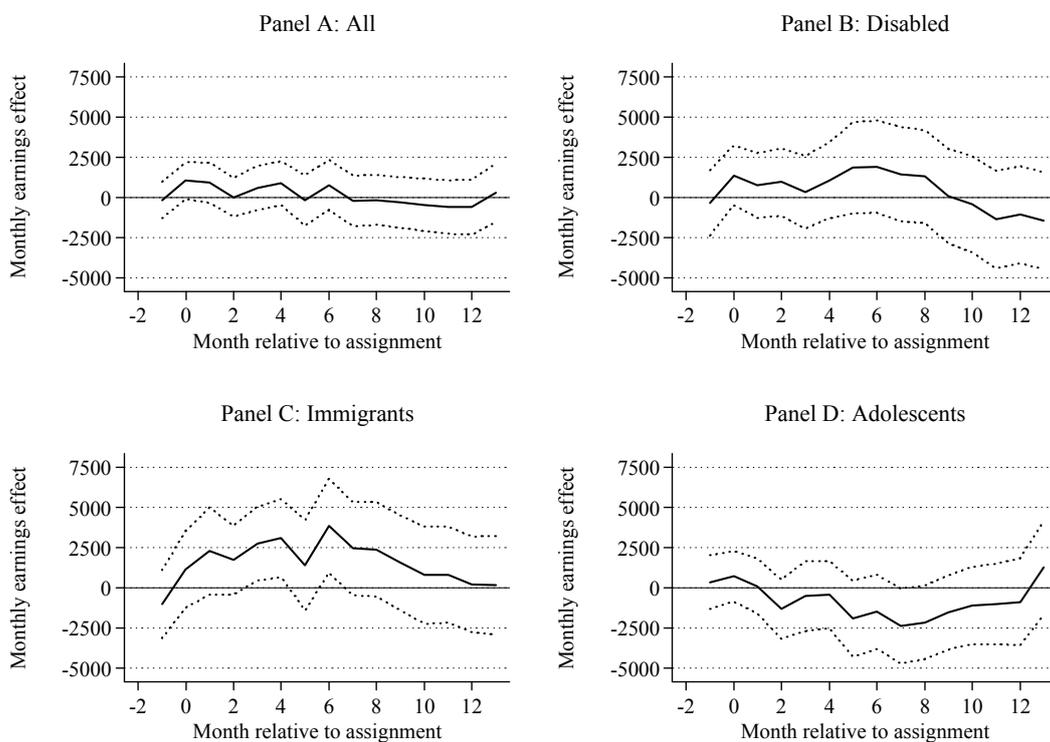


Figure 3. Earning effects of private placement services at different months after randomization

Note: The solid line shows IV-estimates of the month-by-month effect of private job placement services (as compared to the public employment services), while the dotted line show the 95 percent confidence intervals using robust standard errors. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*.

While *figure 3* shows the wage earning effects in specific months as well as displaying patterns over time, we would also like to measure the overall effect on earnings. We have therefore estimated the sum of wage earnings at different months after randomization.

In *Table 9* columns 1 to 4 we estimate the effects of private placement service on aggregated wage earnings 3/6/9/12 months after randomization, respectively. In panel A (columns 1 to 4) we see small and insignificant overall effects on aggregated earnings for all time spans. The effect on aggregated earnings for disabled is much larger—6,000 SEK after 6 months as well as after 12 months—but since the sample size is small (only 13 percent of the experiment) estimates are statistically insignificant.

For immigrants (Panel C), on the other hand, we find positive and statistically significant effects on aggregated earnings for all time spans. After three months the benefit of being at a private provider, instead of the PES, is 7,900 SEK and after another three months the aggregated earnings effect has increased to 17,300 SEK. After twelve months, immigrants at a private job placement agency has gained 28,000 SEK compared their peers at the PES. Since the average aggregated wage earnings over the same time span at the PES is 58,700 SEK, this amount to an 48 percent increase in earnings.

The aggregated earnings effect for adolescents show a different pattern and are negative over all time spans from the randomization, but insignificant.

The results show that for immigrants there is a positive effect on earnings from being at a private provider, in particular in the later phase of the intervention period (3 to 6 months after randomization). In fact, as long as twelve months after randomization there is still a positive effect on aggregated earnings. For adolescents, results instead indicate a negative effect on wage earnings from being at a private provider. The estimates displayed in *figure 3* are negative for all month and is significant 7 months after randomization. (May need to be slightly revised after re-estimation of F3)

In addition to wage earnings, we try to capture other aspects of matching quality by using survey questions on the number of hours worked per week 6/9 months after the randomization. *Table 9* column 5 shows that there is a small overall reduction in the number of weekly hours worked when being at a private provider, but this reduction is not statistically significant. What is interesting though is that the estimated effect for immigrants is substantial while still being insignificant; the point estimate indicates that immigrants at a private provider works 4.2 hours more per week. This strengthens the hypothesis that the increase in aggregated earnings may, at least partially, be due to

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more hours worked. For adolescents we find a negative and significant effect, indicating that adolescents at private providers work 5 hours less per week. This also indicates that the negative earnings pattern may partly be due to effects on the intensive margin.

In the last column of *Table 9* we use survey information 6/9 months after randomization to assess a more qualitative measure of matching quality; namely job satisfaction. We do not find any effect on job satisfaction from having been at a private job placement agency; neither an overall effect nor effects for the different subgroups.

Table 9. Effects of private placement services on aggregated earning at different months after randomization, hours worked and job satisfaction (reviderad)

Aggregated earnings 3 months after randomization	Aggregated earnings 6 months after randomization	Aggregated earnings 9 months after randomization	Aggregated earnings 12 months after randomization	Hours worked 6/9 months after randomization	Job satisfaction 6/9 months after randomization
Panel A: All					
1926 (1646)	3796 (3393)	3408 (5233)	2456 (7319)	-2.186 (1.884)	-0.0414 (0.0545)
Panel B: Disabled					
1842 (2766)	6284 (6012)	9163 (9559)	6210 (13122)	-2.806 (3.659)	-0.137 (0.109)
Panel C: Immigrants					
7883*** (2774)	17287*** (5815)	24824*** (9193)	27993** (12611)	4.576 (3.179)	0.146 (0.0897)
Panel D: Adolescents					
-1921 (2499)	-5864 (5143)	-12028 (7834)	-18493 (11367)	-5.432* (2.941)	-0.111 (0.0854)

Note: The table shows the IV-estimates of the effect of private job placement services (as compared to the public employment services). Each cell show the effect from a separate regression, with different outcomes across columns and different (sub)samples across lines. The outcome variable is indicated in the column heading. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*. Robust standard errors are in parentheses, */**/** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence.

Wellbeing

From the individuals perspective we are not only interested in whether an individual has a job; how many hours he works, or if he earns sufficient. These aspects of the working life can more be seen as intermediate measures. On a more general level, what we are interested in is whether an intervention affects a person’s wellbeing. From this perspective we can view a person’s self-perceived wellbeing as a broader measure of matching quality.

For this purpose we have asked a number of questions on individuals’ self-perceived life situation 6/9 months after randomization; that is, they were surveyed three months after the end of the intervention period. *Table 10* shows the effects on the (self-perceived) life situation from being at a private provider instead of the PES, where positive effects indicate a better life situation. We do not find any overall effects on the life quality measures. However, we do find that disabled who were at private providers are feeling less active; feeling they have had a less interesting day, three months after the end of the trial period at private providers. It is difficult to know how to understand these effects, but one potential interpretation is that these individuals put great hope in that the private providers would be able to help them out of unemployment. When this did not materialize they were disappointed and despaired.

Table 10. Effects of private placement services on individuals’ life situation (reviderat)

Feeling happy and in a good mood (1-6)	Feeling active and full of energy (1-6)	My day has been interesting (1-6)	Overall life satisfaction (1-10)
Panel A: All			
-0.134 (0.154)	-0.0265 (0.160)	-0.0621 (0.170)	-0.0227 (0.294)
Panel B: Disabled			
-0.532 (0.330)	-0.723** (0.350)	-1.104*** (0.356)	-0.859 (0.643)
Panel C: Immigrants			
0.119 (0.297)	-0.0305 (0.296)	0.0878 (0.311)	-0.0196 (0.586)
Panel D: Adolescents			
-0.152 (0.219)	0.224 (0.232)	0.244 (0.252)	0.263 (0.407)

Note: The table shows the IV-estimates of the effect of private job placement services (as compared to the public employment services). All outcome variables are measured on a scale where higher values indicate a better life situation. Each cell show the effect from a separate regression, with different outcomes across columns and different (sub)samples across lines. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*. Robust standard errors are in parentheses, */**/** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence.

5 Conclusions

When a government should provide services in-house and when it should contract out provision is a central policy question. In this paper we have assessed the case for contracting out employment services to private job placement agencies in Sweden. The setting exhibits many of the ex-ante arguments for when the scope for private provision is likely to be large, as suggested by Grossman and Hart (1986), Hart and Moore (1990) and Hart (1995). In this setting, the service is well defined and contracts are highly incentivized; the extent of non-contractible quality is limited; there is a substantial competition for contracts; current performance may have consequences for future procurement; and there is a substantial capacity for innovation.

In order to identify the effects of private employment services we have generated experimental variation where unemployed adolescents, disabled, and immigrants were randomly given an opportunity to switch from the PES to a private job placement agency during the intervention period. Over 2,400 unemployed individuals were randomly allocated into an experimental group, of whom about a quarter decided to switch to a private provider. Almost 2,400 individuals were randomly assigned to a control group, who did not get the opportunity to switch providers.

The results indicate that private job placement agencies innovate the business in the sense that they use a more labor intense technology when providing employment services; unemployed at private providers meet their case worker 1.5 times as often. They also felt that they received more help in improving their job search strategies and more help in finding vacancies, than those at the PES. In general, unemployed at private placement agencies were more satisfied with their case worker. This more frequent interaction with the case worker resulted in a higher job search intensity for adolescents at a private provider; they initiated more contacts with prospective employers, applied for more jobs, and were called to more interviews, than had they been at the PES. Adolescents at the PES, on the other hand, spent more time in job training at employers and internships. Thus, the higher job search intensity for unemployed youths at private providers may in part be driven by lower job search effort among program participants at PES (locking-in effects).

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This increased interaction with case workers did not improve the overall chances of finding a job for unemployed at private placement agencies. There are however important heterogeneities; private providers improved the chances of finding employment and increased earnings for immigrants. We also have some support for private providers having a negative effect on earnings and hours worked for adolescents; in particular after the end of the intervention period.

The positive effects of private placement services for immigrants is particularly large at the end of the intervention period (six first months after randomization) when contractors had strong incentives to find employment for the job seeker in order to obtain full payments. This may indicate that the marginal product of effort, from the placement agencies perspective, was highest for this group. Immigrants are at disadvantage, partly because they may be lacking necessary contacts on the Swedish labor market. In particular, such contacts may be exhausted when being unemployed. Since private providers work actively with helping job seekers to initiate contacts with employers, they may be particularly productive for immigrants.

The potentially negative effects on employment and wage earnings for adolescents at private placement agencies come despite the fact that they applied for more jobs and attended more job interviews. The way we can reconcile this apparent paradox, is that adolescents at the PES received more job training and had more internships, and that these may have generated the necessary interaction with employers to secure employment.

The general conclusion from our finding is that one size does not fit all. Even if the ex-ante case for contracting out employment services in the present setting is strong we do not find any overall effect of effectiveness. There is a substantial heterogeneity, however, which suggests that one has to be careful when deciding on which services to produce in-house and which to contract out.

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Appendix

Table A1. Descriptive statistics of outcome variables Ej reviderad ('Number of months employed' ska revideras om ny definition)

Outcome variables	Experimental group	Control group	Number of observations
Aggregated income 1-3 months (SEK)	10552	9740	4804
Aggregated income 1-6 months (SEK)	24992	23336	4804
Aggregated income 1-9 months (SEK)	42404	40505	4804
Aggregated income 1-12 months (SEK)	60850	58739	4452
Number of months employed 1-3 months	0.720	0.677	4804
Number of months employed 1-6 months	1.633	1.551	4804
Number of months employed 1-9 months	2.755	2.659	4804
Number of months employed 1-12 months	3.948	3.848	4452
Employment 1/3 months after randomization	0.318	0.306	2838
Employment 6/9 months after randomization	0.411	0.410	3415
Hours worked 6/9 months after randomization	12.843	13.112	3408
Job satisfaction 6/9 months after randomization	0.358	0.361	3347
In contact with case worker last week	0.544	0.345	1680
Case worker helped me to improve my job search	0.191	0.050	1680
Case worker provided information of vacancies	0.222	0.109	1680
Sufficient help from case workers to find a job	0.555	0.410	1553
Job search training the last 30 days	0.379	0.242	1680
Job training the last the 30 days	0.094	0.122	1680
Job fair the last 30 days	0.205	0.162	1678
Number of self initiated contacts with employers the last 30 days	3.692	2.979	1593
Number of jobs applied for the last 30 days	8.736	7.724	1621
Number of unannounced jobs applied for the last 30 days	3.284	2.914	1589
Number of job interviews in the last 30 days	0.770	0.544	1621
Feeling happy and in a good mood (1-6)	2.881	2.861	3275
Feeling active and full of energy (1-6)	2.943	2.960	3257
My day have been interesting (1-6)	3.127	3.126	3254
Overall life satisfaction (1-10)	6.149	6.111	3173

Table A2. Effects of private placement services on income 6 months after randomization: OLS, reduced form and IV

	OLS Exp	OLS All	First stage	Reduced form	IV
Panel A: No individual controls					
Private employment service	-4555** (1908)	-2658* (1376)			5418 (3713)
Experimental group			0.277*** (0.0200)	1501 (912.0)	
Panel B: Individual controls					
Private employment service	-3257* (1635)	-2036 (1246)			3796 (3393)
Experimental group			0.277*** (0.0199)	1050 (810.4)	
Male	2676* (1573)	2015* (1144)	-0.0228** (0.0113)	2085* (1141)	2171** (953.0)
Age	-60.63 (107.9)	-132.0* (71.07)	0.00181* (0.000948)	-135.5* (69.98)	-142.3** (70.38)
Unemployed, months	-331.3* (183.3)	-128.4 (104.5)	0.00245* (0.00142)	-133.7 (105.8)	-143.0 (161.9)
Education upper sec.	6288*** (1552)	6517*** (1027)	0.0166 (0.0131)	6461*** (1026)	6398*** (994.7)
Education University	15066*** (2905)	15264*** (2292)	0.0254 (0.0250)	15147*** (2283)	15051*** (1908)
Non-Nordic citizen	-5671*** (2028)	-5115*** (1266)	-0.00144 (0.0144)	-5121*** (1267)	-5116*** (1509)
Unemployment insurance	206.8 (1619)	1855 (1427)	-0.00649 (0.0141)	1871 (1424)	1896 (1312)
Searching full time empl.	4225 (2866)	4224** (1673)	0.0195 (0.0247)	4166** (1698)	4092** (1842)
Extended search area	961.9 (1634)	714.5 (895.0)	-0.00538 (0.0113)	712.7 (888.2)	733.2 (1037)
Long term unemployed	-1935 (2344)	-1845 (1726)	-0.00985 (0.0140)	-1812 (1736)	-1774 (1797)
Pre-study income>0	7789*** (2212)	7509*** (1681)	0.00875 (0.0135)	7512*** (1680)	7478*** (1192)
Monthly (12) pre-study inc.	Yes	Yes	Yes	Yes	Yes
Observations	2410	4804	4804	4804	4804
# Groups	44	51	51	51	51

Note: All models include an intercept and fixed effects of region×target group×experimental wave.

Table A3X. Effects of private placement services on income 6 months after randomization: OLS, reduced form and IV, with individual controls

	OLS (Exp grp)	First stage	Reduced form	IV
Panel A: All				
Private employment service	-3257* (1635)			3796 (3393)
Experimental group		0.277*** (0.0199)	1050 (810.4)	
Observations	2410	4804	4804	4804
# Groups	44	51	51	51
Panel B: Disabled				
Private employment service	-10774** (3993)			6284 (6012)
Experimental group		0.378*** (0.0288)	2377 (2084)	
Observations	308	613	613	613
# Groups	15	17	17	17
Panel C: Immigrants				
Private employment service	-2186 (3904)			17287*** (5815)
Experimental group		0.316*** (0.0211)	5463*** (1520)	
Observations	642	1281	1281	1281
# Groups	13	14	14	14
Panel D: Adolescents				
Private employment service	-1505 (1711)			-5864 (5143)
Experimental group		0.237*** (0.0250)	-1388* (681.2)	
Observations	1460	2910	2910	2910
# Groups	16	20	20	20

Note: All models include an intercept and fixed effects of region×target group×experimental wave.

Table A4. Employment effects of private placement services using survey information
 OBS addera en ytterligare kolumn här (med A2-utfallet)?

Employment 1/3 months after randomization	Employment 6/9 months after randomization
Panel A: All	
0.014 (0.057)	-0.024 (0.055)
Panel B: Disabled	
0.056 (0.122)	-0.027 (0.108)
Panel C: Immigrants	
-0.008 (0.089)	0.110 (0.093)
Panel D: Adolescents	
0.006 (0.087)	-0.092 (0.085)

Note: The table shows the IV-estimates of the effect of private job placement services (as compared to the public employment services). Each cell show the effect from a different regression, with different outcomes across columns and different (sub)samples across lines. All models include fixed effects for each sub experiment and controls for background characteristics described in *Table 3*. Robust standard errors parenthesis, */**/** significant at 10/5/1 percent.