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

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In many countries welfare services that traditionally have been provided by the public sector are 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 closer interaction with their case worker than unemployed at the Public Employment Service (PES); e.g., they receive more assistance in improving their job search technology. We do not find any overall difference in the chances of finding employment between private placement agencies and the PES, but this hides important heterogeneities across different types of unemployed. In particular, private providers are better at providing employment services to immigrants, whereas they may be worse for adolescents. Any effects tend to fade away over time.

Keywords: Job placement, Contracting out, Randomized experiment
JEL-codes: H44, J68, L33

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1 Introduction

Are private contractors better at providing welfare services than are public providers? In many OECD countries welfare 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—with 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 suggests that many public sector activities are 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). Hence, the case for contracting out differs across services and needs to be assessed empirically.

In this paper we use experimental data to empirically assess the case for contracting out job placement: 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. Even if private placement services are present in many countries[1], evidence of its effectiveness is still scarce.[2]

In 2007 the Swedish centre-right government gave the Public Employment Service (PES) instructions to use private contractors more actively as an alternative to in-house

[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).

[2] Winterhager (2006) find small negative and Bernard and Wolff (2008) find positive, 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. A potential worry in these observational studies is that there may be remaining differences across individuals at different providers of employment services.

provision in order to improve matching and strengthen groups with weak labor market attachment (Regeringen 2007). As a result the PES, in July 2007, launched a trial scheme where private contractors were commissioned to match *hard-to-place* unemployed to jobs on the regular labor market. The trial was setup as an experimental intervention where unemployed were randomly assigned to either a private placement agency or to the PES. The private placement agencies faced high-powered incentives (60 percent of the full payment based on successful placements), as compared with no similar financial motivation for the PES.

Our results indicate that unemployed at private placement agencies receive more assistance in improving their job search technology and are more content with their placement worker than are the unemployed at the PES. While we do not find any overall difference in the probability of employment, our results show that immigrants at private providers get significantly higher employment probabilities and wage earnings up to 12 months after randomization. Private contractors also appear to be worse at providing such services to adolescents. An important additional finding is that absent the experimental variation we would have reached different conclusions about the effectiveness of private placement services, even though we have a rich set of covariates.

The procurement process in our setting is representative for public tendering of welfare services. Contracts are awarded based on a two-stage sealed bid tendering process where both price and quality is considered in the second stage; which is in line with EU directive (2004/18/EC) on public contracts. In general, the higher the complexity of the service supplied the more important are non-price attributes in contract awarding criteria (Carpinetti, Piga and Zanza 2006).

The incentives induced by the contracts are strong—as contractors are remunerated on the final outcome, ie. successful placement—compared to contracting of welfare services in other settings which are often paid on some intermediate outcome (eg. fee-for-service) or the demand attracted (eg. voucher per client attracted). Moreover, our contracted outcome captures many aspects of quality and is observable and verifiable, thus further aligning incentives towards efficient production of the desired outcome.

Hence, by providing empirical evidence in a setting with strongly incentivized 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 Hart, Shleifer and Vishney 1997, 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).

2 Institutional setting

The PES in Sweden plays a central role for Swedish labor market policy. In addition to matching and general labor market counseling, case workers assign jobseekers to labor market programs and administer labor market related rehabilitation for those with reduced work capacity (e.g. disabled). The PES also has a control function in the unemployment insurance by monitoring that claimants fulfill the requirements in the insurance of actively searching for jobs (Sibbmark 2008).

The role for private providers in implementing Swedish labor market policy has traditionally been limited; in fact, the PES had a monopoly on employment services on the Swedish labor market up until 1992 when commercial temping and recruiting agencies were allowed to operate (Olofsson and Wadensjö 2009). These are still regulated and are, for example, not allowed to charge jobseekers for matching services.

2.1 Trial with private placement agencies

In 2007, the centre-right government gave the PES instructions to more actively use private contractors to improve the matching between job seekers and employers (Regeringen 2007). The idea was that private providers 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, unemployed within certain target groups were randomly assigned either to a private placement agency, or to the PES. The random assignment is described in section 3.1.

2.1.1 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 (the Malmö metropolitan area, Norrköping and Sundsvall) 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*.[3]

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 the sense 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. They were thus judged to be ready for the labor market, but suffering from difficulties in marketing their skill profiles.

In the procurement, the 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. Also, unemployed who were assigned to private placement agencies did not have access to regular labor market programs during the contracted period.[4] Contractors could not refuse anyone assigned to them, so there was no room to cherry pick easy cases.

[3] The decision to contract placement services for these groups in the trial was taken by the head of PES, with the motivation that these were the PES's groups of priority. It should also be noted the centre-right came to power in the fall of 2006 on a ticket to reintegrate groups far from the labor market.

[4] 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; All groups could get a certain wage subsidy if they were eligible; Individuals for whom the unemployment insurance was exhausted were transferred into a different benefit scheme.

The contracted providers also took over the control function in the unemployment insurance from the PES. Private providers had to report to the PES violations in the requirement for unemployed to activity be searching for jobs, 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.

2.1.2 Assignment of contracted slots

Unemployed covered by the procurement were randomly allocated to the contracted 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 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.[5]

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

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

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

2.1.3 Competition for contracts

At each regional site two competing providers were procured for each target group. The procurement procedure encompassed two stages. In the first stage, bidders had to document their proficiency (e.g. their experience from similar assignments and the

[5] 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

competence of their personnel). 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 on employers; time interacting with job seekers; and the degree of innovation. Bids were finally selected on both quality (60 percent) and 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 regarding price and quality.

The providers awarded a contract did not have any prior experience in the exact services procured, since these services were previously provided by the PES only. Still, the awarded firms did have experience in job placement services, reintegration services, rehabilitation, and labor market training; for example at large lay-offs, firm closures or for individuals being on long-term sick leave as covered by collective agreements between trade unions and employers. Their experience of the particular groups covered by the procurement may, however, have been limited.

The two stage sealed bid tendering, which cuts the lowest quality tail on contractors, is a procedure consistent both with the US and EU legislation for procurement (Carpinetti, Piga and Zanza, 2006). In fact, the Swedish Public Procurement Act (2007:1091) which builds on EU directive (2004/18/EC) on public contracts is based on the principle of most economically advantageous tendering (MEAT), which allows a principal to base the evaluation of bids on other criteria than price. In an overview of European and US public procurement practices, Carpinetti, Piga and Zanza (2006) find that the higher is the complexity of the service supplied the more important are non-price attributes in contract awarding criteria.

2.1.4 Incentives from contracts

The private placement agencies were remunerated largely based on successful placements. The contracts stated a price per unemployed as decided through the procurement; this price differed both across target groups and across providers. 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 a 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.

Table 2 shows average, minimum and maximum contracted prices for different target groups. There was substantial variation on payments between target groups within regions, while the compensation for the same target group did not vary much across regions. On average, private providers received the highest compensation for disabled jobseekers, followed by immigrants and youths. From control group data in *Table 10* it follows that the price variation partly reflects the expected job-finding rates for different groups.

Table 2. Contracted prices (SEK per unemployed) by target group

	Mean	Min	Max
Adolescents	20,193	12,200	25,000
Immigrants	27,452	20,280	35,000
Disabled	35,150	30,000	42,000
Total	25,111		

Note: Means are weighted by the number of treated.

To gain some further insight about the magnitudes involved, we have related the contracted payments for different target groups to their average monthly wages as employed. On average, the private employment agencies received the market worth of about one month's of production (wages plus payroll taxes) for placing an unemployed individual in a job. Thus, assuming that individuals who exited unemployment kept their jobs in three months (which was the formal requirement for receiving the final 60 percent of the payment), society would balance costs and benefits if private employment agencies could boost job finding rates by 17 percentage points.[6] The increase in the job-finding rate needs not to be more than 3.5 percentage points if individuals instead kept their jobs one year on average. Thus, with the payment schedule used in the

[6] The break-even job-finding rate (δ) is derived by equalizing costs (payment to the employment agencies) and benefits (production): $0.4p(1 - \delta) + 0.6p\delta = 0.6ym\delta$, where p is the price, y is monthly production, and m is months of work. Making use of the fact that the price equals roughly one month worth of production ($p = y$), the break-even job-finding rate equals: $\delta = 2/(3m - 1)$.

procurement, even relatively weak effects of employment services may be profitable to society.[7]

Two competing providers were procured for each target group at every regional site. The compensation received, however, varied substantially between different private employment agencies. In some cases, one provider received more than twice the payments compared to the other provider for the same target group in the same region. On average, the agency with the highest compensation scheme, received about 60 percent higher payments than the agency with the lowest compensation scheme. To some extent these price differences reflect the quality ranking of the providers in the procurement.

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.

In our setting the remuneration of contractors is to a large extent (60 percent of the contracted price) based on successful placements. This suggests that the remuneration principle provides stronger incentives for achieving the desired outcome than in many

[7] Note that the simple cost-benefit analysis concerns the total effect of private job agencies, and not the relative effect of private employment services compared to PES. Thus, we relate the total costs of private employment services (payments) to the total benefits (production), and not the difference in costs between private and public agencies (if any) to the difference in production (which is the main focus of this paper).

other situations of public tendering of welfare services, where incentives are more directed at producing an intermediate service; for example are contracted physicians often paid by fee-for-service, and voucher schools in the Swedish setting are remunerated per student.

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 limited in time and scope, it could be viewed, at the time, as a platform for a larger scale privatization of placement services. In fact, the centre-right government expressed such a political will when instructing the PES in 2007 to more actively use private entrepreneurs when providing placement services (Regeringen 2007).[8] The trial scheme could therefore be seen as a storefront, thus giving contractors additional incentives to provide high quality and efficient services in the hope of being awarded future contracts. The trial may also have given the PES incentives to prove their efficiency when facing the threat of a larger scale privatization of its services.

2.2 What is the treatment?

Even if our evaluation will capture the net impact of 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 of changing from a public to a private provider, since ownership in itself can provide motivation.[9] A related issue is that private and public providers within this trial scheme have different incentives; where private placement agencies face substantial financial incentives. An additional incentive effect may also come from private providers hoping to be awarded additional contracts in future procurements.

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

[8] The instructions to the PES in 2008 (Swedish Government 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.
[9] 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.

tenders in the second stage are only selected on the price, there is a considerable risk that firms awarded commission are those that combine a low price with low quality.[10] 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 the 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 general problem when assessing the effect of an intervention is that individuals who are being assigned to, or self-select 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 between treated and non-treated.

The introduction of the trial with private placement service gave us an opportunity to set up an experiment, together with the central administration of the PES. In the

[10] 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.

experimental intervention, unemployed were randomized into an experimental group and a control group; those assigned to the experimental group were then given an opportunity to switch from the PES to a private placement agency during the trial period, while those in the control group remained at the PES.

3.1.1 Randomization procedure

The experiment was staged in six experimental waves. At each wave, unemployed within the sampling frame (target groups in each region) were either randomized to a specific private provider or to a control group at the PES; or else belonged to the non-experimental group. The randomization was carried out within each region-target group-wave combination, thus generating 33 sub-experiments to be used in the analysis. [11]

The randomization was carried out by the central body of the PES, and is based random numbers attached to birth dates; where the same random numbers are used in all waves. At each wave the central body of the PES used their administrative registers to identify all unemployed within the sampling frame; both unemployed in the stock of unemployed previously not randomized to either a private provider or the control group, and newly unemployed flowing into the sampling frame. For each sub-experiment (i.e. region-target group-wave combination) the central body of the PES set specific thresholds for the random numbers so that all unemployed in the sampling frame were either allocated to treatment group 1, treatment group 2, control group 1, or control group 2; or else belonged to the non-experimental group. Unemployed who were randomized into either a treatment or a control group in a specific wave did not belong to the sampling frame in subsequent waves. After each randomization the central administration of the PES contacted the local branch and gave them a list with the outcome of the randomization. To account for differences between unemployed belonging to the stock (not previously randomized to treatment or control groups) and newly unemployed flowing into the sampling frame in the analysis, we define separate sub-experiments for “stock” and “flow” unemployed. Moreover, in the analyses we do not distinguish between treatment groups 1 and 2 and between control groups 1 and 2

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

(with the exception of the price analysis in section 4.3). The randomization procedure is illustrated in *Figure 1*.

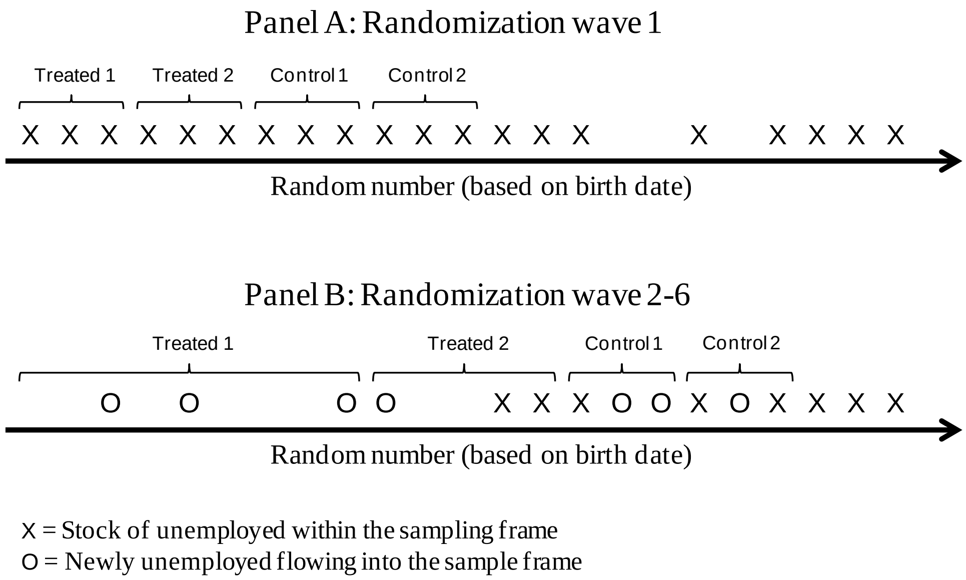

Figure 1. Description of the randomization

It should be noted that local caseworkers at the PES had no opportunity to manipulate the assignment. We have verified that no one randomized to the control group were treated by a private provider. 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. In total, the experimental intervention included 4,804 individuals, of whom 2,410 were randomized to the experimental group.

Table 3 shows how individuals subjected to the randomization are distributed across regional sites, target groups, as well as the relative size of the experimental waves. The largest regional trial site was Norrköping followed by Sundsvall 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 the 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.

Table 3. Sample description

	Experimental group	Control group
Regional trial sites		
Malmö	0.251	0.249
Sundsvall	0.293	0.294
Norrköping	0.456	0.457
	1.000	*1.000*
Target group		
Adolescents	0.606	0.606
Immigrants	0.266	0.267
Disabled	0.128	0.127
	1.000	*1.000*
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
	1.000	*1.000*
Observations	2,410	2,394

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

At the end of this meeting the individuals had to decide on whether 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 opportunity could not opt back to the PES during the 6 (3) month intervention period and private providers could not refuse anyone assigned to them.

3.1.2 Outcome of the randomization

To check if our random assignment was successful in balancing the experimental and control group, we compare the groups with respect to an array of observable and pre-determined background characteristics; see *Table 4*. We find the experimental and control groups to be similar with respect to gender, age, non-Nordic citizenship and the 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 under 25 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 also have similar educational attainment; around 62 percent have high school education, while only 12 to 13 percent carry a university degree. Turning to income the 12 months before the intervention[12], we find no differences in pre-study income or the share with a zero income. All in all, the randomization has generated a good balance between the groups. This is also confirmed by the fact that there is no difference in predicted wage earnings 6 months after the randomization.[13]

Table 4. Balance of the experiment

	Experimental group	Control group	Difference	(Standard Error)
Male	0.520	0.533	-0.013	(0.015)
Age	29.1	29.1	0.047	(0.342)
Unemployed (months)	3.54	3.57	-0.033	(0.110)
Long term unemployed	0.254	0.258	-0.004	(0.013)
Education compulsory	0.241	0.257	-0.016	(0.013)
Education upper sec	0.626	0.623	0.002	(0.014)
Education University	0.133	0.120	0.013	(0.010)
Pre-study income	47547	44539	3007	(2007)
Pre-study income>0	0.701	0.697	0.003	(0.014)
Non-Nordic citizen	0.133	0.133	0.001	(0.010)
Unemployment insurance	0.393	0.386	0.007	(0.014)
Searching full time employment	0.961	0.960	0.000	(0.006)
Extended search area	0.380	0.370	0.010	(0.014)
Predicted wage earnings	23688	23336	352	(448)
Observations	2410	2394		

Note: Column 1 displays mean characteristics for the experimental group, while column 2 displays weighted mean characteristics for the control group, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group. Differences and standard errors come from a regressing each characteristic on an indicator for group using a weighted regression, where D=1(0) for the experimental (control) group. The predicted wage earnings are obtained by regressing wage earnings 6 months after the randomization on the background characteristics and a fixed effect for each region-target group-wave cluster for individuals in the control group, and evaluating the experimental and the control groups at the obtained coefficients. $^{*}/^{**}/^{***}$ indicates that the difference is significantly different from zero at the 10/5/1 percent level of confidence.

[12] The pre-study income is defined as the income during the 12 calendar months before randomization month.

[13] The predicted wage earnings are obtained by regressing wage earnings 6 months after the randomization on the background characteristics and a fixed effect for each region-target group-wave cluster for individuals in the control group, and evaluating the experimental and control groups at the obtained coefficients.

3.1.3 Compliance

The compliance in the experiment, or the take-up, was relatively low; only 28 percent of those randomized to the experimental group chose to switch from the PES to a private placement agency. As seen in *Table 5* 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 individuals who have recently found employment,[14] and in part due to a relatively low share of the unemployed being eligible for benefits from the unemployment insurance. Only individuals on unemployment insurance could get sanctions in their unemployment benefits for not attending the meeting, and 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 (23 percent covered) 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 5. Compliance in the experimental group

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

Note: The numbers within parenthesis display the proportion of unemployed relative to the full experimental group by target group.

An important question for the interpretation of the results is whether these compliers are representative of the underlying population; that is, if individuals deciding to switch from the PES to a private placement agency, if given the opportunity, have better (or worse) re-employment prospects. In Table 6 we therefore describe differences in

[14] For a discussion on misclassification in Swedish unemployment registers see Bring and Carling (2000) and Bennmarker et al. (2000).

observable characteristics between compliers and non-compliers. Compliers are not fully representative of the population in the target groups. We see that compliers have on average higher educational attainment, are older and have a longer unemployment spell. Moreover individuals with unemployment insurance are more likely to comply with the randomization, as are women.

In sum, compliers seem to be a positively selected group of individuals in some respects and negatively selected in others. On average, however, there is no difference in employment prospects between compliers and never-takers based on these observed characteristics: Predicted wage earnings six months after randomization are similar for the two groups.

Table 6. Characteristics of compliers and non-compliers

	Compliers	Non-Compliers	Difference	(Standard Error)
Male	0.477	0.537	-0.060***	(0.023)
Age	31.4	28.2	3.244***	(0.522)
Unemployed (months)	3.839	3.420	0.420**	(0.166)
Long term unemployed	0.260	0.251	0.009	(0.020)
Education compulsory	0.230	0.245	-0.015	(0.019)
Education upper sec	0.614	0.630	-0.016	(0.022)
Education University	0.155	0.125	0.031**	(0.015)
Pre-study income	48102	47333	769	(3134)
Pre-study income>0	0.701	0.701	0.000	(0.021)
Non-Nordic citizen	0.148	0.128	0.020	(0.015)
Unemployment insurance	0.430	0.379	0.051**	(0.022)
Searching full time employment	0.951	0.964	-0.014	(0.009)
Extended search area	0.392	0.376	0.016	(0.022)
Predicted wage earnings	23015	23945	-930	(712)
Observations	669	1741		

Note: Column 1 and 2 display mean characteristics for compliers and non-compliers in the experimental group. Differences and standard errors come from regressing each characteristic on an indicator for compliance in a weighted regression, where D=1(0) for the compliance (non-compliance). The predicted wage earnings are obtained by regressing wage earnings 6 months after the randomization on the background characteristics and a fixed effect for each region-target group-wave cluster for individuals in the control group, and evaluating compliers and non-compliers at the obtained coefficients. $^{*}/^{**}/^{***}$ indicates that the difference is significantly different from zero at the 10/5/1 percent level of confidence.

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 4* (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 private providers have strong incentives to make sure they receive their payments.

We use two different data sources for outcomes; both survey information and wage income data that employers are mandated to report to the tax authorities.[15] Descriptive statistics of all outcome variables are available in Appendix B. For income tax declaration purposes employers have to report the annual wage sum paid to each employee, and the months for which wage is paid. For every individual in the experiment we 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 and an employment indicator month-by-month for each individual.[16]

We have also administered two surveys to all individuals in the experiment.[17] The first survey was collected either one or three months after individuals were subjected to randomization, and is mainly focused at capturing differences in working methods, but also collects information on short run employment outcomes. [18] 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.

[15] We do not use information on employment status from the PES's unemployment register. Private providers in the (post) intervention period have much stronger incentives to report employment than have the PES.

[16] Employment is defined as having a monthly wage earning larger than 9,700/9,400/5,700 SEK for disabled/immigrants/adolescents. The cutoff is based on the median monthly wage earnings for respective group (before the experiment), representing 63%/61%/37 % of the full-time minimum wage (first percentile). We have used a cut-off larger than zero to reduce noise caused by e.g. delayed holiday payments or over-time compensation.

[17] 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, both to an individual in the experimental group and to an individual in the control group. Fortunately the error was random, but it effectively reduced the response rate to 60 percent.

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

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. We find no systematic differences in observable characteristics between responders in the experimental and the control groups.

3.3 Estimation method

In the experiment, compliance to treatment was voluntary for individuals who were randomized into the experimental group. Any differences in outcomes between the experimental group and the control group therefore reflect the intention-to-treat effect.[19]

To estimate the treatment effects of receiving job placement services from a private contractor—rather that the intention-to-treat effect—we use the random assignment as an instrument for going to a private provider. The experimental set-up, where individuals in the experimental group received an offer to switch to a private agency, but where those in the control group were excluded from treatment, makes it possible to identify the effect of treatment on the treated (TT) (Imbens and Angrist 1994). This can be obtained under somewhat milder assumptions than what is typically required to identify the local average treatment effect (LATE) in instrumental variable approaches.[20] The identifying assumption is that the assignment of offers is ignorable (see for example Angrist, Imbens and Rubin 1996).

That the treatment offers were really randomly assigned is indicated in *Table 4*, showing the balance of the experiment, 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 A1*, column 2, in the Appendix).

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

[19] The intention-to-treat effects for accumulated income six months after randomization are displayed in Appendix B.
[20] Since individuals in the control group cannot receive treatment, there are no "always-takers" or "defiers". Thus, the monotonicity assumption (no defiers) is fulfilled by definition.

$$Y_{ij} = \alpha + \delta Private_{ij} + \mathbf{X_{ij}\beta} + \lambda_j + \varepsilon_{ij}$$
$$Private_{ij} = \alpha + \phi Random_{ij} + \mathbf{X_{ij}\theta} + \lambda_j + e_{ij},$$

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 a private placement agency, which is endogenous due to non-compliance. In order to ensure balance among treated and non-treated we instrument treatment status with the random assignment of the option of getting treatment, $Random_{ij}$. To further ensure balance and to reduce residual variance we control for a vector, $\mathbf{X_{ij}}$, of background characteristics described in *Table 4*.[21] 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.[22] Hence, δ captures the average effect of private placement services for individuals choosing to participate, i.e., the average effect of treatment on the treated.

3.3.1 Benefits of experimental variation

The benefits of using an experimental approach—rather than relying on selection on observables—can be appreciated from *Table A1* in the Appendix. Here we assess the effects of private placement services on the accumulated wage earnings 6 months after randomization. Looking first at the OLS estimates in column 1, where we compare unemployed who are under treatment at private placement agencies with those in the experimental group choosing to remain at the PES.[23] When controlling for a rich set of covariates, $\mathbf{X_{ij}}$, in the lower panel we find a negative and significant estimate: Being under treatment at a private job placement agency reduces earnings 6 months after randomization with over 3,000 SEK. Comparing this estimate where we control for $\mathbf{X_{ij}}$ with the upper panel result without covariates, we see that the point estimate is slightly reduced but stay essentially the same, thus suggesting that the selection to private providers on observables would not be a problem.

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[21] Column 4 of *Table A1* in the Appendix display the effects on accumulated wage earnings 6 months after randomization with, and without, the vector, $\mathbf{X_{ij}}$, of background characteristics. The IV-estimates without controlling for the baseline covariates are given for all outcomes in Appendix B.

[22] In the econometric specifications we use 51 fixed effects, rather than 33 as in the number of sub-experiments. The reason is that we define separate experiments for individuals being “stock” and “flow” sampled, see *Figure 1*.

[23] We confine this analysis to those in experimental group as this is the population we would have analyzed absent of the experiment.

When we instead exploit the experimental variation to identify the effect of private employment services, we find positive and insignificant effects (See the IV-estimates in column 4). As discussed above, our setting with full compliance in the control group implies that both the IV-estimate and the OLS-estimate should be equivalent to the average effect of treatment on the treated (Imbens and Angrist 1994). Hence, the benefits of our identification strategy are obvious; had we tried to identify effects by conditioning on a rich set of observables instead of running an experiment, we would have risked drawing erroneous conclusions about the effectiveness of private employment services (Equality of point estimates is rejected p-value=0.055). This is particularly true when analyzing the different target groups separately: For the disabled group the OLS gives a strong negative effect on earnings (-10,774 SEK) while the IV gives a positive but non-significant effect (Equality of point estimates is rejected p-value=0.0146); for immigrants the IV estimates indicate strong positive effect (17,287 SEK) whereas the OLS gives a negative and non-significant effect (Equality of point estimates is rejected p-value=0.0032). These estimates on subgroups are reported in Appendix B.

4 Results

The motivation for contracting out job placement services to private providers is that private providers may prove to be more effective in matching unemployed to vacancies. In this section we will first analyze differences in the technology of delivering jobs to unemployed; that is we describe differences in how the unemployed spend their time, how they search for jobs and how they interact with their case worker. Thereafter we analyze the effects of private placement services on labor market outcomes.

In all analyzes we control for the background characteristics described in *Table 4*. All results follow through also when not controlling for background characteristics; these sensitivity analyses are reported in Appendix B.

4.1 Differences in working methods

The general picture is that private job placement agencies use a more labor intense technology. Unemployed at private providers spend more time with their case officer where they get more information on vacancies, receive more help in improving their job search strategies, and are more satisfied with their case worker. Adolescents, in particular, appear to have 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 2* we describe the number of 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 mean values of the control group and for private placement services the figure displays IV-estimates of the effects of going to a private provider added to the means of the control group. The number of hours per week sum up to 40 for all individuals surveyed, thus 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 unemployed at the PES; the difference of 1 hour and 20 minutes being statistically significant. This implies that private placement agencies are substantially more labor intense in delivering placement services.

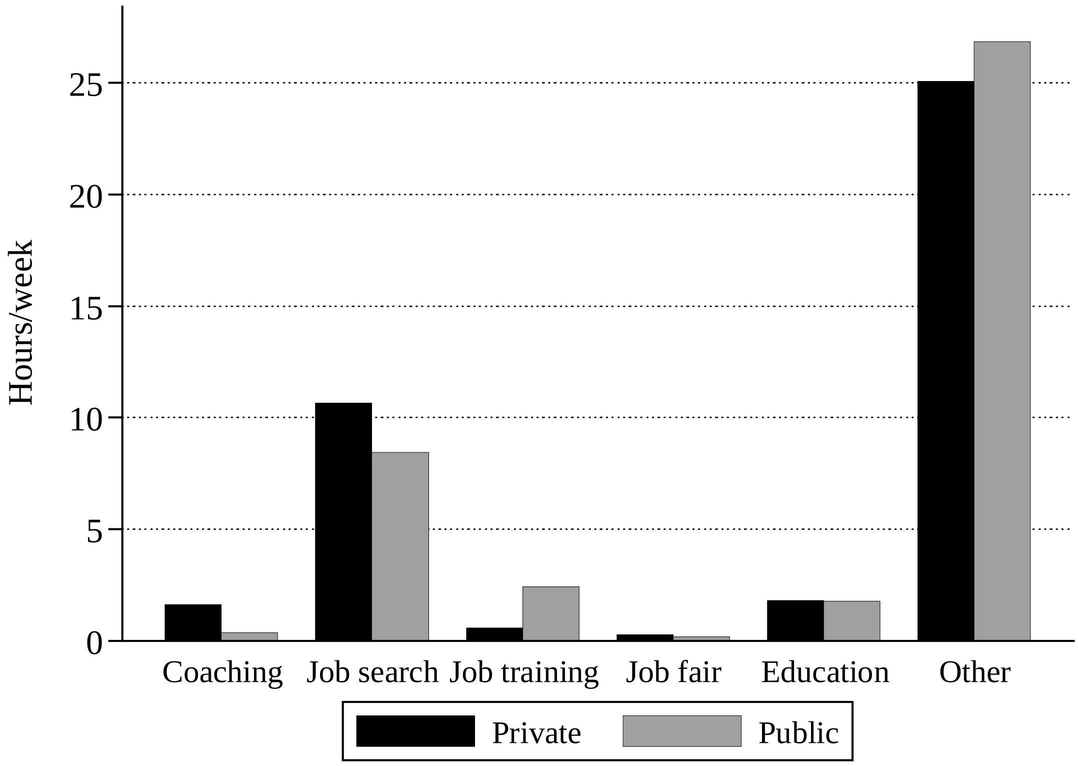

Figure 2. Hours spent in different activities last week

Note: For the public employment services the figure displays means of the control group. For private placement services the figure displays IV-estimates of the effects of going to a private provider added to the means of the control group. Only the difference in hours spent in *Coaching* is statistically significant ($p<0.001$). Point estimates and standard errors of differences are displayed in Appendix B.

Figure 2 also indicates that unemployed at private providers spend more time searching for a job; the unemployed at private providers spend almost 11 hours a week searching for jobs, while those at the PES spend around eight and a half hours. This includes getting instructions on how to search for jobs effectively (e.g. writing a CV and preparing for interviews). Another difference is that the PES uses job training and internships at employers to a larger extent. While these differences in hours spent on searching jobs and in job training are suggestive they do not reach statistical significance. Separate results for each target group are reported in Appendix B.

The results in *Table 7* corroborate the finding that unemployed at private providers spent substantially more time with their case officer every week. 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. As only 35 percent of the unemployed at the PES meets with their case worker in a given week this difference amounts 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 on their job search strategy and provide them with information on available vacancies. This is particularly true for immigrants; a group with potentially weaker connection to norms and networks on the Swedish labor market. The relative effects are very large, since almost no one at the PES report to have received help to improve their job search strategy or information on specific vacancies.

Table 7. Effects of private placement services on contacts with case worker

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***	0.335***	0.274***	0.341***
(0.0586)	(0.0373)	(0.0443)	(0.0613)
[0.345]	[0.050]	[0.109]	[0.410]
Panel B: Disabled			
0.397***	0.303***	0.196**	0.541***
(0.150)	(0.0881)	(0.0977)	(0.167)
[0.391]	[0.013]	[0.072]	[0.398]
Panel C: Immigrants			
0.486***	0.504***	0.421***	0.322***
(0.0948)	(0.0689)	(0.0792)	(0.0987)
[0.357]	[0.075]	[0.094]	[0.338]
Panel D: Adolescents			
0.490***	0.222***	0.184***	0.251***
(0.0884)	(0.0532)	(0.0661)	(0.0910)
[0.322]	[0.047]	[0.130]	[0.460]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Unemployed at private providers are also much more satisfied with the service received, as seen in column 4 of *Table 7*; the share that says that they have received sufficient help to find a job is 0.34 higher, which amounts to a 83 percent increase. It is in particular disabled, a group that is possibly furthest away from the labor market, who state that they have received sufficient help when being at a private provider.

Figure 2 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 8* this is supported by survey questions asking on job search activities during the last month. However, the lower probability of participating in job training emanates entirely from adolescents, while immigrants and disabled are not less likely to attend job training. A potential explanation for this is that the PES has positive experience of job training for adolescents, and therefore have specific programs geared at providing job training for adolescents.

Table 8. 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***	-0.0467	0.103**
(0.0545)	(0.0378)	(0.0469)
[0.242]	[0.122]	[0.162]
	Panel B: Disabled	
0.557***	0.0440	0.171
(0.132)	(0.0912)	(0.108)
[0.151]	[0.065]	[0.097]
	Panel C: Immigrants	
0.461***	0.0488	0.228***
(0.0907)	(0.0554)	(0.0858)
[0.266]	[0.095]	[0.198]
	Panel D: Adolescents	
0.221***	-0.128**	-0.0268
(0.0826)	(0.0607)	(0.0678)
[0.256]	[0.158]	[0.160]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

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 8* shows that immigrants and disabled at private job placement agencies are much more likely to attend such events. Adolescents at private providers, on the other hand, do not visit job fairs more often than those at the PES.

Consistent with *Figure 2*, we also find that unemployed at private providers more frequently participate 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 143 percent is large, (35 percentage points), and effects are present in all subgroups.

While unemployed at private providers have a more frequent interaction with their case worker—helping job seekers to improve job search techniques and creating contact surfaces with employers—we are interested in whether this resulted in higher job search intensity. When asking about job search intensity during the last month, *Table 9*, we find that being exposed to a private provider causes 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. The point estimates for immigrants and disabled suggest that also these may have been more active in initiating contacts with employers, but the estimate are imprecise.

Table 9. 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**	1.999	0.753	0.534***
(0.690)	(1.303)	(0.704)	(0.176)
[2.979]	[7.724]	[2.914]	[0.544]
Panel B: Disabled			
1.493	0.931	1.367	0.639
(1.868)	(3.208)	(1.476)	(0.463)
[2.725]	[6.174]	[2.114]	[0.347]
Panel C: Immigrants			
0.686	0.158	-0.295	-0.0645
(0.899)	(2.172)	(1.186)	(0.349)
[3.204]	[7.313]	[3.055]	[0.765]
Panel D: Adolescents			
2.220*	4.258**	1.402	0.983***
(1.204)	(2.079)	(1.118)	(0.243)
[2.926]	[8.510]	[3.080]	[0.475]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

4.2 Effects on labor 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 though 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. We assess effects both on the prospects of finding a job and three proxy measures of matching quality: monthly wage earnings; hours worked; and job satisfaction. The results do not enable us to reject the null hypothesis of no overall effect of private employment services on the prospects of finding a job. There are however important heterogeneities across the target groups; in particular, we systematically find positive effects on employment and earnings for immigrants at private providers and also some support that private providers would have a negative effects on earnings and hours worked for adolescents.

4.2.1 Employment

In *Table 10* we present effects on employment of receiving job placement services from private providers. 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 wage income over a threshold. We estimate the employment effect as the number of months with earnings 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. The results show that unemployed at a private job placement agency worked on average 0.05 months (8 percent) more during the three first months after randomization, but this difference is not statistically significant. However, the large standard error implies that we can only rule out that the effects in not larger than a 49 percent increase (or smaller than a 28 percent reduction) in employment.

The average effect hides interesting differences across subgroups. While the point estimate for adolescents is negative, the effect is positive and significant for immigrants. Immigrants at private providers worked 0.42 months more during the first quarter after randomization, than had they been at the PES, thus corresponding to a 119 percent increase in employment. This pattern is similar also six, nine and twelve months after randomization; there is still an indication that adolescents at private providers are doing worse, while the effect for immigrants is still large and positive.

Table 10. Employment effects of private placement services summed over different numbers of months after randomization

Employment 1-3 months	Employment 1-6 months	Employment 1-9 months	Employment 1-12 months
Panel A: All			
0.0481	0.142	0.155	0.108
(0.104)	(0.202)	(0.308)	(0.428)
[0.561]	[1.330]	[2.321]	[3.360]
Panel B: Disabled			
0.132	0.317	0.418	0.268
(0.170)	(0.349)	(0.556)	(0.773)
[0.297]	[0.728]	[1.396]	[2.211]
Panel C: Immigrants			
0.419***	0.963***	1.241***	1.312**
(0.146)	(0.299)	(0.470)	(0.653)
[0.352]	[0.880]	[1.678]	[2.588]
Panel D: Adolescents			
-0.209	-0.418	-0.591	-0.862

(0.171)	(0.328)	(0.493)	(0.710)
[0.708]	[1.653]	[2.796]	[4.019]

Note: The table shows the IV-estimates of the effect of private job placement services. The outcome variable is the number of months with a wage earning above 9,700/9,400/5,700 SEK for the Disabled/Immigrants/Adolescents. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, */**/*** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

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 also present 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 at 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. The solid line represents the effect on the probability of finding a job in a specific month, and the dotted lines indicate the 95-percent confidence interval of the estimated effect. Panel A displays the overall employment effects of placement services at private providers; we see here that effects are close to zero and insignificant throughout the whole follow-up period 13 months from randomization. Still, we cannot rule out substantial positive or negative effects.

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, but we should be careful interpreting results as disabled is the smallest subgroup (only 13 percent of the treated); even if point estimates are substantial in size they are fairly imprecisely estimated.

For immigrants there is a positive employment effect from being at a private provider peaking in the latter part of the intervention period six months after the randomization, with the estimates being significant 2, 3, 4 and 6 months after randomization. Over a longer follow-up period the size of the estimated effects peters out. The pattern with a potentially negative employment effect for adolescents is visible in Panel D, where point estimates are consistently negative between the second and the eighth month after randomization. These effects never reach statistical significance though.

In sum, immigrants who switched to a private job agency were more likely to find employment in the following months, than those who remained at the PES. Youths who were served by a private job agency, on the other hand, appears to become worse off. How can we reconcile these divergent results? The difference in employment effects may stem from two possible channels; differences in the treatment received (type and dose) and/or differences in the response to a given type of treatment (heterogeneous treatment effects).

The previous section showed that both immigrants and youths received more job search assistance at the private job agencies than at the PES. However, the working methods differed. Immigrants were significantly more likely to receive help to improve their job search, and to get information on job vacancies, than did the youths. Further, immigrants who were served by a private provider were significantly more likely to attended more job fairs, while youths instead were less likely to receive job training. Thus, the larger employment effects for immigrants is consistent with a different type of job search assistance at the private employment agencies, while the lower probability to attend job training for youths may partly explain the negative effects for them.[24]

To investigate whether some groups are more responsive to treatment than others, we have conducted a number of heterogeneity analyses with respect to the individuals' background characteristics. In particular, we have divided the data by a number of pre-determined variables, such as gender, education, age, unemployment insurance, length of unemployment spell and pre-randomization earnings. The most striking result from this exercise, is that the positive effects of switching to a private employment agency is concentrated among individuals with higher-than-average yearly earnings prior to randomization. The effect for individuals with lower-than-average pre-randomization

[24] It may seem a bit puzzling that adolescents on the one hand increase their search intensity at private providers, but on the other hand are less likely to find jobs. One possible explanation for this result is that youths at private agencies are less likely, than those at the PES, to attend job training at employers. Thus, the difference in job search intensity for adolescents may in part be driven by lower job search effort among program participants at PES (locking-in effects). Another possible explanation is that youths who go to private providers are encouraged to search jobs for which they are not fully qualified. They may also oversell their qualifications and competences, which will make them more likely to go to job interviews. Once meeting with the employers, however, their chances of getting the job may be small.

earnings is significantly negative.[25] Thus, private agencies seem to be more efficient in providing jobs for the relatively better job-seekers, while the PES are more efficient in finding jobs for those with worse prospects. Since youths are over-represented in the group of individuals with no or little prior labor market experience, this may also help to explain the divergent result between immigrants and youths.

We think the heterogeneous effects by pre-experiment earnings can be understood given the different incentives and regulations for private and public employment agencies. Private providers had incentives to focus on individuals where they expected the highest marginal returns to their services. Presumably, it is easier to find jobs for individuals who are more closely attached to the labor market, than for individuals with no or little previous labor market experience. The PES, on the other hand, has lesser incentives to discriminate job-seekers by prior labor market experience. Quite the contrary, their regulations instruct them to focus especially on the unemployed with the poorest prospects. Moreover, the PES has long experience with helping their weakest clients, and may be more efficient in doing so than the private providers.

[25] Having low earnings before the experiment may just be a proxy for being young, and, thus, not very informative about the mechanisms behind the divergent results for youths and immigrants. However, we have conducted the heterogeneity analyses also within target groups. The employment effects for adolescents then becomes positive for those with higher-than-average pre-assignment earnings, but the point estimate is not statistically significant. The employment effect for youths with lower pre-earnings is still negatively significant. On a similar note, the employment effect for immigrants with higher-than-average earnings before the experiment is still positive, while the effect for those with lower pre-earnings becomes insignificantly negative or very close to zero. Thus, private employment agencies seem to be more efficient in providing jobs for individuals with higher-than-average pre-assignment earnings both in general and within target groups.

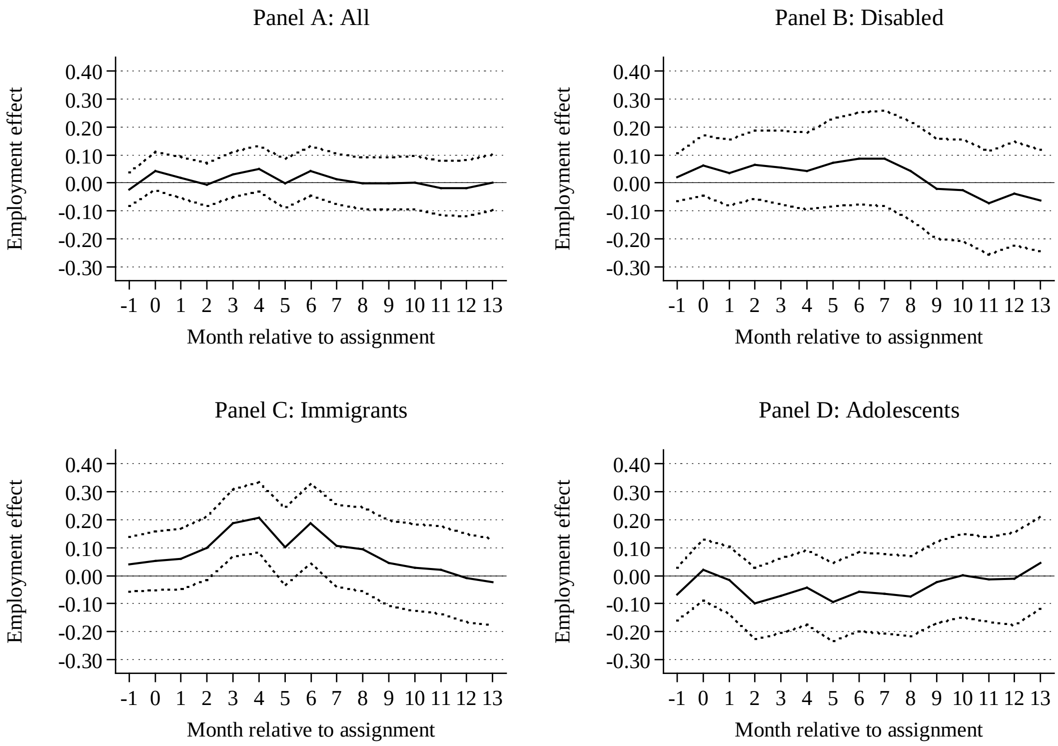

Figure 3. Employment effects of private placement services at different months after randomization

Note: The solid line shows IV-estimates of the month-by-month effect of private job placement services, while the dotted lines show the 95 percent confidence intervals using robust standard errors. The outcome variable is positive wage earnings. All models include fixed effects for each region-target group-wave cluster and controls for background characteristics described in *Table 4*. Pre-study income 2 to 13 months before the randomization is included as separate variables for each month.

In these analyzes we have defined employment by way of having a positive monthly wage earnings over a threshold. We have also survey information on employment status, capturing employment at a much lower intensity; employment is defined by respondents answering yes on the question “Did you work to some extent during the last week”. When using the survey definition of employment, reported in Appendix B, we find much smaller employment effect than in *Table 10* and in *Figure 3*. From this we conclude that the employment effects of being under treatment at a private placement agency is present at the margin of substantial work intensity. The more demanding definition of employment is also more aligned with the incentives facing the private contractor; 60 percent of the contracted sum is given to the provider if the client gets a job for three months with full time employment, than is the survey definition.

4.2.2 Wage earnings, hours worked and job satisfaction

Apart from the extensive margin of labor market participation, public and private providers of employment services may also differ with respect to the match quality between employers and job seekers. We therefore estimate the effects on wage earnings, hours worked and job satisfaction.

The earnings effect captures the quality of the match in terms of both hours worked and whether the individual is at a job where his particular set of skills is more productive. In *Figure 4* we display the earnings effect in SEK—month-by-month—of being under treatment at a private job placement agency rather that the PES; the results are very similar to the employment effect. The overall earnings effect is small in size and insignificant up to 13 months after randomization, while we find a positive pattern for immigrants and a negative pattern for adolescents.

Interestingly, in Panel D the earnings effect for adolescents becomes strikingly negative 5 through 12 months after randomization. As the intervention for adolescents had a duration of three months, we see no differences while at the private provider, but after the intervention ended there may be detrimental effects for adolescents from having been at a private provider; in fact, the negative effect is significant 7 and 8 months after randomization.

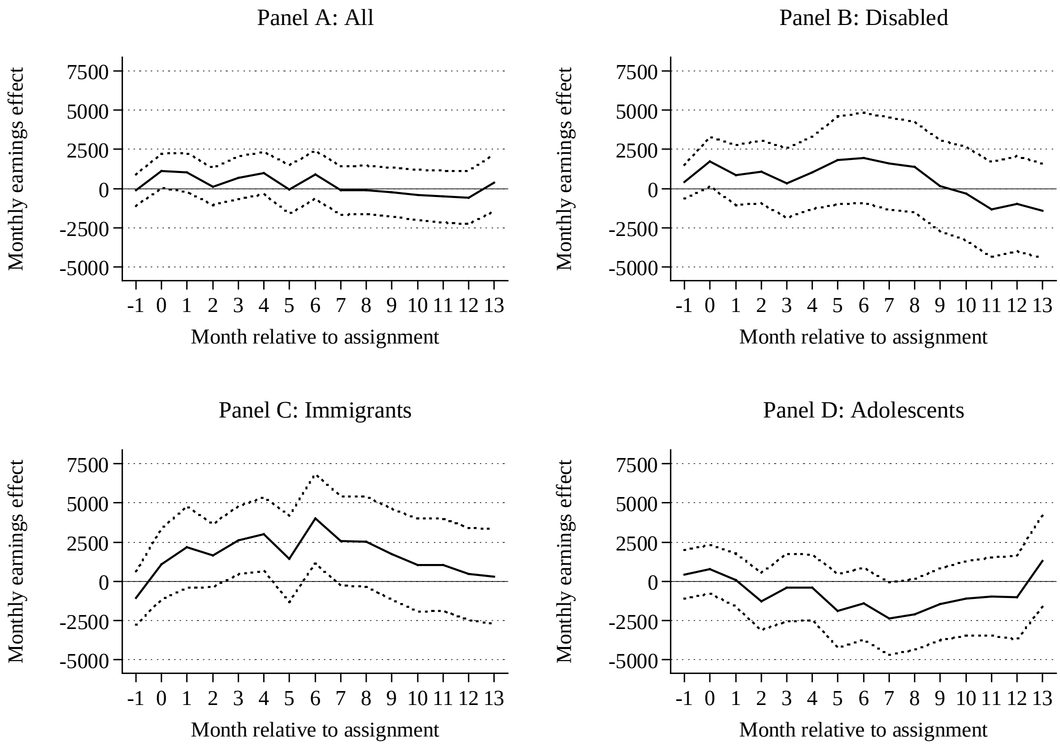

Figure 4. Earning effects of private placement services at different months after randomization

Note: The solid line shows IV-estimates of the month-by-month effect of private job placement services, while the dotted lines show the 95 percent confidence intervals using robust standard errors. The outcome variable is wage earnings. All models include fixed effects for each region-target group-wave cluster and controls for background characteristics described in *Table 4*. Pre-study income 2 to 13 months before the randomization is included as separate variables for each month.

In *Table 11* we estimate the effects of private placement service on aggregated wage earnings 3/6/9/12 months after randomization. In panel A (columns 1 to 4) the overall effects on aggregated earning is insignificant for all time spans. For immigrants (Panel C), on the other hand, we find positive and statistically significant effects on aggregated earnings. After three months the benefit of being at a private provider, instead of the PES, is 7,900 SEK (93 percent) and after another three months the aggregated earnings effect has increased to 17,300 SEK (86 percent). After twelve months, immigrants at a private job placement agency have gained 28,000 SEK compared their peers at the PES; this amount to a 51 percent increase in earnings. The aggregated earnings effect for adolescents, on the other hand, is negative over all time spans from the randomization, but do not reach statistical significance.

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 11* column 5 shows that there on average is an overall reduction in the number of weekly hours worked by 17 percent 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 positive though still being insignificant; the point estimate indicates that immigrants at a private provider works 4.6 hours (43 percent) more per week. (Note that this effect is significant in the specification without covariates) This strengthens the hypothesis that the increase in aggregated earnings partly is due to more hours worked. For adolescents we find a negative and marginally significant effect, indicating that adolescents at private providers work 5 hours (37 percent) 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 11* 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 for the different subgroups.

Table 11. Effects of private placement services on aggregated earning at different months after randomization, hours worked and job satisfaction

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	3796	3408	2456	-2.186	-0.0414
(1646)	(3393)	(5233)	(7319)	(1.884)	(0.0545)
[9740]	[23336]	[40505]	[58739]	[13.112]	[0.361]
Panel B: Disabled					
1842	6284	9163	6210	-2.806	-0.137
(2766)	(6012)	(9559)	(13122)	(3.659)	(0.109)
[7062]	[16490]	[29591]	[45517]	[11.289]	[0.320]
Panel C: Immigrants					
7883***	17287***	24824***	27993**	4.576	0.146
(2774)	(5815)	(9193)	(12611)	(3.179)	(0.0897)
[8508]	[20180]	[36372]	[54646]	[10.756]	[0.298]
Panel D: Adolescents					
-1921	-5864	-12028	-18493	-5.432*	-0.111
(2499)	(5143)	(7834)	(11367)	(2.941)	(0.0854)
[10836]	[26141]	[44583]	[63917]	[14.681]	[0.401]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, */**/*** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

4.3 Price effects

So far we have assumed that all private providers are equal. In reality, however, they differ in contracted price and possibly also in quality. One important question is to what extent different compensation schemes can incentivize private employment agencies to place the unemployed in jobs more efficiently. As noted above, two competing providers were procured for each target group at every regional site, with substantial price differences between them. In this section we attempt to estimate if private providers that receive a higher payment are more efficient in providing jobs than are providers with lower compensation.

It is important to note that the private employment agencies have two margins at which they can maximize revenues. First, they can attract as many unemployed individuals from the PES as possible. This would give them the fixed amount (40 %) of the contracted payment, irrespectively of how successful they are in placing the unemployed in jobs. Second, they can try to find jobs for the unemployed more efficiently. This would give them the final (60 %) of the payment. Presumably, the employment agencies will strive harder along both these margins when the economic incentives to do so increase.

A standard way of analyzing heterogeneous treatment effects is to estimate a model, where the effect of treatment is allowed to vary by the price (i.e. to interact the treatment indicator with the price). This would tell us if private agencies that receive a higher payment also are more efficient in providing jobs for the unemployed. To judge how economic incentives affect behavior among the private employment agencies, we estimate the net effect of giving a private provider a higher compensation scheme (intention-to-treat).

Generally, there are two main complications with estimating the effect of monetary incentives on different outcomes for the unemployed. First, since private providers were selected both on quality and price, a low price may signal low quality. Thus, we would expect a positive correlation between quality and price. Note, however, that all contractors had to meet some minimum quality requirements, and that low-quality providers were eliminated in the two-stage tendering procedure. This will probably weaken the correlation between price and quality somewhat, but we may still overstate the effect of price on the effectiveness of different providers. Second, the composition of the unemployed at different private providers may potentially differ if individuals were free to choose among providers. For instance, if high-price agencies were more successful in attracting unemployed with good employment prospects, the comparison between providers with different compensation schemes may thus be misleading about the effect of economic incentives.

One feature of our experimental design that we have not exploited so far is that individuals were not only randomly given an option to go to a private provider; individuals in the experimental group were also randomly allocated to one out of two providers. Thus, individuals were not free to choose between private providers, and we would expect individuals who were randomly assigned to different providers to be equal on average. This enables us to compare providers for the same target group, in the same region, and with the same average composition of the unemployed, but with different compensation schemes. In other words, we exploit the random allocation of experimental group members to different providers, to study how price incentivize providers.

Formally, we estimate the following equation for individuals in the experimental group: [1]

$$Y_{ijs} = \pi + \theta \ln\left(Price_{ijs}\right) + \mathbf{X}_{\mathbf{ijs}}\psi + \phi_j + \eta_{ijs},$$

where Y_{ijs} is the outcome of individual i in sub-experiment j who was given the option to switch to provider s and $\ln(Price_{ijs})$ is the logarithm of the contracted price for that provider. As before, we add fixed effects for every sub-experiment and some basic control variables. The identifying assumptions for giving θ a causal interpretation as the effect of price incentives is that individuals are randomly assigned to providers with

[1] In principle, also individuals in the control group were randomly allocated to a control group for a specific provider. In practice, however, this makes little sense, since the treatment at PES for individuals assigned to different control groups did not vary. We would therefore expect the effect of being assigned to PES to be the same on average, regardless of which provider's control group they were assigned to. Thus, the control group is uninformative about the relative effectiveness of different providers, and we restrict the analysis to the experimental group only.

different compensation schemes, and that the price is uncorrelated with unobserved outcome determinants (e.g. the quality of the provider).

Table 12. Effects of prices on compliance, employment and income in private placement services.

Compliance	Employment 1-6 months	Employment 1-12 months	Aggregated earnings 6 months after randomization	Aggregated earnings 12 months after randomization
		Panel A: All		
0.047	-0.045	-0.118	76	-646
(0.037)	(0.157)	(0.326)	(2617)	(5414)
		Panel B: Disabled		
0.102	0.588	1.580	14694	44093*
(0.190)	(0.639)	(1.426)	(10905)	(23816)
		Panel C: Immigrants		
0.058	-0.018	-0.457	362	-11369
(0.077)	(0.271)	(0.598)	(5271)	(11510)
		Panel D: Adolescents		
0.044	-0.060	-0.176	-38	-359
(0.044)	(0.194)	(0.401)	(3078)	(6248)

Note: The table shows the estimates of the effect of log(price) on different outcomes, in private job placement services. Each cell show the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Only job-seekers randomized into an experimental group were included in the analyses. All models include fixed effects for each region-target group-wave cluster. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence.

Table 12 shows the effect of contracted prices on compliance probabilities, employment probabilities and wage earnings for the unemployed. Generally, the results show no clear-cut effects of economic incentives on these outcomes. Even though higher-paid private providers have stronger incentives to attract more (and better) job-seekers, the compliance rate does not differ by the compensation received.[2] The effect of prices on aggregated earnings 1-12 months after randomization is positive and weakly significantly different from zero for disabled job-seekers. This may indicate that private job agencies with higher compensation schemes are more efficient in finding jobs for this group. The overall picture, however, is that the link between price structure and success of private job agencies is rather weak. This may be surprising, not at least given the concern that the price partly reflects quality. It is important to note, however,

[2] We have also analyzed if higher-paid private providers are more likely to attract job-seekers with better observable attributes, but found no such systematic pattern.

that the variation in the contracted prices is restricted; after all there are only 14 different compensation schemes, and, thus, the price effects are not measured with any higher precision. Therefore, the analysis can only offer limited evidence on how economic incentives affect placement efficiency.

5 Conclusions

Whether a government should provide services in-house or whether 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.

While our paper captures the effect of private placement services relative to the PES, we would ideally like to contrast these to the value-added of the PES. With a highly efficient PES, positive effects of private placement would be more indicative of technology innovation, than if the PES were operating very inefficiently. Regrettably, it is not possible to evaluate the value-added of the PES in the Swedish setting, since essentially all unemployed are treated by the PES (Forslund and Vikström 2011). Still, our relative results give guidance to policy makers on where to spend marginal resources on placement services.

Our 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 2.4 times as often. The job-seekers 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 difference in job search intensity for adolescents may in part be driven by lower job search effort among program participants at PES (locking-in effects).

This increased interaction with case workers did not significantly improve the overall chances of finding a job for unemployed at private placement agencies; still the point estimates are consistent with substantial effects. There are however important heterogeneities; private providers improved the chances of finding employment and increased earnings for immigrants. We also find some support for private providers having a negative effect on earnings and hours worked for adolescents; particularly after the end of the intervention period. Apart from differences in the type of job search assistance received, the divergent results for immigrants and youths may also be driven by differences in economic incentives and regulations between public and private providers.

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 have fewer 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.

It is interesting to note that the OLS-estimates are much more negative (less positive) than the IV-estimates, given that we cannot characterize the compliers as weaker on the labor market based on the observable characteristics. Hence, the difference between the OLS- and IV-estimates suggests that compliers are negatively selected on unobservable characteristics; that is, that individuals who chose to switch to the private placement agencies are those with bad unobservable re-employment prospects. Since these individuals would fare worse than average if remaining at the PES, they may be more likely to benefit from placement services provided by private agencies; that is, the selection to private placement agencies would be based on rational expectations on high returns. If this indeed was the case, the overall effects would be smaller if private placement services were made universal. However, we do not know the effect for non-compliers in our setting.

The general conclusion from our study 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 definite support for an overall effect. 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 A

Table A1. Effects of private placement services on accumulated wage earnings 6 months after randomization: OLS, reduced form and IV

	OLS (exp. group)	First stage	Reduced form	IV
		Panel A: No individual controls		
Private employment service	-4,555***			5,418
	(1,585)			(3,713)
Experimental group		0.277***	1,501	
		(0.00914)	(1,025)	
		Panel B: Individual controls		
Private employment service	-3,257**			3,796
	(1,408)			(3,393)
Experimental group		0.277***	1,050	
		(0.00912)	(936.6)	
Male	2,676**	-0.0228**	2,085**	2,171**
	(1,361)	(0.00934)	(943.6)	(953.0)
Age	-60.63	0.00181**	-135.5*	-142.3**
	(104.1)	(0.000785)	(70.31)	(70.38)
Unemployed, months	-331.3	0.00245	-133.7	-143.0
	(243.8)	(0.00214)	(160.8)	(161.9)
Education upper sec.	6,288***	0.0166	6,461***	6,398***
	(1,437)	(0.0111)	(991.9)	(994.7)
Education University	15,066***	0.0254	15,147***	15,051***
	(2,879)	(0.0168)	(1,910)	(1,908)
Non-Nordic citizen	-5,671**	-0.00144	-5,121***	-5,116***
	(2,330)	(0.0157)	(1,506)	(1,509)
Unemployment insurance	206.8	-0.00649	1,871	1,896
	(1,812)	(0.0117)	(1,310)	(1,312)
Searching full time empl.	4,225	0.0195	4,166**	4,092**
	(2,665)	(0.0255)	(1,833)	(1,842)
Extended search area	961.9	-0.00538	712.7	733.2
	(1,525)	(0.01000)	(1,034)	(1,037)
Long term unemployed	-1,935	-0.00985	-1,812	-1,774
	(2,537)	(0.0189)	(1,795)	(1,797)
Pre-study income>0	7789***	0.00875	7512***	7478***
	(1653)	(0.0115)	(1192)	(1192)
Monthly (12) pre-study inc.	Yes	Yes	Yes	Yes
Observations	2410	4804	4804	4804
# Groups	44	51	51	51

Note: All models include fixed effects for each region-target group-wave cluster. Robust standard errors are in parentheses, */**/*** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Equality of the OLS and IV estimates for "Private employment service" in Panel B is rejected at p-value= 0.055.

Appendix B

Table B1. Descriptive statistics of outcome variables

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	0.590	0.561	4804
Number of months employed 1-6 months	1.401	1.330	4804
Number of months employed 1-9 months	2.415	2.321	4804
Number of months employed 1-12 months	3.461	3.360	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

Note: Column 1 display mean outcomes for the experimental group, while column 2 displays weighted mean outcomes for the control group, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Table B2. Effects of private placement services on accumulated 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	-3,257**			3,796
	(1,408)			(3,393)
Experimental group		0.277***	1,050	
		(0.00912)	(936.6)	
Observations	2410	4804	4804	4804
# Groups	44	51	51	51
		Panel B: Disabled		
Private employment service	-10,774***			6,284
	(3,532)			(6,012)
Experimental group		0.378***	2,377	
		(0.0282)	(2,240)	
Observations	308	613	613	613
# Groups	15	17	17	17
		Panel C: Immigrants		
Private employment service	-2,186			17,287***
	(3,146)			(5,815)
Experimental group		0.316***	5,463***	
		(0.0185)	(1,805)	
Observations	642	1281	1281	1281
# Groups	13	14	14	14
		Panel D: Adolescents		
Private employment service	-1,505			-5,864
	(1,735)			(5,143)
Experimental group		0.237***	-1,388	
		(0.0112)	(1,216)	
Observations	1460	2910	2910	2910
# Groups	16	20	20	20

Note: All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 3*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, */**/*** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Equality of the OLS and IV estimates for All/Disabled/Immigrants/Adolescents is rejected at p-value=0.055/0.0146/0.0032/0.424

Table B3. Employment effects of private placement services using survey information

	Employment 1/3 months after randomization Survey	Employment 6/9 months after randomization Survey
Panel A: All		
	0.0166	-0.0323
	(0.0558)	(0.0548)
	[0.306]	[0.410]
Panel B: Disabled		
	-0.00802	-0.0540
	(0.127)	(0.108)
	[0.208]	[0.357]
Panel C: Immigrants		
	-0.000237	0.112
	(0.0863)	(0.0912)
	[0.265]	[0.342]
Panel D: Adolescents		
	0.0198	-0.101
	(0.0870)	(0.0854)
	[0.347]	[0.455]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Table B4. Effects of private placement services on hours spent in different activities last week

Coaching	Job search	Job training	Job fair	Education	Other
		Panel A: All			
1.268***	2.215	-1.840	0.115	0.0160	-1.774
(0.292)	(1.608)	(1.286)	(0.138)	(1.077)	(2.050)
[0.270]	[8.287]	[2.613]	[0.168]	[1.885]	[26.776]
		Panel B: Disabled			
3.102	3.940	0.851	0.833	-1.482	-7.244
(2.114)	(6.337)	(3.864)	(0.567)	(2.643)	(7.246)
[0.298]	[9.712]	[1.248]	[0.052]	[1.042]	[27.648]
		Panel C: Immigrants			
0.957***	1.294	0.956	0.332	1.561	-5.100
(0.298)	(2.755)	(1.759)	(0.296)	(2.090)	(3.379)
[0.305]	[9.207]	[1.584]	[0.222]	[2.674]	[26.007]
		Panel D: Adolescents			
1.061***	2.441	-4.368**	-0.169	-0.667	1.702
(0.273)	(2.089)	(2.050)	(0.139)	(1.393)	(2.839)
[0.240]	[7.267]	[3.678]	[0.175]	[1.694]	[26.947]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns. All models include fixed effects for each region-target group-wave cluster and controls for the background characteristics described in *Table 4*. Pre-study income during the 12 calendar months before the randomization is included as separate variables for each month. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Table B5. Effects of private placement services on contacts with case worker estimated *without* covariates

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		
0.485***	0.335***	0.278***	0.340***
(0.0580)	(0.0369)	(0.0440)	(0.0607)
[0.345]	[0.050]	[0.109]	[0.410]
	Panel B: Disabled		
0.417***	0.321***	0.197**	0.542***
(0.144)	(0.0784)	(0.0924)	(0.154)
[0.391]	[0.013]	[0.072]	[0.398]
	Panel C: Immigrants		
0.500***	0.500***	0.430***	0.362***
(0.0931)	(0.0674)	(0.0769)	(0.0973)
[0.357]	[0.075]	[0.094]	[0.338]
	Panel D: Adolescents		
0.497***	0.221***	0.197***	0.256***
(0.0862)	(0.0511)	(0.0653)	(0.0901)
[0.322]	[0.047]	[0.130]	[0.460]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Table B6. Effects of private placement services on job search activities the last 30 days estimated *without* covariates

Job search training	Job training	Job fair
	Panel A: All	
0.348***	-0.0613	0.106**
(0.0542)	(0.0378)	(0.0466)
[0.242]	[0.122]	[0.162]
	Panel B: Disabled	
0.490***	0.0387	0.165*
(0.126)	(0.0789)	(0.0983)
[0.151]	[0.065]	[0.097]
	Panel C: Immigrants	
0.453***	0.00422	0.244***
(0.0885)	(0.0574)	(0.0841)
[0.266]	[0.095]	[0.198]
	Panel D: Adolescents	
0.221***	-0.143**	-0.0166
(0.0810)	(0.0601)	(0.0666)
[0.256]	[0.158]	[0.160]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Table B7. Effects of private placement services on job search intensity the last 30 days estimated *without* covariates

Number of self initiated contacts with employers	Number of jobs applied	Number of unannounced job applied	Number of jobs interviews
	Panel A: All		
1.691**	2.345*	0.824	0.554***
(0.674)	(1.290)	(0.697)	(0.173)
[2.979]	[7.724]	[2.914]	[0.544]
	Panel B: Disabled		
1.541	0.312	1.136	0.683*
(1.496)	(2.863)	(1.272)	(0.401)
[2.725]	[6.174]	[2.114]	[0.347]
	Panel C: Immigrants		
0.927	1.236	0.106	-0.0607
(0.872)	(1.984)	(1.124)	(0.320)
[3.204]	[7.313]	[3.055]	[0.765]
	Panel D: Adolescents		
2.299**	3.886*	1.221	0.950***
(1.123)	(2.021)	(1.095)	(0.236)
[2.926]	[8.510]	[3.080]	[0.475]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Table B8. Employment effects of private placement services summed over different numbers of months after randomization estimated *without* covariates

Employment 1-3 months	Employment 1-6 months	Employment 1-9 months	Employment 1-12 months
	Panel A: All		
0.0864	0.224	0.290	0.301
(0.112)	(0.217)	(0.329)	(0.459)
[0.561]	[1.330]	[2.321]	[3.360]
	Panel B: Disabled		
0.136	0.363	0.486	0.359
(0.183)	(0.368)	(0.584)	(0.812)
[0.297]	[0.728]	[1.396]	[2.211]
	Panel C: Immigrants		
0.541***	1.222***	1.661***	1.866**
(0.176)	(0.348)	(0.537)	(0.731)
[0.352]	[0.880]	[1.678]	[2.588]
	Panel D: Adolescents		
-0.202	-0.420	-0.596	-0.820
(0.179)	(0.346)	(0.519)	(0.754)
[0.708]	[1.653]	[2.796]	[4.019]

Note: The table shows the IV-estimates of the effect of private job placement 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 Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave cluster. Robust standard errors are in parentheses, $^{*}/^{**}/^{***}$ indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

Table B9. Effects of private placement services on aggregated earning at different months after randomization, hours worked and job satisfaction estimated *without* covariates

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					
2,648	5,418	6,150	6,659	-1.725	
(1,814)	(3,713)	(5,717)	(8,039)	(1.944)	
[9740]	[23336]	[40505]	[58739]	[13.112]	[0.361]
Panel B: Disabled					
2,641	8,072	11,614	9,494	-2.381	-0.127
(3,151)	(6,625)	(10,442)	(14,336)	(3.711)	(0.110)
[7062]	[16490]	[29591]	[45517]	[11.289]	[0.320]
Panel C: Immigrants					
10,246***	22,654***	33,447***	39,406***	5.485*	0.159*
(3,435)	(6,935)	(10,768)	(14,579)	(3.313)	(0.0922)
[8508]	[20180]	[36372]	[54646]	[10.756]	[0.298]
Panel D: Adolescents					
-1,888	-5,792	-12,038	-17,456	-5.867*	-0.117
(2,664)	(5,496)	(8,403)	(12,309)	(3.003)	(0.0866)
[10836]	[26141]	[44583]	[63917]	[14.681]	[0.401]

Note: The table shows the IV-estimates of the effect of private job placement services. Each cell shows the effect from a separate regression, with different outcomes across columns and different (sub)samples across rows. All models include fixed effects for each region-target group-wave. Robust standard errors are in parentheses, */**/*** indicates that the estimate is significantly different from zero at the 10/5/1 percent level of confidence. Weighted mean characteristics for the control group (PES) are within brackets, where weights are taken from the distribution over strata, defined by region, target group and wave, in the experimental group.

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