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NOT-FOR-PROFIT PROVISION OF JOB TRAINING AND MEDIATION SERVICES: AN EMPIRICAL ANALYSIS USING CONTRACT DATA OF JOB TRAINING SERVICE PROVIDERS

BY

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Summary

This paper analyses the relative performance and selection behaviour of not-for-profit (NFP) and for-profit (FP) job training service providers, using contract data of welfare-to-work trajectories of the Dutch social benefit administration. Our analysis takes full account of selection effects, both ex ante (i.e. during the contracting process) as well as ex post (i.e. at the start of the program). First, for each cohort type of unemployed clients, service providers are only informed on the broad characteristics of cohorts that are contracted, thus limiting the room for adverse selection here. At the start of trajectories, however, selection may occur (directly) by service providers sending back clients or (indirectly) by encouraging clients to start a program, so as to receive additional (fixed) payments at the start of the program. We find strong evidence for FP service providers to be more selective than NFPs in both ways. Regarding the estimation results for the job placement rates, however, we only find weak evidence for NFP job training service providers to outperform FPs in the durability of job contracts.

Key words: welfare programs, non-profits, procurement, selection, effectiveness

JEL classification: I38, L31, H57

1 INTRODUCTION

Similar to hospital services and the education sector, the provision of job training and mediation services comes with asymmetric and incomplete information problems. The public sector, who is the major contractor of job training service providers in most OECD countries, is therefore confronted with both (adverse) selection and moral hazard problems. First, if job training service providers have an informational advantage on clients, adverse selection or 'cream skimming' occurs if they concentrate on clients with *a priori* better job prospects. Second, regarding the placement rates and wages of clients into new jobs, particularly the long term effects of programs may be hard to

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contract upon, thus giving rise to moral hazard problems. Job training service providers may aim at job placement in the short run, while ignoring the durability of these jobs.

It is often argued that not-for-profit (NFP) organisations may act as a response to market failures in the provision of public services (Hansmann 1980). Regarding the provision of job training services, non-profit case-managers are constrained in their ability to pursue personal gains and therefore are less susceptive to adverse selection of clients and moral hazard with respect to job placement. Moreover, the very lack of residual claimants or profit motives provides a valuable commitment to employees that are intrinsically motivated to provide better services (see e.g. Francois 2000). Thus, if NFPs succeed in attracting these employees, this gives them an advantage with respect to FP service providers. In particular, if NFPs receive donated labour, they may produce at lower costs, provide higher quality, or focus on market segments that are less profitable. This mechanism may also be relevant for the provision of job training services, with case-managers being driven by equity concerns (see e.g. Heckman et al. 2002).

Various empirical studies, particularly focussing on the hospital industry, have tested whether NFPs indeed make a difference (see e.g. Eggleston et al. 2006). Empirical evidence for job training services is however scarce. Major exceptions are Heinrich (2000) and Stoll et al. (2003), who investigate whether NFP job training providers make a difference in their selection behaviour and the placement into jobs. Heinrich (2000), who analyses 637 local service providers of JTPA activities, finds no evidence for systematic differences in the selection of client types between the organisation types. Moreover, FPs have higher job placement rates than NFPs at the moment of termination of a program, but these differences do not persist in the long run. Stoll et al. (2003) present some evidence for adverse selection, as non-profit CBO's (Community Based Organisations) in Boston train participants with more 'barrier characteristics' than other organisations. Similar to Heinrich, they do not find CBOs to outperform FP organisations systematically in terms of job placement rates.

One of the key aspects the above studies is the role of selection by job service training providers. Usually no formal treatment is presented of the selection process of job training service providers. Therefore, high job placement rates can simply be due to the fact that providers select clients with high *a priori* job prospects, and reverse. This choice is often led by data considerations. In particular, US job training service providers have discretion in the (ex ante) choice of their clients, causing room for cream skimming. In the above-mentioned studies, several controls are used for observed differentials in the client composition of FPs and NFPs, while leaving unaddressed the biasing impact of unobserved characteristics, such as the motivation or the

health of clients. Thus, if selection is important and predominantly confined to FPs, the relative performance of NFPs will be underestimated.

This paper contributes to the literature by analysing both the relative performance and selection behaviour of NFP job training service providers. In the empirical analysis, we take explicit account of the selection process of clients, both in the allocation of clients prior to programs, as well as at the start of programs. We use contract data from the Dutch social benefit administration, which is the major contractor of job training service providers in the Netherlands. For various client cohorts that have been contracted out over the period 2002–2005, we observe the job training service provider, its legal status (FP or NFP), the actual region the cohort is located, the contracted cohort size, the effective number of program participants and their job placement rates. With these data, we are able to obtain consistent estimates of performance differentials between FP and NFP job training service providers, both in terms of (adverse) selection and job placement rates. We argue that we take full account of selection effects, both ex ante (i.e. in the allocation of clients to providers) as well as ex post (i.e. the sample of clients that starts the program).

First, the social benefit administration procures various client cohorts that are defined by specific client or cohort types (e.g. older workers, immigrants or lower educated) and/or specific program types that have to be followed (e.g. job training, mediation or self-employment). For each cohort type, multiple cohorts are procured to job training service providers. From the perspective of job training providers, these cohorts are ex ante homogeneous, i.e. the actual clients that will follow the program are not known when providers are awarded with contracts. Instead, providers will only have (limited) information on the *average* characteristics of the various cohort types. We therefore argue that the *within* cohort type variation of client cohorts in the performance outcomes of NFP and FP job training service providers is sufficient to obtain consistent estimates of performance differentials. When estimating our model, we thus include cohort type fixed effects, as any selection within cohort types is basically ruled out by the contracting system.

The second innovative aspect of the paper is that (ex post) selection of clients at the start of programs is measured explicitly in our data. As contracted cohorts are assigned to a specific job training service provider, clients that effectively do not participate in a program can be interpreted as ex post selection effects with respect to clients to a particular service provider. Selection can be attributed to the job training provider itself ('provider induced selection') – i.e. they may send back some clients to the social benefit administration when they consider these not to be suitable for the program. Selection effects may also stem from individual clients for whom their reintegration plan has not (yet) been approved by the social benefit administration. In the empirical analysis, we test whether these types of ex post selection differ

between NFPs and FPs. Moreover, as ex post selection may indirectly affect the average performance of the (remaining) clients on the program, we also use these variables as (additional) controls when estimating models for the job placement rates. If adverse selection is important, we may expect the performance of job service training providers to increase with respect to the number of clients that are returned to the social benefit administration.

In what follows, we first explain the Dutch institutional context and the contract data in Section 2. Section 3 describes the empirical strategy followed, as well as the estimation results. Finally, Section 4 concludes.

2 INSTITUTIONAL CONTEXT AND DATA

2.1 *The Contracting Process*

In the Netherlands, the Disability Insurance (DI) and Unemployment Insurance (UI) scheme are mandatory for all workers. Both of these schemes are carried out by the (public) social benefit administration. Similar to the New Deal in the United Kingdom, unemployed clients – also those receiving Social Assistance benefits – should be offered mediation, job training or subsidised employment within 12 months after the start of the benefit spell. This holds both for UI recipients and DI recipients that have residual income capacity.

As of 2002, the Dutch social benefit administration contracts out the delivery of mediation and job training services to private job training service providers only. In the period under investigation (2002–2005), this was done by the procurement of various cohort types. For each cohort type, multiple cohorts with individual clients were contracted to job training service providers. For instance, in 2002 there were 53 cohort types and 474 individual cohorts that were contracted. During the contracting process, the individual clients within a particular cohort were not known yet by the provider – they were only informed on the average characteristics of the cohort types, the expected cohort size, and contract conditions. When contracts were awarded to a provider, however, the client addresses were transferred to the provider, who in turn had to contact the clients and make a reintegration plan. This plan includes a list of proposed activities to get the client back to work, as well as the rights and duties of the provider and the client. In order for the program to get started, the reintegration plan has to be approved by the social benefit administration. In practice, reintegration plans are not formulated for all individual clients, or are not approved in all cases. Thus, there is a distinction between the 'gross' cohort that has been assigned to a provider on the one hand, and the 'net' cohort of clients for which reintegration plans are submitted and approved by the social benefit administration.

Over the years, the exact number of cohort types has decreased substantially (from 53 cohorts in 2002 to 17 in 2005). These types can be characterised by (combinations of) (i) specific client target groups; (ii) program types; (iii) impairment types; and (iv) the sectors where unemployed clients formerly were employed. The most prominent client groups include immigrants, clients with 'substantial re-employment probabilities', older workers and school leavers or dropouts. Relevant instrument types that have been used are job training, job hunting or self-employment. Further, in about one fifth of the cohorts service providers opted for specific DI recipient categories, like those with 'mild' impairments, mental impairments, visual and hearing problems, behavioural problems and pregnancy related impairments. Finally, only a small group of contracts consisted of workers that try to find a job in another sector (mostly the care sector) than the one they were previously employed.

In the bidding process, the relevant criteria were the proposed and past performance, the price, experience with the specific cohort type and the 'need for their proposed services'. Thus, criteria were mostly of a subjective nature and the legal status of job training service providers was not taken into consideration. At the end of each quarter, the social benefit administration chose the service provider that satisfied these criteria the best. Next, contracts with the job training providers were formalised. The payments per program ranged between 2,500 and 4,000 euro (Cuyper et al. 2005) and were partly related to job placements. Typically, the reward per client consisted of a fixed payment when the reintegration plan was approved (20% at maximum), a fixed payment 6 months hereafter (40–50%), and bonus payments in case of job placement for at least 2 and 6 months (40–50%).[1]

2.2 *The Contract Data*

Table 1 shows sample statistics of all cohorts that have been contracted out to job training service providers from 2002 to 2005. In total, there were 107 cohort types, 1,417 contracted cohorts, 283,569 clients that started job training programs and 87 job training service providers that have been contracted. Thus, there were about 13 individual contracts per cohort type on average. Usually contracts started in the quarter after the bidding and awarding process, and the actual programs formally ended 12 months afterwards. Individual groups of clients could have started their program at various moments, but mostly in the first weeks.[2] In this respect, it should be noted that the

1 For the shortest program trajectories, payments included only a fixed payment when the reintegration plan was approved and a bonus payment for individual clients that have found a job for at least 2 months.

2 This means that the cohort that participates in the scheme exists of 'sub-cohorts' that start at different moments in time, but for which contract conditions and the job training service

TABLE 1 – SAMPLE AVERAGES FOR UI AND DI CONTRACTS, 2002–2005

	2002	2003	2004	2005
No. of procured cohort contracts	474	292	339	312
No. of cohort types	53	20	17	17
No. of contracts per cohort type	8.9 (1.5)	15 (2.1)	20 (3.5)	18 (2.6)
No. of clients (gross)	106,156	88,153	65,111	24,149
Trajectory duration (days)				
Successful	280 (4.1)	280 (4.0)	201 (2.9)	95 (4.1)
Unsuccessful	469 (4.1)	539 (5.5)	394 (4.8)	228 (3.9)
Cohort characteristics				
Fraction DI	0.57 (0.023)	0.59 (0.029)	0.67 (0.026)	0.51 (0.028)
Fraction non-profits	0.053 (0.010)	0.031 (0.010)	0.068 (0.014)	0.0032 (0.0032)
Gross cohort size: assigned participants	224 (11)	302 (14)	192 (7.4)	77 (4.9)
Net cohort size: effective program participants	191 (9.8)	250 (11)	156 (6.2)	58 (3.8)
Performance indicators				
Provider induced selection (fraction of gross cohort size)	0.030 (0.0013)	0.029 (0.0014)	0.013 (0.00081)	0.013 (0.0016)
Client induced selection (fraction of gross cohort size)	0.13 (0.0032)	0.15 (0.0036)	0.18 (0.0048)	0.25 (0.0076)
Fraction employed after completion program:	0.32 (0.0054)	0.29 (0.0067)	0.23 (0.0067)	0.082 (0.0045)
Job contract $\leq$12 months	0.25 (0.0045)	0.22 (0.0054)	0.16 (0.0053)	0.059 (0.0036)
Job contract $>$12 months	0.073 (0.0024)	0.069 (0.0026)	0.065 (0.0025)	0.023 (0.0018)

Standard errors between parentheses.

job placement rates in Table 1 are all measured at the end of December 2005. Thus, the time interval between the start of programs and job placement measurement was shortest for trajectories that started in 2005.

Footnote 2 continued
provider is similar. For these 1,417 contracts, we observe 9,173 'sub-cohorts' that have started their program at different dates. We do not observe the starting dates of these sub-cohorts, so aggregating the data to the level of contracts does not give any loss of information.

For each year, a new classification of cohort types was used by the social benefit administration, amounting to 53 types in 2002 and 17 types in 2004 and 2005. In the contract data, multiple contract observations therefore do not reflect yearly variation. Instead, the panel aspect of the data is that we observe multiple cohort contracts per cohort type for separate years, with *within* variation stemming from multiple regions and service providers per cohort type. We return to this issue in the next section, when discussing the empirical strategy of the paper.

In our sample, the number of clients that follows job training programs decreased substantially over the years, particularly in 2005. It should again be noted however that this drop reflects the fact that contract outcomes in 2005 are not fully recorded by December 2005, which is the reference date for all contracts in our data set. Similarly, contracts that started in 2005 that have not been completed yet by the time of recording cause the average trajectory duration for clients that have not found a job (yet) to be incomplete (and therefore underestimated) as well.[3] In the preceding years, the average trajectory duration of (successful) clients that found a job was between 200 and 280 days, whereas the average contract duration for unsuccessful clients varied between 394 and 539 days.

For each contract the observed cohort characteristics are the specific region for the bidding process (7 in total), the job service provider and its legal status (FP or NFP), and the benefit scheme of cohort clients (UI or DI). The share of cohorts with DI recipients ranged from 51 to 67% over the years. NFP job service providers covered 3 to 7% of the contracted cohorts in 2002–2004, whereas their market share is negligible for 2005. Although the market share of NFPs is small, we argue that the absolute number of contracts (55) as well as the number of clients in these contracts (5,532) is sufficiently large to obtain representative estimation results (see Table 2). Furthermore, there is a sharp drop in the average cohort size in 2005, both in the number of clients that are ex ante assigned to job training service providers (i.e. the 'gross' cohort size) and the number of clients that have effectively started their trajectory (i.e. the 'net' or ex post cohort size).

Basically, the social benefit administration assesses the performance of job training service providers on two outcome measures: provider induced selection and job placement rates. Regarding the first outcome measure, job training service providers may send back some clients if they consider these not to be 'suitable for the program'. We define the fraction of these clients of the total contracted cohort as *provider induced selection*. Obviously, high provider

3 As we will estimate model versions with cohort type fixed effects and we have new cohort types for separate years in our sample, any underreporting errors for job placement rates in 2005 will be controlled for. We return to this issue in the empirical implementation of our model.

TABLE 2 – COMPARISON BETWEEN FPs AND NFPs (2002–2005): COHORT CHARACTERISTICS AND PERFORMANCE

	For profit	Non profit
No. of procured cohort contracts	1,359	58
No. of job training service providers	92	15
No. of clients (gross)	278,037	5,532
Cohort characteristics		
Fraction DI	0.58	0.72***
	(0.013)	(0.059)
Cohort size (gross)	205	95***
	(5.7)	(12.4)
Cohort size (net)	169	78***
	(4.8)	(10)
Performance indicators		
Selection, provider induced	0.021	0.010***
	(0.00072)	(0.0020)
Selection, client induced	0.17	0.18
	(0.0027)	(0.015)
Fraction employed after completion program	0.24	0.28
	(0.0038)	(0.025)
Job contract ≤ 12 months	0.18	0.20
	(0.0031)	(0.019)
Job contract > 12 months	0.058	0.084**
	(0.0012)	(0.012)
Trajectory duration (days)		
Successful	220	230
	(2.8)	(19)
Unsuccessful	411	427
	(3.9)	(23)

Significance of any differentials between the sample averages of FPs and NFPs is denoted by ** and *** at 5% and 1%, respectively.

induced selection is not in the interest of the social benefit administration, as these are associated with cream skimming. Therefore, high provider induced selection rates diminish the prospects of future contracts.[4] Next to provider induced selection, we define *client induced selection* as clients that effectively do not participate in programs, but which are not sent back by the service provider. This group consists of clients that either already have found a job by the time the trajectory starts, or for whom the reintegration plans have not been approved by the social benefit administration anyway. In both cases, clients are no longer assigned to their respective job training service provider,

4 In the contracting process, past provider induced selection is one of the criteria for the awarding process, but not in an explicit fashion – for instance by setting maximum standards.

and this does not affect the future prospects of contracts. Unfortunately, there is no information on the size of these two categories.

Over the years, it seems there has been a downward trend in provider induced selection, from 3% in 2002 to only 1.3% in 2005. This contrasts with the evolution of client induced selection, which has risen from 13% in 2002 to 25% in 2005. Job placement rates have decreased over the years, presumably as a result of the economic downturn in 2003 and 2004. Again, however, it should be noted that contracts in 2005 are incomplete, causing underreporting errors here.

Table 2 shows average values of our sample of cohort contracts, stratified according to FP and NFP job training service providers. Only about 4% of our data consists of observations of NFP providers, which predominantly serve smaller cohorts with DI recipients.[5] In sum, 15 NFP job training service providers were awarded with contracts in 2002–2005, serving 55 individual cohorts and 25 cohort types. As four of these cohort types were served by NFPs only, we have 21 cohort types that can be characterised as 'mixed', in the sense that both organisation types were awarded with contracts here. Table 2 also shows that provider induced selection is significantly lower for NFPs than for FPs, whereas the job placement rate is only higher for NFPs for durable job contracts (i.e. at least a contract for 12 months). It is however unclear whether these differentials are related to job service training providers, or the (ex ante) selection of cohort client characteristics. For this, a more formal research design is needed.

3 EMPIRICAL STRATEGY AND ESTIMATION RESULTS

3.1 *Empirical Strategy*

Within the context of the bidding process, job service training providers may bid for cohort types that they consider most profitable. This means that there is room for selection *between* cohort types. As providers have no additional information on the composition of individual cohorts for each cohort type during the bidding process and there is no informational advantage vis-à-vis the social benefit administration, there is however no room for selection effects *within* contract types. Thus, ex ante selection effects of job service training providers (i.e. before the start of training program) can be effectively controlled for by the inclusion of cohort type fixed effects. We can identify the effect of legal status on performance using variation within cohort types. As

5 When applying a simple regression of the contracts share of NFP job training service providers on cohort type characteristics, we find cohort types with DI recipients and with smaller size to have significant coefficient values. No significant effects are found when including the average job placement probability of the cohort type as an explanatory variable. Thus NFPs seem to specialise in DI clients and smaller cohort groups, rather than those with bad job prospects.

performance measures are fractions in all cases, we thus start by formulating a linear probability panel model with cohort types as relevant units:

$$P_{ijtr} = \boldsymbol{\alpha} X_{ijtr} + \boldsymbol{\beta} Y_{it} + \gamma_{it} + \varepsilon_{ijtr} \tag{1}$$

with $i = 1 \ldots I$; $j = 1 \ldots J$; $t = 1 \ldots T$ and $r = 1 \ldots R$.

In the above equation, we define P_{ijtr} as the performance outcome for a contracted cohort type i with job service training provider j, measured at time t (in years) in region r. Performance can either be measured in terms of provider or client induced selection (Section 3.2) or job placement rates (Section 3.3). Matrix $\boldsymbol{X}$ consists of explanatory variables that vary over the contracts in our sample. These include the legal status of contracted providers, six regional dummies, and the log values of cohort size. Matrix $\boldsymbol{Y}$ includes variables that only vary across cohort types in our sample. These are the type of benefit recipients in the cohort (UI or DI) and the years that the contracts start (2002–2005). $\boldsymbol{\beta}$ denotes the impact of these variables. Finally, γ_{it} represents the cohort fixed effect and ε_{ijtr} a residual term that is i.i.d. with an expected value of zero and variance σ^2.

When using standard FE estimation techniques for equation (1), not all coefficients describing the effects of our parameters can be estimated. As variables in $\boldsymbol{Y}$ do not vary within cohort types, we cannot separately identify $\boldsymbol{\beta}$ and γ_{it}. We therefore introduce the parameter vector δ and define it as

$$\delta_{it} = \boldsymbol{\beta} Y_{it} + \gamma_{it} \tag{2}$$

so that equation (1) can be rewritten as

$$P_{ijtr} = \boldsymbol{\alpha} X_{ijtr} + \delta_{it} + \varepsilon_{ijtr} \tag{3}$$

and estimated with FE, using cohort types as fixed effects. Thus, we obtain consistent estimates of $\boldsymbol{\alpha}$, but the value estimates of $\boldsymbol{\delta}$ cannot be interpreted as cohort type effects in a narrow sense.

The use of FE estimation techniques provides us with a consistent coefficient value of the NFP-dummy. Still, we should be aware that in this design all contracts are weighed equally, regardless their size. Individual clients have a higher probability to be part of contract observations with large cohorts than for smaller ones, so equal weights for all observations may be misleading. As a check on the robustness of our results, we therefore will also estimate equation (1), but now using cohort size of the contracts as relative (analytic) weights. Moreover, rescaling the data according to cohort size may also increase the efficiency of our estimates. Within the context of FE estimation, however, rescaling is cumbersome, since observed weights are not constant within cohort contracts. We therefore follow Mundlak (1978), who

proposes a specification for fixed effects with pooled data. In particular, we re-specify the cohort specific effect as

$$\delta_{it}^{*} = \boldsymbol{\alpha}^{*}\ \overline{X}_{it} + \upsilon_{it} \tag{4}$$

where $\overline{X}_{it}$ are the average values of observed contract characteristics (i.e. the legal status, regional dummies, and cohort size) within cohort i at time t. υ_{it} is a residual term that is i.i.d. with an expected value of zero and variance τ^2. Essentially, in equation (4) we include variable averages over the observations within the cohort type sample, so as to estimate the parameter values $\boldsymbol{\alpha}$ using within variation only. As Mundlak (1978) shows, using this approach on pooled data yields estimates that are equivalent to those with FE estimation – at least for those variables with variation within cohort types. In the Mundlak model, we can also include $\boldsymbol{Y}_{it}$: its corresponding parameter is estimated consistently under the assumption that $\boldsymbol{Y}_{it}$ is not correlated with the cohort type individual effect υ_{it}. Thus, we estimate the following equation:

$$P_{ijtr} = \boldsymbol{\alpha} X_{ijtr} + \boldsymbol{\beta} Y_{it} + \boldsymbol{\alpha}^{*}\ \overline{X}_{it} + \phi_{ijtr} \tag{5}$$

with

$$\phi_{ijtr} = \upsilon_{it} + \varepsilon_{ijtr} \tag{6}$$

where ϕ_{ijtr} is i.i.d. with a mean value of zero and a variance equal to $\sigma^2 + \tau^2$. This equation can be estimated by Generalised Least Squares (GLS), allowing for cluster specific variation and cohort size as relative ('analytic') weights. Thus, we obtain consistent estimates of our parameters of interest with different relative weights, but still exploiting the panel aspect of the data. In the next subsection, when estimating the model for the service provider selection and the job placement rates, we will therefore present both the FE and Mundlak estimates. Note that we allow for clustering effects within cohort types for both these specifications.

3.2 *Estimation Results: Selection*

Table 3 presents the estimation results for both the FE and the (weighed) Mundlak model – i.e. equations (3) and (5) – for provider induced selection, client induced selection, and the total (sum of provider and client induced) selection of clients at the start of a program, measured over the time period 2002–2005.[6] We first address the model estimates for provider induced selec-

6 We already argued in the previous section that job placement observations of 2005 contracts are susceptive to underreporting bias, as the time frame of individual programs was not always complete by the time the data were registered (in 2006). Therefore, we also estimated all model forms of Sections 3.2 and 3.3 without 2005 as a robustness check. This yielded estimation results that were virtually equivalent for the complete time span for all relevant parameter values.

TABLE 3 – ESTIMATION RESULTS: PROVIDER AND CLIENT INDUCED SELECTION MODEL (2002–2005), $N = 1{,}417$

	Provider induced selection		Client induced selection		Overall	
	FE	Mundlak	FE	Mundlak	FE	Mundlak
Non-profit (dummy)	−0.0077***	−0.0063**	0.039***	0.044***	0.031**	0.038**
	(0.0029)	(0.0025)	(0.014)	(0.016)	(0.016)	(0.017)
DI cohort (dummy)		0.0066***		−0.0014		0.0052
		(0.0018)		(0.011)		(0.012)
Cohort size, log	0.0011	−0.0019	0.048*	−0.035	0.050**	−0.037
	(0.0038)	(0.0063)	(0.021)	(0.035)	(0.022)	(0.038)
Cohort size, log squared	−0.00061	−0.00023	−0.0066***	0.0025	−0.0072***	0.0023
	(0.00039)	(0.00065)	(0.0023)	(0.0035)	(0.0024)	(0.0038)
F-test regional dummies	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
Constant	0.031***	0.027*	0.12**	0.48***	0.15***	0.50***
	(0.011)	(0.015)	(0.046)	(0.095)	(0.048)	(0.10)
Year = 2003		−0.0065***		0.034***		0.028*
		(0.0022)		(0.012)		(0.014)
Year = 2004		−0.014***		0.061***		0.047***
		(0.0024)		(0.013)		(0.014)
Year = 2005		−0.017***		0.11***		0.097***
		(0.0023)		(0.012)		(0.012)
Average values (per cohort)						
Non-profit		0.0031		−0.080**		−0.076*
		(0.0084)		(0.036)		(0.040)
Cohort size, log		−0.00065		−0.10***		−0.10****
		(0.0059)		(0.030)		(0.032)
Cohort size, log squared		0.00051		0.011***		0.011***
		(0.00064)		(0.0032)		(0.0034)
F-test regional dummies (averages)		$p = 0.094$		$p = 0.524$		$p = 0.485$
F-test value averages		$p = 0.000$		$p = 0.017$		$p = 0.012$
R-squared		25.9%		29.1%		23.2%
% variance due to FE	28.9%		41.6%		38.4%	

Significance of parameter estimates is denoted by *, **, and *** at 10%, 5% and 1%, respectively. Standard errors between parentheses

tion. Providers may take advantage of the opportunity to send back clients – at least to a certain extent – by excluding clients with limited job prospects only that are less profitable in terms of payments per job placement. As the table shows, for both the FE and the Mundlak specification we find FP service providers indeed to show (significantly) higher provider induced

selection, with estimated differentials ranging from 0.6 to 0.8%-point (with an average rate equal to 2% in the total sample).

More strikingly, we find reverse (and positive) effects of the NFP status for client induced selection. In particular, the fraction of clients that choose not to participate in the programs is 4 to 4.5%-point higher for NFP job training service providers. This effect dominates provider induced selection, causing the total fraction of clients at the start of the program to be even lower for NFPs than for FPs as well. This finding is surprising, as the selection behaviour of clients is driven by the legal status of service providers. We therefore suspect FP service providers to affect selection in an indirect manner, namely by taking more effort in preventing clients not to show up at the start of a program, in particular by contacting them as early as possible and subsequently get the reintegration plans approved. In this respect, one should take in mind that providers are paid by the social benefit administration per individual client, both at the start of a program and the moment of job placement. This contract design creates incentives to prevent client induced selection by the job training service provider. FPs may be more susceptible to this, particularly when additional clients also have good job prospects and therefore the probability of job placement rewards is high as well.

One interpretation of the results on provider and client induced selection may be that these are substitutes – i.e. a higher proportion of approved reintegration plans results in a higher return rate that is induced by the providers. When looking at the correlation between client and provider induced selection, however, there is no evidence pointing in this direction. In particular, the observed correlation is insignificant and equal to 0.039 (0.027), whereas the correlation that is observed within cohort types and regions is 0.048 (0.027). From this, we conclude there is no strong evidence for substitution effects.[7]

Regarding the other parameters in our model, we find the yearly and regional dummies to be significant. Over time, the fraction of provider induced selection has decreased substantially, whereas the opposite has occurred for client induced selection. One explanation for this may be that business cycle conditions have improved in the time period under investigation, causing the number of clients finding a job in the time span before the program to have increased as well. Furthermore, for the impact of cohort size on client induced selection, we find clear differences between the FE and Mundlak specification. This well may reflect the fact that the FE estimates are not heteroscedasticy consistent. In particular, the cohort size of the contracts is the prime cause of heteroscedasticity. Thus, the coefficient value of this variable is likely to be susceptible to heteroscedasticity bias. Finally, we

7 It may also be that substitution effects do exist, but complementary effects are of equal size. For instance, client cohorts that are harder to serve may both have less reintegration plans approved and be more likely to be send back by the provider.

find evidence for the hypothesis that cohort type specific effects are correlated with the explanatory variables ($\boldsymbol{X}$) in our model that vary within cohort types. In particular, F-tests on the significance of the parameter estimates of the value averages range from 0 to 1.7%. Thus, controlling for cohort type specific effects – either by the FE or the Mundlak specification – is necessary to obtain consistent estimates.

3.3 *Estimation Results: Job Placement Rates*

So far, our estimates suggest that NFP and FP job training service providers differ in their selection behaviour at the start of the programs they provide. First, FPs are more likely to apply provider induced selection. Second, FPs provide more effort to prevent clients not to show up at the start of a programs and have the reintegration plans approved, so as to ensure (at least) a fixed payment per client, and to maintain the prospect of future payments in case of job placement. In order to test for the possible impact of these selection effects on the job placement rates, we now extend equations (3) and (5) with both provider and client induced selection as explanatory variables. Table 4 presents the resulting coefficient estimates that follow from such an approach, again both for the FE and the Mundlak specification.

From the table, it seems that NFP job training service providers only make a (small) difference in (durable) job placements with contracts that last at least 12 months. Here, NFPs have a job placement rate in this period that is 1.6 to 1.8%-point higher than FPs. For short term contracts and the total job placement rate, however, no significant differences are found – at least not for the Mundlak specification. Thus, although there is weak evidence that NFPs have a better performance in the long run, this effect is too small to lead to an overall better performance.

The estimation results for the job placement model also show that the FE specification gives a more pronounced effect for the NFP status than the Mundlak specification does, with cohort sizes as relative weights. This particularly holds for the overall job placement rate differential, which is significant for the FE model (with 2.6%-point), but smaller and insignificant for the Mundlak model (0.54%-point). A possible explanation for this difference is that performance differentials are larger for small cohorts – i.e. NFPs have a comparative advantage for small cohorts only. Thus, putting more weight on the large cohort decreases the overall differential. We return to this issue in the next subsection.

We find limited evidence for client induced selection to affect job placement rates. In particular, only for the Mundlak specification client induced

TABLE 4 – ESTIMATION RESULTS FOR JOB PLACEMENT RATE MODEL, 2002–2005: OVERALL, SHORT AND LONG TERM CONTRACTS

	Outflow to job contracts ≤12 months		Outflow to job contracts >12 months		Overall outflow	
	FE	Mundlak	FE	Mundlak	FE	Mundlak
Non-profit	0.0084	−0.010	0.018	0.016**	0.026*	0.0054
	(0.016)	(0.017)	(0.012)	(0.0070)	(0.014)	(0.017)
DI cohort (dummy)		−0.066***		−0.0046		−0.071***
		(0.020)		(0.0063)		(0.025)
Cohort size, log	0.038**	0.0010	0.021***	0.010	0.059***	0.011
	(0.016)	(0.026)	(0.0063)	(0.011)	(0.021)	(0.033)
Cohort size, log squared	−0.0035**	0.00055	−0.0021***	−0.0010	−0.0057**	−0.00047
	(0.0018)	(0.0027)	(0.00071)	(0.0010)	(0.0022)	(0.0033)
Provider induced selection	−0.034	−0.16	−0.035	−0.052	−0.069	−0.21
	(0.10)	(0.10)	(0.053)	(0.054)	(0.12)	(0.13)
Client induced selection	−0.064**	0.025	−0.0053	0.0052	−0.070*	0.030
	(0.031)	(0.035)	(0.013)	(0.013)	(0.036)	(0.041)
F-test regional dummies	$p=0.017$	$p=0.005$	$p=0.030$	$p=0.000$	$p=0.008$	$p=0.004$
F-test: client = provider i.s.	$p=0.782$	$p=0.077$	$p=0.592$	$p=0.337$	$p=0.995$	$p=0.067$
Constant	0.090**	0.28	0.010	0.030	0.10**	0.31
	(0.039)	(0.19)	(0.015)	(0.074)	(0.049)	(0.24)
Year = 2003		0.018		0.0085		0.027
		(0.033)		(0.0092)		(0.040)
Year = 2004		−0.0051		0.0044		−0.00074
		(0.062)		(0.018)		(0.078)
Year = 2005		−0.12*		−0.037**		−0.16*
		(0.062)		(0.018)		(0.076)
Average values (per cohort)						
Non-profit		0.025		−0.010		0.014
		(0.092)		(0.030)		(0.12)
Cohort size, log		−0.071		0.00043		−0.070
		(0.068)		(0.030)		(0.085)
Cohort size, log squared		0.0088		0.00042		0.0088
		(0.0074)		(0.0032)		(0.0093)
Provider induced selection		4.0***		0.48		4.4**
		(1.4)		(0.37)		(1.7)
Client induced selection		0.13		0.053		0.19
		(0.39)		(0.11)		(0.49)

TABLE 4 – continued

	Outflow to job contracts ≤12 months		Outflow to job contracts >12 months		Overall outflow	
	FE	Mundlak	FE	Mundlak	FE	Mundlak
F-test regional dummies		$p=0.000$		$p=0.001$		$p=0.000$
F-test value averages		$p=0.000$		$p=0.008$		$p=0.000$
F-test: client = provider induced selection		$p=0.020$		$p=0.331$		$p=0.033$
R-squared		50.4%		20.7%		47.7%
% variance due to FE	69.5%		44.9%		70.5%	

Significance of parameter estimates is denoted by *, **, and *** at 10%, 5% and 1%, respectively. Standard errors between parentheses.

selection has a negative and significant impact on short term job placement.[8] Similar to the selection model, we also find a hump shaped cohort size effect for the FE specification, but no significant effects when using the Mundlak specification. Again, these differences can be explained by the use of cohort size as relative weights. Next, we find regional dummies to increase the fit of our model, while there is no clear pattern emerging from the yearly dummies. Finally, and similar to the selection model, the inclusion of value averages for $\boldsymbol{X}$ in the Mundlak specification increases the fit of the model significantly for all job placement rates, indicating that controlling for cohort type effects is necessary for consistent estimates.

In order to get more insight in the origins of the obtained performance differentials – both in terms of selection and job placement – we re-estimated the Mundlak model for all outcomes of interest, while stratifying the observations according to the benefit scheme of the cohort (UI or DI) and cohort size (smaller or larger than 100 clients) on the other hand.[9] Table 5 presents the resulting coefficient estimates for the interaction terms for the job placement models. Overall, the table shows no evidence for the NFP–FP differ-

8 We also estimated the model without the selection measures as controls. This yielded similar estimates for our parameters of interest.

9 Note that we use the Mundlak specification here. Splitting up the sample in small and large cohorts would yield substantially less observations per cohort type, thus decreasing the efficiency of our estimates. In a Mundlak specfication, however, by assuming that fixed effects do not differ within cohort types (small or large), we can easily exploit the panel aspect of the data by the inclusion of averages that are partially based on larger cohorts as well.

TABLE 5 – ESTIMATED NFP–FP DIFFERENTIAL FOR MUNDLAK MODEL, STRATIFIED ACCORDING TO BENEFIT SCHEME (DI/UI) AND COHORT SIZE (</> 100)

	Benefit scheme		Cohort size		Overall
	UI	DI	<100	>99	
Provider induced selection	0.0026	−0.0076***	−0.0054	−0.0074**	−0.0063*
	(0.0035)	(0.0028)	(0.0032)	(0.0032)	(0.0025)
Client induced selection	0.021**	0.063***	0.038**	0.044**	0.044***
	(0.0078)	(0.015)	(0.018)	(0.021)	(0.016)
Overall selection	0.023**	0.055***	0.033**	0.037*	0.038**
	(0.010)	(0.017)	(0.017)	(0.022)	(0.017)
Job contracts ≤ 12 months	−0.031	−0.010	−0.0058	−0.015	0.010
	(0.028)	(0.020)	(0.019)	(0.027)	(0.017)
Job contracts > 12 months	0.011	0.017**	0.0065	0.015	0.016**
	(0.016)	(0.0075)	(0.015)	(0.011)	(0.0071)
Overall job placement	−0.019	0.0070	0.00073	−0.00053	0.0054
	(0.037)	(0.018)	(0.019)	(0.030)	(0.017)

Significance of parameter estimates is denoted by *, **, and *** at 10%, 5% and 1% respectively

ential in job placement rates to be confined to a particular contract type. It is only for the differentials in the selection outcomes that we find more pronounced results for DI cohorts than for those with UI recipients.[10] One explanation for this result may be that fixed payments per client are higher for DI recipients, thus creating a larger incentive to bring in these clients at the start of a program. FPs in turn may be more susceptive to this incentive, thus increasing both provider and client induced selection for DI recipients.

4 CONCLUSIONS AND DISCUSSION

This paper analyses the relative performance and selection behaviour of NFP job training service providers, using unique contract data from the Dutch social benefit administration. In our analysis, we take full account of selection effects, both ex ante (i.e. during the contracting process) as well as ex post (i.e. at the start of a program). First, given the setup of the contracting system, cohort types in our data are ex ante homogeneous – with job service training providers only being informed on the average characteristics of the

10 Note that the sample averages of the selection measures are of about equal size for the UI and DI cohorts. In particular, the averages for provider and client induced selection are 1.6% and 16.7% for UI cohorts, and 2.3% and 17.6% for DI cohorts.

cohort type. Thus, there is no room for asymmetric information and selection effects of individual cohorts within cohort types. Moreover, the *within* cohort type variation of individual cohorts in terms of performance outcomes of NFP and FP job training service providers is sufficient to obtain consistent estimates of performance differentials. Second, (ex post) selection effects at the start of the program is measured explicitly in our analysis. As contracted cohorts are assigned to a specific job training service provider, clients that effectively do not participate in the programs are labelled as selection effects (or: dropouts) with respect to clients of a particular service provider. These effects can be attributed to the job training provider ('provider induced selection') or individual clients not showing up ('client induced selection').

Our estimation results show that FP are more active in selecting clients, both by sending back more of them (provider induced selection) and indirectly, by stimulating clients to start a program (client induced selection). The higher extent of provider induced selection of FPs indicates that they take more advantage of the opportunity to send back clients by excluding clients with bad job prospects. This effect is however dominated by the lower rate of client induced selection of FPs, causing the fraction of effective participants at the start of the program to be lower for NFPs than for FPs. Thus it may well be that FPs are more susceptive to the reward incentive per individual client at the start of a program.

Regarding the performance of providers in job placement rates, there is only weak evidence for NFP job training service providers to make a difference in the durability of jobs that are found after completion of the program. In particular, we find NFPs to have a job placement rate for job contracts of at least 12 months to be 1.6 to 1.8%-point higher than for FPs. For both the job placement rate for short term contracts and the total job placement rate, however, no significant and systematic differences are found.

We conclude that FP and NFP job training service providers have different performance outcomes, particularly regarding their selection behaviour. For the social benefit administration, however, the conversion from these findings to policy implications is complex. If the contractor aims at maximising the fraction of clients that participates in programs as an equity device, FPs are to be preferred, as the overall fraction of assigned clients that participates is higher than for NFPs. From an efficiency point of view, however, maximising the number of clients that effectively participates may not be a wise thing to do. Additional clients may be less motivated to participate or would have found a job anyhow. Our results indicate some evidence in this direction – that is, clients not showing up at the start of a program have higher re-employment rates than participants. Given the current setup of our analysis, however, we cannot infer whether this finding reflects higher *a priori* chances to find a job, or whether programs lower the probability to find a job. We leave this for future research.

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