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Do Public Employment Services improve employment outcomes?

Evidence from Colombia

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Abstract

The paper conducts an impact evaluation of the Public Employment Service (PES) in Colombia by means of propensity score matching. The results show that participating in the PES increases the probability of having a formal job. Around two thirds of this effect is related to the fact that PES participants are placed in larger companies. By contrast, participation in the PES has a negative effect on wages. This comes from a positive effect on the wages of the low skilled and a negative effect on the wages of the high skilled. For both formal employment and wages, the PES is more effective when the services are provided face-to-face – rather than online. A large set of robustness tests confirms the validity of the methodology used and the robustness of the results obtained.

Keywords: Public employment services; informality; wages; propensity score

JEL codes: J21, J23, J46, J48

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1. Introduction

Active labour market policies (ALMPs) have gained increasing importance in Latin America and the Caribbean (LAC) since the beginning of the 2000s as helpful policy instruments to sustain productive employment. This reflects a policy shift by governments in the region to complement traditional interventions aimed at poverty reduction (such as conditional cash transfers, CCTs), with policies targeted at increasing the employability of the labour force. As a result, a variety of ALMPs has emerged in the region that does not strictly reflect the experience of developed economies. Indeed, ALMPs in LAC tend to have a generally broader focus (i.e. combine together different interventions such as training and public works) and a wider target group (i.e. eligibility requirements are rather low) (ILO, 2016). Colombia represents a paradigmatic example of this policy approach and recent evolution. Indeed, public spending on ALMPs has increased from 0.001 to 0.317 per cent of GDP between 2000 and 2010. At this level, spending on active interventions in Colombia is comparable with spending on CCTs (0.347 per cent of GDP) and is more than ten times higher than spending on unemployment benefits (Cerutti et al., 2014). Public expenditure on ALMPs is still lower in Colombia than in Argentina, Brazil and Chile (all countries with higher levels of GDP per capita); but it is higher than in any other country in the region with available information. The bulk of public expenditure in Colombia is devoted to training (86.9 per cent), followed by expenditure on labour market services (10.8 per cent), start-up incentives (2.2 per cent) and public works schemes (0.1 per cent).

Training and labour market services thus represent the two major areas of spending for ALMPs in Colombia – with expenditures on the two types of interventions as a share of GDP being the highest in LAC (Cerutti et al., 2014). The *Servicio Nacional de Aprendizaje* (SENA) is the public organization in charge of providing both vocational training (since 1957) and public labour market services (since 1989). This is provided by the SENA through two different institutions that can be accessed independently, the *Sistema Nacional de Formación para el Trabajo* (SNPT) for training and the *Agencia Pública de Empleo* (APE) that represents the Colombian Public Employment Service (PES). The impact of SENA training courses has been extensively evaluated since the 1970s, generally finding only minimal effects on earnings and employment for participants – see the seminal paper by Puryear (1977) and studies by Gómez and Libreros (1984), Jimenez and Kugler (1987), Jimenez et al. (1989), López (1994a; b), and more recently Gaviria and Núñez (2003) and Medina and Núñez (2005). However, the effects of participation in the Colombian PES have not yet been investigated; leaving unanswered questions about the effectiveness of labour market services in the country. This paper aims to fill this gap by estimating the effects of participation in APE. In this way, the analysis also contributes to the wider debate on the effectiveness of PESs in developing economies.

Indeed a number of papers have assessed the effectiveness of PESs in developed economies, generally finding positive results in improving participants' (short-term) labour market outcomes (Card et al., 2015 for the results from a meta-analysis). However, these results and the related policy implications cannot be easily extended to developing economies. This concerns both structural differences in the functioning of the labour markets (e.g. high share of informality, lower incidence of long-term unemployment) as well as differences in the structure and scope of ALMPs between advanced and developing economies (Auer et al., 2008; ILO, 2016). Looking at LAC in particular, only four evaluations of PESs have been conducted and their results reveal a rather mixed picture. Chacaltana and Sulmont (2003) find indeed a positive effect of the PES in Peru on both employment chances and wages; while Vera (2013) finds that participation in the PES in Peru increases unemployment spells. A study in Brazil reports no significant effects of programme participation on employment, but a positive effect on the probability of being in a formal job (FIPE/USP-IPEA, 2000). Similarly, Flores Lima (2010) finds no significant effects of the PES on the probability of finding a job in Mexico; but a positive effect on earnings and formality.[1]

Colombia represents an extremely interesting case to examine the effects of labour market services. Indeed, the share of informal employment is still considerably above the average for LAC (54.4 per cent of non-agricultural employment compared to a regional value of 46.8) and has only marginally decreased over the past decade despite sustained economic growth (it was at 57.6 per cent in 2004) (ILO, 2014). Moreover, research has reported a high degree of labour market segmentation with low transition rates between informal and formal employment (Mondragón-Vélez et al., 2010; Peña, 2013); while evidence from longitudinal data shows that informal jobs often represent the first step for those entering the Colombian labour market and that they are associated with lower wages and an higher risk of unemployment recurrence (ILO, 2016; OECD, 2016). Moreover, the incidence of long-term unemployment is relatively low in the country (5 per cent of total unemployment in 2013, compared to an OECD average of 35 per cent); while the job turnover rate is extremely high (average job tenure of 6.4 years in 2013, compared to an OECD average of 10.1). In this context, PESs can have a potentially important role in breaking informality traps and lead to a more efficient allocation of labour. However, the PES in Colombia is used only by a small minority of the labour force – accounting for just one per cent of the job matches taking place every year. This badly compares with the results of developed economies (e.g. 9.6 per cent of job matches occurred through PESs in the European Union in 2012); but also with data from other countries in LAC (e.g. 3.8 per cent of job matches in Brazil occurs though the PES). In order to tackle these issues, the Colombian Government has implemented in 2013 a reform of the PES with the aim of increasing the reach of labour market services in the country by fostering the collaboration between public and private providers of labour intermediation.

Using propensity score matching (PSM), this paper compares the employment outcomes of individuals that found a job through the PES with those of comparable non-participants that found their job through alternative job-search methods. The data used in the analysis comes from the Colombian household survey *Gran Encuesta Integrada de Hogares* (GEIH), which contains a wealth of information on household and personal characteristics as well as both previous and current employment status. In this context, PSM can represent an extremely valid instrument to determine treatment effects – which has been extensively used in the area of PESs. Due to data limitations (to be discussed below) the analysis cannot directly

[1] However, none of these studies meets the requirements to enter the meta-analysis by Card et al. (2015).

investigate the effects of programme participation on the probability of finding a job; but only on the quality of the job found – defined in this case as the formal nature of the job and wages. However, employment quality is an extremely useful outcome of interest in the evaluation of PESs and has been used by a number of previous studies (for example Blundell et al., 2004; Crépon et al., 2013). Indeed, the quality of the job found influences the probability that the job-seeker will return to unemployment (and therefore to social assistance and potentially to the PES, all expensive services for public finances). As a result, the attention of policy makers has increasingly shifted towards enhancing PESs' effectiveness in adequately placing job-seekers in quality jobs – rather than simply doing it rapidly (OECD, 2015). These considerations are particularly relevant in Colombia, given the labour market challenges mentioned above (e.g. high turnover rates).

The results of the analysis show that participating in the PES in Colombia has a positive effect on the probability of having a formal (rather than informal) job. Around two thirds of this effect is related to the PES's capacity of placing job-seekers in larger companies; which research has shown being characterised by a higher degree of labour law compliance (Almeida and Carneiro, 2012). By contrast, finding a job through the PES in Colombia has a negative effect on earnings – which is also confirmed by a decomposition of the wage gap into observed and unobserved factors. This derives from a positive effect on the wages of the low-skilled and a negative effect on the wages of the high-skilled. Finally, the results show that the Colombian PES is more effective when the services are provided face-to-face (i.e. in PES centres) rather than online. In particular, the positive effect of the PES on formality disappears when considering online matches only; while the negative effect on wages is generally non-statistically significant (or of reduced magnitude) when restricting the sample to face-to-face matches. Overall, these results point towards the capacity of the PES in lifting the employment opportunities of the lower segment of the labour force (e.g. though labour orientation or small scale human capital enhancement); while remaining ineffective in placing high-skilled job-seekers in productive employment. Additionally, the results point towards the importance of the channel of services' provision (i.e. face-to-face versus online) in determining the effectiveness of the PES.

The rest of the paper is organized as follows: section 2 describes the policy to be evaluated and the data with selected descriptive statistics; section 3 introduces the general theoretical framework for microeconometric evaluation and the identifying assumptions needed; section 4 controls the plausibility of these assumptions and applies PSM in the specific context, section 5 presents the results of the analysis, section 6 concludes.

2. The programme and data

2.1 PES in Colombia: The *Agencia Pública de Empleo* (APE)

Labour market services originally played a limited role in Colombia, with labour inspectors that were in charge of visiting selected enterprises with the aim of finding possible employment opportunities for job-seekers that had registered their availability at the Ministry of Labour. With the ratification of the ILO Convention No. 88 of 1948 (Decree 37 of 1967), the Government increased its commitment to provide labour market services nationwide. This was initially done through the activities of the *Dirección General del Servicio Nacional de Empleo* (SENALDE); while from 1989 the responsibility in the area of labour market services was assigned to the *Servicio Nacional de Aprendizaje* (SENA). SENA is a public institution depending from the Ministry of Labour that since 1957 was already in charge of providing public vocational training in the country – which remains its principal mandate, both in terms of public spending and coverage. The role of SENA in the area of labour intermediation was initially limited to collecting information about the demand and the supply of labour. Decree 249 of 2004 expanded the competencies of SENA in the area of PESs, adding the responsibilities of job-search assistance, counselling and placement. Since 2006, software has been introduced and labour intermediation can occur either on-line or face-to-face with councillors. A reform in 2013 has instituted a new agency in charge of the public provision of labour market services within SENA (the *Agencia Pública de Empleo*, APE) to join the newly constituted network of public and private providers of labour intermediation (the *Servicio Público de Empleo*, SPE).[2] With this reform, the Government has aimed at expanding the coverage of labour market services in the country by fostering collaboration between public and private providers.

In order to proceed with the registration in the system of the PES (either on-line or in APE centres), the job-seeker is asked to enter identification and contact details, information on educational attainments, training programmes completed, previous work experiences as well as the professional competencies and preferences for the new job (including location). After registration takes place, the system automatically generates the CV of the job-seeker and produces a certificate of registration into the PES. The job-seeker can then directly apply online to the vacancies that match with his/her profile and/or seek advice from APE centres in order to start an individualised path. In the first case, the software automatically lists all the vacancies whose requirements are met by the job-seeker. The job-seeker can consult the vacancy notice (including the number of candidates that have already applied) and directly apply – with no need to provide any additional vacancy specific information. If the employer is interested in further continuing the selection process, the jobseeker will be contacted – while the contact details of the employer are not made publicly available. If instead the jobseeker chooses to receive individualised job-search assistance, he/she can visit APE centres. Upon the first meeting, APE staff distinguishes between: (i) those jobseekers that are employable and would only need some form of labour intermediation (e.g. CV counselling, vacancies' screening, interview); (ii) those that are not yet ready for (re-)entering the labour market and to whom APE staff provides more structured labour market orientation (e.g. career advice) and identifies possible training courses (also provided at SENA); and (iii) those that are willing to start their own business and for which APE provides entrepreneurial support. The profile of the jobseeker is cancelled from the registry if he/she fails to attend an interview that was made available through APE or if he/she does not attend a training course to which had registered.

[2] The paper will refer to the APE in defining the Colombian PES, even for the period before the 2013 reform.

Enterprises need to undergo a similar process for registering into the PES, specifying the main characteristics of the company (e.g. legal status), the areas of operation and the contact details. They can then post vacancies by detailing the professional and occupational status, the tasks required and the main characteristics of the job offer – following a pre-established form available in the software. Once registration has taken place, enterprises can decide to either wait for interested candidates to contact them (following the procedure described above) or alternatively to autonomously search for suitable profiles in the system. All this can be done either online via APE software or by visiting PES centres. In the latter case, APE staff – in accordance with the employer – consults the PES job bank to look for suitable candidates and in certain circumstances also conducts semi-structured interviews with interested applicants to assess their competencies. Based on this pre-screening exercise, APE staff compiles a first list of potential candidates that is then made available to the enterprise. However, employers can demand for the entire list of jobseekers that have applied to their vacancy. If a large number of vacancies is available in the same sector and/or region, even recruiting events can be organised by APE. Alternatively, employers can ask the availability of specific rooms in APE centres to conduct interviews (*microruedas*). In all the different cases, the enterprise should at the end notify APE – either through the software, via email or telephone – for each candidate that had applied though APE whether he/she was selected and (eventually) the reason for the rejection. Failure to comply with this reporting duty impedes the employer to post additional vacancies. Additionally, enterprises can be cancelled from the registry if they close three consecutive vacancies without having selected any candidate that had applied through APE. The purpose of this cancellation policy is to encourage employers to contact APE staff in order to better detail their job announcements, such that available candidates that are in the system can be matched. If this clarification takes place, APE staff can unblock the enterprise in the software.

There are no specific eligibility requirements for participating in the PES, as these services are open to everyone (e.g. unemployed, underemployed, employed, inactive) and free-of-charge. Moreover, unemployment benefits in Colombia are not connected with the PES and do not present any activation requirement whose fulfilment is mandatory for receiving the benefit – either connected to the PES or with any other institution. In particular, upon job loss individuals that earned less than four times the minimum wage are entitled to receive for a maximum of six months a family allowance whose amount is proportional to the number of dependents in the household. This represents a form of social assistance whose receipt is not conditional to actively looking for a job or participating in activation measures (e.g. training, the PES). However, this transfer is available only for formal workers whose employer contributed four per cent of their payroll to a family compensation fund (*Cajas de Compensación*) for at least 12 months in the three years before the job loss. As such, unemployment benefits' coverage is rather limited – either because most of job losses occur in the informal sector or because employers even in the formal sector do not regularly contribute to the *Cajas de Compensación*. In particular, it was estimated that in August 2014 only 0.5 per cent of the unemployed received this type of benefits (OECD, 2016). For the purpose of the analysis, this implies that there is no explicit connection between the PES and passive policies in Colombia and that PES participants are unlikely to receive any financial support during their unemployment spell – or as likely as other categories of job-seekers. Looking at the demand side, employers after the 2013 reform have the obligation to post their vacancies within the SPE – so that the vacancies that are available in APE software should be representative of all vacancies in the labour market.[3] However, this requirement has been only recently introduced and in practice legislation has not been enforced.

APE operates online or through the network of public centres present nationwide – 33 principal offices (32 in each district and an additional one in Bogota), 40 satellite offices and 4 mobile offices. In each centre, posts are available for job-seekers and employers for face-to-face counselling with PES staff. Ethnic minorities or individuals that have been victim of terrorism are assisted in different posts specifically targeted to them. Computers are also made available in the centres for job-seekers that independently want to update their profile in the software. Rooms are available in each APE centre for classroom teaching (e.g. foreign language) where job-seekers that have been assigned to training courses can participate. Data for 2014 shows that in the course of the year, 994,902 jobseekers had registered their profiles in APE to look for a job and 529,148 of them had approached APE centres to get individualised job-search assistance. At the same time, 261,357 vacancies had been published by enterprises in the system and as a result 180,081 job matches have occurred during the year (Government of Colombia, 2015). This means that 18.1 per cent of the jobseekers that used APE to look for a job were successful in their search; while 68.9 per cent of the vacancies posted in the system were filled. However, it is not possible to understand from this data whether the job match occurred trough APE or with alternative job-search methods that were used in parallel.

2.2 Dataset and descriptive statistics

The data used for the analysis comes from the household survey *Gran Encuesta Integrada de Hogares* (GEIH) conducted by the *Departamento Administrativo Nacional de Estadística* (DANE). The current version of the GEIH has become fully operational in 2007; when the sample size and coverage have been expanded (from 13 metropolitan areas to 24 metropolitan areas and all rural areas), electronic devices have been introduced for data collection and the scope of the analysis has been extended. In particular, the GEIH results from the integration of the previous Colombian household survey (the *Encuesta Continua de Hogares*) with another survey conducted by the DANE (the *Encuesta de Calidad de Vida*). The current version of the GEIH is composed of 15 permanent modules covering different demographic and socioeconomic aspects. This allows obtaining a wealth of information on individual and household characteristics as well as their labour market status. In particular, the labour market module asks employed individuals the mechanism used to find their current job (which will be used to define treatment and control groups), the main characteristic of their job (e.g. employment duration, occupation, sector of activity, earnings, hours worked, social security coverage) and some information on previous labour market history (e.g. duration of last unemployment spell, job tenure in previous job, previous occupation). The survey does not have a panel structure, but some longitudinal features can be partially

[3] A waiver is granted for vacancies related to directorial positions.

retrieved through the available information. The survey is composed of a two-stage stratified sample and it interviews every year around 250,000 households nationwide, representing the most extensive survey in the country. Given these features, the GEIH is the main data source used to compute economic and labour market indicators in Colombia (see for instance OECD, 2016 and ILO, 2014); while it has been already used in a number of econometric studies (Diaz, 2012 and Nicodemo and García, 2015 for some recent applications).

The paper focuses on employed individuals interviewed in the GEIH between 2008 and 2014 and defines treatment based on information on the job-search mechanism through which they found their current job. The treatment group corresponds to individuals that found a job through the PES and were still in that job at the time of interview. For this reason, the analysis cannot investigate the effect of participation on the probability of finding a job – but only the effects on the quality of the job found. In particular, the analysis investigates the effects of participation in the PES on current employment characteristics – as measured by the formal nature of their job and wage levels.[4] Since the survey does not have a panel structure and does not provide detailed information on previous job-search history (but only on the successful job-search method and the length of the unemployment duration), it is not possible to investigate the effects of participation on finding a job. For conducting this type of analysis, it should be assumed that jobseekers looked for a job for the entire duration of their unemployment spell and that they used only the successful job-search method continuously. This is however in contradiction with available evidence from Colombia, which shows how job-seekers often use different search methods simultaneously and that they tend to revert to formal channels (such as the PES) only after having unsuccessfully attempted other informal mechanisms (such as relatives and friends) (Uribe and Gomez, 2006; Uribe and Viáfara, 2009). Despite limiting the scope the analysis, restricting the treatment group to those that actually found a job through APE has some substantial methodological advantages. In particular, it limits the risk of considering as treated those individuals that are formally registered in the PES, but are not actively looking for a job. This is an issue that empirical studies have shown to weaken the estimation strategy of PESs' evaluations (Naticchioni and Loriga, 2010) and could be of particular relevance in Colombia, as PES participants do not have any sanction or incentive for looking for a job (e.g. losing the eligibility to unemployment benefits).

The control group corresponds to individuals that between 2008 and 2014 were in a job that they had found through an alternative channel (i.e. different from the PES). This includes the following job-search options in the GEIH: (i) posting or replying to a classified job advertisement (henceforth, "classified advertisements"); (ii) obtaining labour market services by private employment agencies ("private agencies"); (iii) directly contacting and/or visiting employers ("employers"); and (iv) enquiring relatives and friends ("relatives and friends").[5] Previous studies (whose results are largely confirmed from the descriptive statistics presented

[4] Definition of formality follows ILO guidelines and includes a number of characteristics such as social security coverage, presence of a formal contract and nature of the employment relation – thus representing a more fine grained definition than the one used in the majority of previous applications. See ILO (2014) for details.

[5] The analysis does not include the last option available in the GEIH, which refers to those that found a job through calls ("*para convocatoria*").

below) show that the majority of the job-seekers in Colombia use informal channels of job-search (i.e. relatives and friends and direct contact with employers); while only a minority reverts to formal channels (i.e. classified advertisements, private agencies and the PES). Moreover, those job-seekers that use informal channels have been found to be older, more likely to be men and generally less educated (Uribe and Gómez, 2006). Given the differences between these job-search methods, the analysis compares PES participants with these control groups separately. This represents a considerable improvement with respect to previous studies that have examined the effectiveness of job-search channels in Colombia by pooling together different formal and informal channels (as grouped above) without accounting for differences in their functioning and participants' self-selection into them (Nicodemo and Garcia, 2015; Diaz, 2012). Additionally, the present analysis eliminates individuals at their first job; for which there is no information on previous labour market experience – which is instead essential for the estimation of the propensity score (Heckman et al., 1999).

The final sample includes 5,809 treated and 779,626 control individuals – divided by category of job-search, with relatives and friends representing by far the largest group. Selected descriptive statistics show that participants are generally younger than non-participants. This is reflected also in their lower likelihood of being heads of households and the lower probability of being married. In terms of years of education, evidence confirms that better educated individuals generally use formal channels of job-search (or the direct contact with the employer); while low-skilled job-seekers tend to turn to relatives and friends when looking for a job. Importantly, PES participants with a tertiary degree are significantly more likely to have acquired technical (rather than academic) training. This can be explained by the fact that SENA is the main provider of vocational training in the country (as mentioned in the introduction) and SENA graduates might revert to APE upon completion of their training in order to look for a job. This might also explain the differences in average age between the control and treatment groups. No notable differences are instead found with respect to family or household characteristics – as measured for instance by the employment status of other members of the household or the characteristics of the house. Turning to previous labour market experience, individuals in the control group tend to have longer job tenures in their previous jobs and shorter unemployment spells than the treatment group – in both cases probably reflecting differences in ages. However, the occupational status in their previous position is similar between treatment and control groups (Table 1). Around two per cent of the sample is constituted by individuals that – independently from the job-search method used – have found their job online (rather than through face-to-face contact). This share varies from 36 per cent of those that used job announcements to less than one per cent of those that contacted relatives and friends for looking for a job (10 per cent of PES participants). Their descriptive statistics do not significantly vary compared to those that found a job though face-to-face contact (a part from the average age, which as expected is significantly lower for those that found a job online). The descriptive statistics of the two groups (on-line and face-to-face) by job-search method used are presented in Appendix A.

Table 1: Selected descriptive statistics of participants and non-participants

	PES		Classified Advertisements		Private Agencies		Employers		Relatives and Friends	
Number of observations	5,809		8,916		27,286		168,788		574,636	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
Personal characteristics										
Average age	29.46	8.45	32.68	9.18	32.86	9.23	35.87	10.39	36.58	12.02
Male	0.49	0.50	0.46	0.49	0.56	0.50	0.54	0.50	0.56	0.50
Average years of education	13.04	2.11	12.74	3.49	11.73	3.03	12.36	3.77	9.45	4.42
Vocational training	0.48	0.50	0.24	0.42	0.21	0.41	0.19	0.39	0.11	0.31
Married	0.19	0.39	0.21	0.41	0.22	0.41	0.29	0.45	0.22	0.41
Head of household	0.33	0.47	0.38	0.49	0.42	0.49	0.48	0.50	0.47	0.50
Household characteristics										
Children in the family	1.08	1.07	1.03	1.06	1.20	1.13	1.18	1.13	1.34	1.30
Unemployed in the household	0.17	0.38	0.16	0.36	0.18	0.39	0.16	0.37	0.17	0.37
Number of rooms (average)	3.69	1.30	3.66	1.42	3.59	1.29	3.73	1.30	3.49	1.39
Wall brick	0.98	0.15	0.98	0.14	0.97	0.15	0.97	0.17	0.92	0.27
Floor tile	0.70	0.46	0.77	0.42	0.71	0.45	0.73	0.44	0.58	0.49
Previous labour market history										
Previous job duration (in months)	22.53	29.84	26.82	33.57	29.94	38.91	36.71	45.82	37.90	53.95
Unemployment spell (in months)	6.62	11.50	4.14	8.76	4.05	9.10	3.72	9.51	4.44	11.41
Previous private employee	0.75	0.43	0.81	0.39	0.80	0.40	0.75	0.44	0.63	0.48

3. Treatment effects with matching estimators

The estimation strategy makes standard identifying assumptions used in the context of PSM (Heckman et al., 1999). Since the analysis considers the effects of one programme on participants compared with the status of non-participation, it is possible to use the potential outcome framework with two potential outcomes: Y_1 (employment outcome of the treated) and Y_0 (employment outcome of the untreated). The outcome that is actually observed in the data for any individual i is equal to $Y_i = Y_{i,1} * D_i + (1 - D_i) * Y_{i,0}$; where $D \in \{0,1\}$ takes the value of 1 if the individual is treated and 0 otherwise. The treatment effect is defined as $\Delta = Y_{i,1} - Y_{i,0}$. The parameter of interest is the average treatment effect on the treated (ATT):

$$ATT = E(\Delta \mid D = 1) = E(Y_1 \mid D = 1) - E(Y_0 \mid D = 1) \quad (1)$$

The first term corresponds to the average outcome of interest among treated individuals; while the second term represents the average outcome of interest among the treated if they had not been treated. If one is willing to assume that this second term is equal to $E(Y_0 \mid D = 0)$; then it would be possible to use the simple average of the outcome of interest for the control group as counterfactual. However, this would require participants and non-participants to differ only for their decision to participate. Matching techniques are based instead on the (weaker) assumption that, conditional on a vector of observable covariates denoted by X, the relevant outcome Y is independent of D. However, matching directly on the covariates (i.e. exact matching) can be problematic – especially when X is of high dimension.[6] A possibility for solving this problem is to use balancing scores (denoted as $b(X)$). These are functions of the relevant observable covariates X, with the property that the

[6] In these cases, it is likely that conditional on some values of X there is no variation in treatment.

conditional distribution of X given $b(X)$ is independent from assignment to treatment. The propensity score $P(X)$ is a possible balancing score, which summarizes the information that is relevant for the treatment into a single number. Two assumptions need to hold for the propensity score to provide a valid matching algorithm; while a third one is needed independently from the estimation strategy:

Assumption 1: *Conditional independence*

$$Y_0 \coprod D \mid P(X) \quad (2)$$

which states that the outcome of interest of non-participants has – conditional on $P(X)$ – the same distribution of the outcome that participants would have if they had not participated in the programme (Heckman et al., 1997). This assumption can be directly derived from $[Y_0 \coprod D \mid X]$, which implies that reducing the dimensionality problem using the propensity score (as discussed) does not require additional assumptions compared to exact matching

Assumption 2: *Common support*

$$0 < P(D = 1 \mid X) < 1 \quad (3)$$

which states that the propensity score of the participants should be strictly between 0 and 1 for any given value of *X*. In the sample, this implies that for any given value of *X* there should be both participants and non-participants in the data. This serves to rule out the hypothesis of perfect predictability of D given X and to ensure that individuals with the same values of X have a positive probability of being both participants and non-participants.

Assumption 3: *Stable unit treatment value assumption*

$$Y\ (i, s, p) = Y\ (i, s, p') = Y\ (s, p) \text{ for } s\ \in S_p\ \cap\ S_{p'} \text{ and for all } p, p' \in P \quad (4)$$

which states that outcomes for an individual i under treatment s are the same in two different policy regimes of the treatment p and p'. As it turns out, the SUTVA imposes two exclusive restrictions: (i) it rules out social interactions across treatment groups; and (ii) it excludes any effect of the assignment to treatment on potential outcomes (Heckman, 2005).

After having defined the estimation strategy, an additional step concerns taking into account the fact that the outcomes of interest for this analysis (i.e. formal nature of the job and wage levels) are in this specific case recursively defined conditional on the intermediate outcomes of (i) having found a job; and (ii) still being in that job at the time of interview. Intuitively, this requires taking into account that these individuals are not a random sub-sample of the treatment and control groups. This is an important issue in program evaluation that applies irrespective of the estimation strategy chosen (i.e. see for instance Attanasio et al. (2011) for a recent application from a randomized trial in Colombia). Different methods have been developed in the literature to deal with this issue, with the choice among them being mainly driven by data at hand and the research question to be answered.[7] The present analysis follows an important stream of the literature of impact evaluation in the use of the Heckman sample selection model (Heckman, 1979; Maddala, 1983).

4. Implementation of matching and identification of the exclusion restriction

After having chosen to use matching techniques, the researcher is confronted with a number of steps aimed at checking the validity of the chosen strategy. These include the estimation of the propensity score (section 4.1); the choice of the matching algorithm (4.2); and the definition of the area of common support (4.3) (Caliendo and Kopeinig, 2008). Additionally and given this specific research question, it is also needed to identify valid exclusion restrictions (4.4). However and before moving forward, the discussion should cover the validity in the given research context of the assumptions behind PSM specified above.

The CIA is indeed a particularly strong assumption and its plausibility depends on the available data as well as the programme to be evaluated (Caliendo and Künn, 2012). Blundell et al. (2005) argue that its plausibility should be discussed on a case-by-case basis, taking into account the richness of the data and the institutional framework in which selection into treatment takes place. For the CIA to hold, the analysis needs to condition – and thus include in the estimation of the propensity score – all variables that jointly determine (i) programme participation; and (ii) the outcome of interest. Although there is no common rule on the set of necessary information, previous work suggests the need to include personal and household characteristics, previous labour market history and regional labour market indicators (Lechner and Wunsch, 2013). In the case of the evaluation of APE, the GEIH presents a rich collection of both individual and household characteristics that could serve this function – including information on previous labour market experience, which the literature has found being key predictor of programme participation (Heckman et al., 1999). Additionally, it is worth noting that PSM has been extensively used for the evaluation of PESs in both developed and developing economies – assuming that selection into treatment does not critically rely on unobservable characteristics compared to other ALMPs (e.g. training).[8] Additionally and specifically for the case of Colombia, previous research has shown that individuals do not self-select themselves into different job-search channels (i.e. especially formal versus informal) based on unobservable characteristics (Diaz, 2012). Finally and although it is not possible to directly test the validity of the CIA, it is still possible to check how much the results are sensitive to its eventual violation. These tests are conducted (Appendix D) and show the overall soundness of the methodology.

Similarly than with the CIA, the validity of the SUTVA in this specific case cannot be directly tested but can only be discussed based on economic theory and results from previous

[7] In particular, these methods can be divided in (i) parametric (Heckman, 1979); (ii) semi-parametric (Ichimura and Lee, 1991; Ahn and Powell, 1993); and (iii) non -parametric approaches (Horowitz and Manski, 2000; Lee, 2009). All these models differ in their identifying assumptions and ways to handle sample selection.

[8] Papers include Naticchioni and Loriga (2010), Rodriguez-Planas (2010) and Heinrich et al. (2013) as well as the majority of the impact evaluations of PESs in LAC mentioned in the introduction.

research. Evidence for a possible violation of the first restriction of the SUTVA (absence of general equilibrium effects) has been recently reported for the PES in France (Crépon et al., 2013). In particular, their results indicate that the positive effects obtained by PES participants in finding a job have come at the expense of non-participating eligible individuals. In the case of the present evaluation, a similar situation would occur if formal and better paid jobs that are taken by PES participants come at the expense of similar employment opportunities that become unavailable to non-participants specifically due to the existence of the PES. However, (i) the limited share of individuals participating in the PES in Colombia (around one per cent of total job-matches each year); and (ii) the focus of the analysis on employment quality (rather than on the probability of finding a job) should considerably limit the risk of any displacement effect. Additionally, Blundell et al. (2004) do not find any evidence of displacement effects in the United Kingdom for a programme that combined job-search assistance with wage subsidies. In the case of Colombia, Attanasio et al. (2011) do not report displacement effects from a vocational training programme.

4.1 Estimation of the propensity score

When using PSM, the first choices concern (i) the model to be used for the estimation of the propensity score; and (ii) the selection of the variables to be included. Regarding the first step, little advice is available with respect to the functional form to be used – with any discrete choice model potentially fulfilling the task (Smith, 2000). However, a clear preference has emerged in the literature towards logit or probit models – that generally held very similar results (Caliendo and Kopeinig, 2008). More advice is instead available with respect to the choice of the variables to be included in the estimation of the propensity score. As seen above, only variables that influence at the same time programme participation and the outcome of interest should be included. For these reasons, all variables included in the estimation should pre-date programme participation and be unrelated with its effects – or with its anticipation. Hence, the choice should be subject to the knowledge of the labour market where the programme intervenes and the economic theory behind the effects of the programme (Sianesi, 2004). Different models for the propensity score are tested for the purpose of the present analysis, looking at results of different tests suggested by the literature on PSM (Caliendo and Kopeinig, 2008) while also following previous literature on the selection of individuals in PESs in developing countries (Chacaltana and Sulmont, 2003). A detailed discussion of these choices and the results of the tests are reported in Appendix B. The final model that is chosen includes a rich series of personal and educational covariates, household characteristics, information on previous labour market history (including the so-called Heckman correction, to be discussed below) and regional dummies and labour market indicators – while matching is performed on the exact year.

The results of the model of the propensity score (Table C.1 in Appendix C) show that the probability of participation in the PES decreases with age; whereas it increases with educational attainments – in both cases the effect is of higher magnitude for men than women. Being single increases the probability to participate compared to the other categories (cohabiting, married, divorced and widow) and the effect is particularly important for women. Dummies for the kinship status are generally statistically significant; while the number of children in the household does not affect the probability of participation overall (except for the control group classified advertisements and relatives and friends), but it has a negative effect for women. Living in an apartment (rather than a house) positively predicts participation, which in turn is negatively associated with the number of rooms in which the household lives – which might suggest that participants come from a lower socio-economic background. Additionally, having a source of non-labour income affects the probability of PES participation (positively in the case of income from rents and negatively in case of other non-labour income). Turning to the previous career history, duration of the unemployment spell and the length of tenure in the last job both significantly affect the probability of participation – positively and negatively respectively. Similarly, the occupational status in the previous job is statistically significant – especially when comparing PES participants with those that found their job through employers or relatives and friends. The Heckman correction for sample selection has a strong positive significant effect on participation – with the exception of the control group classified jobs, when the effect is positive but not statistically significant. Finally, regional dummies and regional unemployment rates also are in most of the cases statistically significant (although no clear pattern emerges).

4.2 Matching algorithm and quality of matching

After having estimated the propensity score, the following step concerns the choice of the matching algorithm. Different options have been suggested – nearest neighbour, radius and kernel matching among others. All approaches will give asymptotically the same results, but in small samples the choice of the algorithm can be important (Smith, 2000). It is therefore preferred to test different matching algorithms and compare their goodness in reducing the bias. The results of these tests are displayed separately for the matching of the same pool of PES participants with different control groups (Table 2). According to the expectations, matching should reduce the mean standardised bias (MSB) between control and treatment groups – as observable characteristics should be balanced between the two groups after matching (Rosenbaum and Rubin, 1985). According to empirical studies, a MSB below 5 per cent after matching should be sufficient (Caliendo and Kopeinig, 2008). In this case, the MSB decreases from an average value around 15-20 per cent before matching to values between 2 and 4 after matching – with no significant differences across control groups. Sianesi (2004) suggests as an additional test to estimate the propensity score of matched individuals before and after matching and compare the pseudo-R^2. The underlying assumption is that – after matching has taken place – there should be no systematic difference in observable characteristics between control and treatment groups – hence the pseudo-R^2 should decrease. This is also confirmed in this case, with the pseudo-R^2 decreasing substantially in all specifications – with the exception of relatives and friends, for which the decrease in the pseudo-R^2 is less marked. The same results are obtained when looking at the t-test of equality of means, with the number of variables with statistically significant differences in means between treatment and control groups decreasing after matching – with the unique exception of kernel matching with the control group employers.

Table 2: Quality indicators of the matching algorithm

	Classified Advertisements				Private Agencies			
	Unmatch	Neighbour	Caliper	Kernel	Unmatch	Neighbour	Caliper	Kernel
Pseudo R2	0.17	0.03	0.03	0.03	0.15	0.01	0.01	0.01
Mean standardised bias	14.0	3.7	3.5	3.3	14.4	2.2	2.2	2.0
t-test of equality of means								
10% level		13	12	10		8	7	5
5% level		13	10	7		3	3	1
1% level		5	5	4		1	1	1
	Employers				**Relatives and Friends**			
	Unmatch	Neighbour	Caliper	Kernel	Unmatch	Neighbour	Caliper	Kernel
Pseudo R2	0.13	0.01	0.01	0.02	0.22	0.14	0.14	0.14
Mean standardised bias	16.6	1.7	1.7	4.6	25.1	3.7	3.7	3.7
t-test of equality of means								
10% level		3	3	14		4	4	4
5% level		2	2	13		2	2	2
1% level		2	2	13		2	2	2

Note: Nearest neighbour matching is obtained with N=1 with replacement. Caliper matching uses as caliper 0.2 of the standard deviation of the logit of the propensity score. Kernel matching uses 100 replications and a bandwidth of 0.06. The dependent variable is in all specifications the dummy for formal employment. The different specifications all include 59 independent variables (including the departmental dummies). The t-test panel reports the number of variables whose difference between treatment and control groups is statistically significant.

Finally, it is useful to provide also a graphical representation of how the matching procedure balances observable characteristics between treated and non-treated individuals. Figures C.1 and C.2 in Appendix C (reporting box plots and density plots) show how the propensity score between treated and untreated individuals becomes extremely similar after matching. Given that the results of these tests are not significantly sensitive to changes in the matching algorithm chosen, the rest of the analysis follows previous literature opting for caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement (Austin, 2011). However, the robustness of the results is also tested following a number of changes in the matching algorithm used (Appendix D).

4.3 Area of common support

The additional step in order to verify the quality of the matching is to check the area of common support between treated and non-treated individuals. The most straightforward way is to conduct a graphical analysis of the density distribution of the propensity score in the two groups – see Figure C.3 in Appendix C. This figure shows that – as expected – the propensity score is on average higher for participants than non-participants. Observations outside the area of common support correspond to participating individuals with propensity scores approaching one – representing the case of perfect predictability of participation. Table 3 contains information on the number of observations lost with the "minima and maxima" procedure. Following this procedure, observations whose propensity score is smaller (larger) than the minimum (maximum) in the opposite group are deleted. It is important to note that if the proportion of individuals lost is small; this creates few problems (Bryson et al., 2002). However, if the share of lost individuals is significant; concerns might arise with respect to the internal validity of the obtained results. The results show how the total share of treated individuals lost due to matching is fairly low for all the different comparison groups – from 2.33 per cent in the case of matching with employers to 0.31 per cent for private agencies.

Importantly, there is some non-randomness in the characteristics of participating individuals that are lost – which should be kept in mind when interpreting the results. This follows from differences in observable characteristics between treated and non-treated individuals as discussed in section 2.2. Indeed, the share of individuals lost below the age of 30 is higher than the average share of observations lost. Similarly, lost observations are more likely to correspond to highly educated individuals. However, the share of observations that is dropped from the analysis is still acceptable according to previous empirical studies.[9]

Table 3: Number of treated individuals before and after matching

		Before Matching	Classified Advertisements		Private Agencies		Employers		Relatives and Friends	
			After	Lost (%)	After	Lost (%)	After	Lost (%)	After	Lost (%)
Total		5,443	5,424	0.35	5,426	0.31	5,316	2.33	5,347	1.76
	<30	3,174	3,159	0.47	3,162	0.38	3,059	3.62	3,087	2.74
Age	**30-40**	1,637	1,634	0.18	1,633	0.24	1,626	0.67	1,628	0.55
	>40	632	631	0.16	631	0.16	631	0.16	632	0.00
	Below high school	121	121	0	121	0.00	121	0.00	121	0.00
Education	**High school**	1,009	1,006	0.30	1,009	0.00	1,004	0.50	1,007	0.20
	College	4,313	4,297	0.37	4,296	0.39	4,191	2.83	4,219	2.18

Note: The common support is checked using formal employment as outcome variable and performing the analysis by caliper matching (caliper of 0.01)

4.4 Exclusion restriction

Before turning to the results of the evaluation, the analysis should discuss the exclusion restrictions that are needed to deal with sample selection. Indeed and as mentioned in section 3, measures of the quality of employment are recursively defined based on the realisation of the intermediate outcomes of (i) having obtained a job; and (ii) being in the job at time of interview. The issue is common to a number of impact evaluations of both experimental and quasi-experimental nature. However, in this case the analysis needs to deal with a more severe problem than the one generally encountered; given that (i) the database is composed of repeated cross-sections; and (ii) information on the successful job search method is available only for those individuals that are employed at the time of interview. As mentioned in section 3, this paper uses the selection model approach to account for incidental truncation of the dependent variable; arguing that the richness of the data available in the GEIH allows identifying suitable instruments.

For the definition of the determinants of employment, the analysis follows previous literature that has modelled sample selection in the Colombian labour market (Badel and Peña, 2010). Since matching occurs by exact year, the employment equation is also computed separately for each year in the period under consideration. More challenging is the identification of the exclusion restriction, as no consensus has yet emerged in the literature on the presence of variables that (independently from the context of the analysis) can credibly influence only labour market participation; without also affecting employment conditions (and wages in particular) – even after introducing a rich set of covariates. In this specific case, we exclude

[9] For instance, Caliendo et al. (2008) lose between 5 and 10 per cent of the individuals below the age of 25.

instruments related to the employment situation of other members of the household (e.g. presence of unemployed in the household) as well as general household characteristics (e.g. number of children in the household), as in a largely informal labour market with high levels of working poverty it is difficult to rule-out the presence of intra-household spill-over effects that influence both the probability of being employed (as desired) as well as employment conditions at work.[10] For instance, previous work in Colombia has already revealed the presence of neighbourhood effects that influence the choice of the job-search method and consequently employment conditions (Nicodemo and García, 2015).

The exclusion restriction used in this paper corresponds to a dummy which equals one if the individual is paying for the house where he/she lives – either because the house is rented or because despite being the owner, the individual is still paying for it (e.g. mortgage) – compared to a situation in which the individual is the owner of the house and has fully paid for it. This situation is likely to have a direct impact on the probability of participating in the labour market (which is anyway tested in the first stage); since individuals that need to meet these payments (e.g. rent) are likely to have an additional incentive to participate in the labour market with respect to comparable individuals who do not need to pay for housing. However, turning this into a dummy – rather than using the face value of the payment – limits the risk of linking the instrument to other socio-economic characteristics of the household (e.g. income) that might be connected with the outcome of interest (especially wage), even after introducing a rich set of covariates. Indeed, paying for the house is a relatively common situation in Colombia (46 per cent of the individuals in the sample) which is not necessarily connected to specific socio-economic characteristics. This is also checked by looking at selected descriptive statistics (Appendix A, Tables A.3 and A.4), which show that the two groups of individuals (i.e. paying and not paying for the house) are substantially homogeneous with respect to the main aspects (e.g. years of education, age, gender) that could potentially influence employment conditions (e.g. wages).

5. Empirical results

This section presents the results of the analysis on the effects of participation in APE on formal employment (5.1) and wage levels (5.2). A rich set of robustness tests is conducted to test the validity of these results following: (i) changes in the matching algorithm; (ii) changes in the area of common support and; (iii) the possible presence of unobserved heterogeneity. These are typical tests that the literature suggests to conduct as part of the implementation of PSM (see for instance Caliendo et al., 2008), which in this case have been tailored to the possible violations of the assumptions which are more likely to occur in the present analysis. Moreover, a decomposition of the wage gap between treated and untreated individuals by quantiles is performed to assess the validity of PSM as an estimation strategy in the given research context (Machado and Mata, 2005). All these tests provide supporting evidence for the chosen methodology and their results can be consulted in Appendix D.

[10] For instance, Diaz (2012) is willing to make this assumption and uses as an instrument for the use of informal job-search channels in Colombia a dummy which takes the value of one if the closest member of the household also found the current job through informal channels.

5.1 Formal employment

The analysis first examines the effects of participation in the PES in Colombia on the probability of finding a formal (rather than informal) job. The results show that treatment increases the likelihood of having a formal job; when treated individuals are compared to those that found a job through classified advertisements, direct contact with the employer and relatives and friends. The effect is instead negative – but of limited magnitude and statistical significance – when treated individuals are compared to those that have found their job through private employment agencies – Tables from C.2 to C.5. In the preferred specification, APE participation increases the likelihood of having a formal job by 9 percentage points with respect to those that used classified advertisements, 5 percentage points with respect to those that contacted employers and 31 percentage points for those that enquired their relatives and friends. The order of magnitude is consistent with evidence according to which jobs found through informal networks (such as friends and family) are more likely to be of informal nature (Diaz, 2012; Nicodemo and García, 2015). For all the control groups, the effect is stronger for women than men and for low- than high-educated individuals – possibly reflecting the initially higher levels of informality among the two groups.[11] Additionally, different evaluations have reported that low-skilled jobseekers have more positive results from participating in the PESs in both developed and developing countries (Gregg and Wadsworth, 1996; Heinrich et al., 2013; Rodriguez-Planas, 2007).

The analysis then aims to disentangle the mechanisms through which this effect of the PES on formality operates in Colombia. For this purpose, the baseline specification is increased with an additional control for the size of the company where the individual is employed.[12] This reveals that across the control groups for which a positive and significant effect of the PES on formality is found in the baseline specification (i.e. classified advertisements, employers, relatives and friends), around two-thirds of this effect is connected to APE's capacity to place individuals in larger companies. This can be connected to the results of previous research, which has shown that large companies in developing economies are more likely to comply with labour legislation (Almeida and Carneiro, 2012; Almeida and Ronconi, 2012). The effect of PES participation on formal employment becomes instead statistically non-significant for the control group of private employment agencies when adding the additional control for firm size. This can be connected to: (i) the similarity of the services provided by public and private providers of labour market services (e.g. counselling, orientation); and (ii) the similarities in terms of the profiles of the companies that approach public and private providers of labour intermediation (e.g. large companies).

Additionally, the analysis investigates how on-line and face-to-face systems of service provision differ with respect to their effectiveness in placing job-seekers in formal jobs. This is an important aspect for PESs in many developed and developing economies, as recent

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[11] Low-skilled individuals are defined as those that have obtained at most a high-school degree; while high-skilled individuals are those that have enrolled in a tertiary education degree (without necessarily completing it), which can be either university or vocational training (provided the training is counted as formal education).

[12] However, the "correct" specification should not control for firm size since this is an aspect that it is likely to be part of the effect of participation – see discussion in section 4.1.

policy initiatives have increasingly been focused on investing in the development of on-line platforms of labour intermediation as a means to increase PES coverage (OECD, 2015). However, the results show that, when restricting the analysis to online matches, the effects of the PES on the probability of having a formal job is alternatively negative and statistically significant (control groups: private agencies and employers) or non-statistically significant (control group: classified advertisements) or positive and significant but of substantially lower magnitude (control group: relatives and friends). By contrast, the results of the analysis when the sample is restricted to face-to-face matches confirm the positive effects of PES participation on the probability of having a formal job. These differences in the results between on-line and face-to-face matches cannot be ascribed to composition effects between the two groups of job-seekers (as discussed in section 2.2, presenting the descriptive statistics) and demonstrate instead how the effect of the PES on formal employment does not merely come from companies complying with labour legislation self-selecting into the PES – with informal companies remaining instead outside the system.[13] Rather, the analysis suggests that the effects on formality come from a better labour market matching resulting from the (face-to-face) services provided by the APE.

5.2 Wages

Turning to the effects of treatment on wages, the analysis reveals that participation in the PES in Colombia has generally a negative effect on hourly earnings. This is true when PES participants are compared with the control groups of classified advertisements, private employment agencies and direct contact with the employer; while the effect is positive and statistically significant when the control group is composed by those that found their job through relatives and friends. The magnitude of the effects is somehow smaller than with formal employment, with the coefficients ranging from 2 to 5 per cent across the different comparison groups. Previous research on Colombia has shown that the use of formal job-search channels had a positive effect on wages, especially at the bottom of the income distribution (Diaz, 2012; Nicodemo and García, 2015). This analysis is able to disentangle the mechanisms through which different formal and informal channels have an effect on wages; confirming the results with respect to the larger informal control group (i.e. relatives and friends) while showing that looking for a job through direct contact with the employer (i.e. the other job-search method which is traditionally considered informal) has a positive effect on wages compared to the PES. This could be related to the lower transactional costs when the recruitment is conducted by directly approaching the employer (i.e. the vacancy does not necessarily need to be posted) as well as the probably lower asymmetries of information on the quality of the candidate connected to his/her personal knowledge by the employer (Dustmann et al., forth.). At the same time, the analysis reveals how among the different formal job-search channels (i.e. the PES, private employment agencies and classified advertisements), the PES is the less effective in placing candidates in well-paid jobs – although the effect when the control group is private agencies is of limited magnitude and

[13] Indeed, if this was the case the results would not differ (at least not substantially) between online and face-to-face matches – as the job bank is the same for the job-seekers using the two types of PES system provision.

statistical significance. This might be related to stigmatization effects on PES participants or the lack of capacity of APE to attract productive enterprises in the system (ILO, 2016).[14]

Adding the control for the size of the firm where the individual is employed reveals that the negative effect of the PES on wages is even more substantial. In particular, the negative coefficient increases in absolute magnitude when PES participants are compared with the control groups classified advertisements and employers; while turning from positive to negative when the control group is composed by those that have found their current job though relatives and friends. As for the results on formality, this is connected to PES's capacity to place individuals in larger companies – which have been traditionally shown paying higher wages.[15] By contrast, adding the additional control for firm size does not significantly change the results when the comparison group is composed by those that have found their job through private employment agencies – thus confirming the similar size structure of the companies reverting to public or private labour intermediation. However, even in this case the coefficient loses statistically significant when adding the additional control for firm size; thus confirming the generally similar services provided by public and private providers of labour market services – as in the case of formal employment.

Differentiating the results by age and educational levels reveals that – in contrast to the results on formality – male PES participants do relatively better than female participants. In particular, the effect of the PES on wages is non-statistically significant for men when the control group is composed by private employment agencies and employers; while being negative and statistically significant for women. Additionally, the positive effect of the PES on wages when the control group is relatives and friends is statistically significant only for men – while being positive but not significant for women.[16] Turning to differences by educational levels reveals that the overall effect on wages comes from a positive effect on the wages of the low-skilled and a (generally stronger in magnitude) effect on the wages of the high-skilled. This is a particularly strong result, which stands for all control groups analysed and is compatible with the idea that labour intermediation reduces wage dispersion at the bottom of the income distribution – by for instance ensuring compliance with minimum wage legislation. At the same time, the negative effect on the wages of the high-skilled might signal PES incapacity to attract high-quality vacancies. Finally, the results also confirm that APE is less effective online than face-to-face. In particular, among the different control groups the results show that the effect of the PES on wages is negative and statistically significant when restricting the analysis to online matches only; while being non-significant (or of lower magnitude) when looking at job-matches occurred after face-to-face job-search.

[14] As an alternative explanation, employers might be able to shift (either partially of entirely) the cost of mandated benefits that they need to provide to formal workers. For example, Almeida and Carneiro (2012) find that in Brazil formal workers trade compliance to mandated benefits with lower wages.

[15] However and as mentioned above, the "correct" specification of the propensity score should not control for firm size; since this is likely to be part of the effect of treatment.

[16] The results do not substantially differ between genders for the control group of classified announcements.

6. Conclusions

This paper examines by means of PSM the effects of participation in the PES in Colombia on the probability of being employed in a formal (rather than informal) job and hourly wage levels. This is a particularly important question from a policy point of view, since many LAC countries have recently increased substantially their public investments in ALMPs as a means to sustain the creation of productive employment and ultimately raise productivity. However, very little is known with respect to the effectiveness of ALMPs (and the PES in particular) in the region; while results from studies conducted in advanced economies cannot be easily extended. This refers to both structural differences in the functioning of the labour markets (e.g. high levels of informal employment) as well as differences in the nature and scope of ALMPs between developed and developing economies (Auer, 2008).

The results of the analysis show that finding a job trough the PES in Colombia has a positive effect on the probability of having a formal (rather than informal) job. This is partially connected to PES's capacity to place individuals in larger companies, which are traditionally characterised by a higher level of labour law compliance. By contrast, participation in the PES has a negative effect on hourly wages. This derives from a positive effect on the wages of the low-skilled and a negative effect on the wages of the high-skilled. For both formality and wages, the PES is more effective when the services are provided face-to-face rather than online. The validity of the chosen strategy is repeatedly tested following (i) changes in the matching algorithm; (ii) changes in the area of common support and (iii) possible presence of unobserved heterogeneity (Appendix D). These tests follow the literature on PSM; while also expanding with respect to previous studies in order to take into considerations the possible violations of the assumptions behind PSM that are more likely to occur in the given research context (e.g. Machado and Mata wage decomposition).

When discussing the results, some caveats should be kept in mind. The first one refers to the fact that there is no information concerning the intensity of the treatment among participants (e.g. how many visits to PES centres) and the analysis can therefore only investigate the extensive margin (APE participants compared to non-participants), but not the intensive one (different types of APE participants). Similarly, no information is available on the functioning of the different PES centres (e.g. number of social workers, cases dealt within a month); so that it is not possible to look at the heterogeneity of the effects across different types of PESs. Finally, the analysis cannot account for the risk of contamination, whereas those that have found a job through alternative job-search channels were in the first place unsuccessful with the PES. However, evidence from Colombia seems to suggest that contamination might eventually work in the opposite direction – with individuals first looking for a job informally and only afterwards turning towards formal channels such as the PES (Uribe and Gómez, 2005). This means that the analysis might be underestimating the true effects of APE participation.

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Appendix A: Additional descriptive statistics

Table A.1: Selected descriptive statistics of treatment and control groups (found job through the internet)

	PES		Classified Advertisements		Private Agencies		Employers		Relatives and Friends	
Number of observations	591		3,229		2,911		5,382		2,034	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
Personal characteristics										
Average age	29.35	7.41	29.67	7.05	29.52	6.73	30.19	7.43	34.53	10.59
Male	0.50	0.50	0.49	0.50	0.49	0.50	0.50	0.50	0.56	0.50
Average years of education	13.31	2.10	13.98	2.61	13.61	2.48	13.91	2.67	11.66	4.06
Vocational training	0.47	0.50	0.32	0.47	0.34	0.47	0.31	0.46	0.19	0.39
Married	0.20	0.40	0.19	0.39	0.20	0.40	0.21	0.41	0.26	0.44
Head of household	0.35	0.48	0.31	0.46	0.33	0.47	0.35	0.48	0.47	0.50
Household characteristics										
Children in the family	1.02	0.98	0.84	0.91	0.89	0.97	0.89	0.96	1.20	1.16
Unemployed in the household	0.16	0.37	0.15	0.36	0.15	0.36	0.14	0.35	0.15	0.36
Number of rooms (average)	3.71	1.43	3.75	1.40	3.78	1.39	3.74	1.33	3.61	1.37
Wall brick	0.98	0.15	0.99	0.10	0.99	0.10	0.99	0.10	0.96	0.19
Floor tile	0.74	0.44	0.81	0.39	0.83	0.37	0.81	0.39	0.69	0.46
Previous labour market history										
Previous job duration (in months)	21.38	25.83	22.40	27.93	21.81	27.97	24.13	31.69	35.52	47.40
Unemployment spell (in months)	5.37	9.20	3.52	6.92	3.37	6.71	3.42	7.01	4.43	12.48
Previous private employee	0.77	0.42	0.85	0.36	0.86	0.35	0.85	0.36	0.74	0.44

Table A.2: Selected descriptive statistics of treatment and control groups (found job through face-to-face contact)

	PES		Classified Advertisements		Private Agencies		Employers		Relatives and Friends	
Number of observations	5,218		5,687		24,375		163,405		572,599	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
Personal characteristics										
Average age	29.47	8.56	34.39	9.80	33.26	9.41	36.06	10.42	36.59	12.03
Male	0.50	0.50	0.45	0.50	0.56	0.50	0.55	0.50	0.56	0.50
Average years of education	13.01	2.11	11.99	3.74	11.50	3.01	12.31	3.80	9.45	4.42
Vocational training	0.48	0.50	0.19	0.39	0.20	0.40	0.19	0.39	0.11	0.31
Married	0.19	0.39	0.23	0.422	0.22	0.41	0.29	0.45	0.22	0.41
Head of household	0.33	0.47	0.43	0.49	0.43	0.49	0.48	0.50	0.47	0.50
Household characteristics										
Children in the family	1.09	1.08	1.14	1.12	1.24	1.14	1.19	1.13	1.34	1.30
Unemployed in the household	0.17	0.38	0.16	0.37	0.19	0.39	0.17	0.37	0.17	0.37
Number of rooms (average)	3.68	1.28	3.61	1.43	3.57	1.28	3.72	1.30	3.49	1.39
Wall brick	0.98	0.16	0.98	0.16	0.98	0.15	0.97	0.17	0.92	0.27
Floor tile	0.70	0.46	0.75	0.43	0.70	0.46	0.73	0.45	0.58	0.49
Previous labour market history										
Previous job duration (in months)	22.66	30.25	29.32	36.15	30.91	39.91	37.13	46.16	37.91	53.97
Unemployment spell (in months)	6.99	12.58	4.75	10.78	4.14	9.34	3.73	9.59	4.88	13.07
Previous private employee	0.75	0.44	0.79	0.40	0.79	0.41	0.74	0.44	0.63	0.48

Table A.3: Selected descriptive statistics of treatment and control groups (paying for the house)

	PES		Classified Advertisements		Private Agencies		Employers		Relatives and Friends	
Number of observations	2,831		5,099		13,941		81,839		261,187	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
Personal characteristics										
Average age	29.23	8.02	32.13	8.70	32.04	8.60	34.34	9.51	34.89	11.00
Male	0.48	0.50	0.46	0.50	0.54	0.50	0.54	0.50	0.54	0.50
Average years of education	13.07	2.09	12.59	3.46	11.74	2.96	12.33	3.62	9.87	4.12
Vocational training	0.49	0.50	0.23	0.42	0.22	0.41	0.20	0.40	0.11	0.32
Married	0.21	0.41	0.23	0.42	0.22	0.41	0.28	0.45	0.21	0.41
Head of household	0.43	0.50	0.48	0.50	0.51	0.50	0.57	0.50	0.54	0.50
Household characteristics										
Children in the family	1.02	0.99	1.04	1.05	1.16	1.06	1.15	1.07	1.30	1.21
Unemployed in the household	0.16	0.36	0.14	0.34	0.17	0.37	0.15	0.35	0.16	0.36
Number of rooms (average)	3.38	1.22	3.30	1.32	3.30	1.22	3.41	1.23	3.22	1.30
Wall brick	0.98	0.13	0.99	0.12	0.98	0.12	0.98	0.13	0.96	0.19
Floor tile	0.75	0.43	0.80	0.40	0.77	0.42	0.77	0.42	0.67	0.47
Previous labour market history										
Previous job duration (in months)	22.93	30.26	26.19	32.06	27.83	34.81	33.39	40.53	34.36	47.59
Unemployment spell (in months)	6.37	11.14	4.18	9.61	3.80	8.47	3.48	8.79	4.69	12.41
Previous private employee	0.75	0.43	0.81	0.39	0.81	0.39	0.77	0.42	0.68	0.47

Table A.4: Selected descriptive statistics of treatment and control groups (not paying for the house)

	PES		Classified Advertisements		Private Agencies		Employers		Relatives and Friends	
Number of observations	2,978		3,816		13,345		86,949		313,446	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
Personal characteristics										
Average age	29.68	8.84	33.42	9.75	33.72	9.78	37.31	10.96	38.00	12.63
Male	0.51	0.50	0.47	0.50	0.57	0.49	0.55	0.50	0.58	0.49
Average years of education	13.01	2.12	12.96	3.52	11.72	3.10	12.40	3.92	9.11	4.62
Vocational training	0.47	0.50	0.25	0.44	0.21	0.41	0.19	0.39	0.10	0.30
Married	0.18	0.38	0.20	0.40	0.22	0.41	0.30	0.46	0.23	0.42
Head of household	0.23	0.42	0.26	0.44	0.32	0.47	0.39	0.49	0.42	0.49
Household characteristics										
Children in the family	1.15	1.14	1.02	1.09	1.25	1.19	1.20	1.18	1.37	1.37
Unemployed in the household	0.19	0.39	0.19	0.39	0.20	0.40	0.18	0.39	0.18	0.38
Number of rooms (average)	3.97	1.30	4.14	1.42	3.88	1.30	4.02	1.30	3.72	1.43
Wall brick	0.97	0.18	0.97	0.16	0.97	0.17	0.96	0.20	0.89	0.31
Floor tile	0.66	0.48	0.74	0.44	0.66	0.48	0.69	0.46	0.51	0.50
Previous labour market history										
Previous job duration (in months)	22.15	29.42	27.66	35.49	32.15	42.67	39.83	50.10	40.86	58.56
Unemployment spell (in months)	6.86	11.83	4.46	9.54	4.33	9.72	3.96	10.14	5.03	13.59
Previous private employee	0.75	0.43	0.81	0.39	0.78	0.41	0.73	0.45	0.58	0.49

Appendix B: Estimation of the propensity score

As discussed in section 4.1 of the paper, one of the first steps in the estimation of the propensity score is the choice of the variables to be included. In particular, the literature suggests including all variables that jointly determine (i) programme participation; and (ii) the outcome of interest. In case of uncertainty regarding the relevance of a variable, questions might arise on whether it should be included in the estimation. Indeed, over-specified models should be avoided because (i) including extraneous variables might exacerbate the common support problem – thus reducing the number of individuals included in the analysis without improving its precision; and (ii) although the inclusion of extraneous variables will not affect the inconsistency and bias of the estimates, it could nevertheless increases their variance (Bryson at al., 2002). On the other hand, Rubin and Thomas (1996) argue that variables should be excluded from the estimation of the propensity score only if there is consensus about their uncorrelatedness with programme participation or if they are not proper covariates. This latter approach is followed in a number of papers (including Jalan and Ravallion, 2003; Caliendo et al., 2005).

In order to choose between these options, we follow Caliendo and Kopeinig (2008) and test the ability of different models (from the most parsimonious to the most generous) to predict participation. Indeed, a number of indicators can be looked at when assessing the goodness of different propensity score specifications – such that the discussion presented above can be solved empirically. In particular, the "hit or miss" method is constructed with the aim of maximising the correct prediction rates of participation (the larger the hit rate, the better the model). According to this method, an individual i is assigned the value of one if the corresponding propensity score is larger than the share of individuals in the sample participating in the treatment – otherwise the individual is classified as zero. An alternative indicator of goodness of the specification is the pseudo-R^2, which instead captures how well the covariates X explain the probability of participation. Both statistics have been computed for a number of specifications. The analysis started with basic specifications – from line 1 to 5 – containing only one category of covariates at a time (personal characteristics, educational characteristics, household characteristics, previous career and geographical indicators) and then added sets of covariates together (lines from 6 to 8) until reaching the full specification (line 9) (Table B.1). It is important to note that the final matching – as it will be used for the discussion of the results – occurs on the exact year – rather than by including yearly dummies.[18]

The results of this analysis show that across the different control groups – classified advertisements, private agencies, employers and relatives and friends – the pseudo-R^2 is maximised in all cases in the specification with the entire set of covariates included – as expected. The "hit or miss" method reveals instead a more complex scenario. Indeed, the hit-rate is maximised by the specification including personal characteristics in some cases (when

[18] The matching has not been conducted by both exact year and department because the high number of departments (24) – matched over seven years – would have led to a relatively high share of individuals for which there was not a comparable control – and that would have been excluded from the analysis.

the control group is classified advertisements, private employment agencies and direct contact with the employer); while in the case of relatives and friends the specification with only regional dummies does the best job in predicting participation.

Following the "hit or miss" method would then justify using different specifications for each control group identified above, including models that predict participation only based on regional dummies and labour market indicators. However, this has very limited justification from an economic viewpoint as important characteristics critically influencing participation would be excluded. Importantly, it has to be kept in mind that the main objective of PSM is not to correctly predict participation (which is instead the goal of the "hit or miss method"); but rather to balance covariates (Augurzky and Schmidt, 2001). For these reasons, the analysis opts for the specification with the entire set of covariates for estimating the propensity score.[19] This results in the inclusion of 59 variables (including the departmental dummies); while matching is conducted on the exact year. The covariates that are included reflect an understanding of selection into PESs in LAC (Chacaltana and Sulmont, 2003; Vera, 2013); while following previous studies that use matching techniques to evaluate SENA training (in particular Medina and Núñez, 2005). Importantly, results in both the estimation of the propensity score (e.g. number of individuals off-support) and the outcome of interest are not particularly sensitive to the inclusion (or exclusion) of single variables – proving overall the stability of the propensity score.

Table B.1: Hit-Rates and Pseudo R2 for Different Propensity Score Specifications

Specification					Classified Advertisements		Private Agencies		Employers		Relatives and Friends	
Personal	Education	Household	Career	Region	Hit-rate	R2	Hit-rate	R2	Hit-rate	R2	Hit-rate	R2
X					0.534	0.027	0.508	0.025	0.459	0.050	0.409	0.043
	X				0.471	0.073	0.362	0.101	0.284	0.081	0.261	0.135
		X			0.516	0.043	0.422	0.020	0.332	0.023	0.374	0.025
			X		0.470	0.066	0.389	0.047	0.388	0.051	0.322	0.136
				X	0.423	0.107	0.344	0.104	0.437	0.035	0.449	0.022
X	X				0.448	0.085	0.363	0.107	0.293	0.106	0.247	0.153
X	X	X			0.452	0.121	0.377	0.119	0.301	0.110	0.249	0.155
X	X	X	X		0.471	0.127	0.375	0.113	0.351	0.094	0.270	0.188
X	X	X	X	X	0.444	0.232	0.351	0.215	0.297	0.155	0.228	0.237

Hit-rates: If the estimated propensity score for the individual is larger than the sample proportion of individuals participating in the programme; then the observation is classified as "one". In the opposite case, observations are classified as "zero". ***Personal:*** Gender, age and relation within household. ***Education:*** Number of years of completed education, vocational education, current enrollment status, writing skills. ***Household:*** Marital status, relation within the household, number of rooms, material for floor and wall, type of house, number of children in the household, presence of unemployed in the household, non-labour income in the household. ***Career:*** Previous occupational status, duration of the last job, unemployment duration, company size, Heckman correction term. ***Region:*** Dummy for each department and regional unemployment rate.

[19] For instance, even in Caliendo et al. (2008) the "hit or miss" method is maximised in some models by the specification containing only regional dummies. However, the authors opt for a more complete specification that better explains programme participation form an economic viewpoint.

Appendix C: Tables and figures

Table C.1: Propensity score estimation

	Control: Classified Advertisements			Control: Private Agencies			Control: Employers			Control: Relatives and Friends		
	Coefficient		S.E.	Coefficient		S.E.	Coefficient		S.E.	Coefficient		S.E.
Personal characteristics												
Age	-0.030	***	0.003	-0.023	***	0.003	-0.054	***	0.003	-0.051	***	0.002
Male	0.400	***	0.072	0.141	**	0.061	0.236	***	0.050	0.158	***	0.049
Years of education	0.000		0.010	0.077	***	0.009	0.023	***	0.007	0.161	***	0.006
Vocational training	1.140	***	0.045	1.026	***	0.037	1.234	***	0.030	1.472	***	0.031
Now enrolled in education	0.472	***	0.152	0.771	***	0.126	0.654	***	0.099	0.605	***	0.097
Writing	1.057		0.999	0.427		0.740	1.343	*	0.712	0.811		0.581
Family status (omitted: single)												
Cohabiting	-0.199	***	0.076	-0.233	***	0.064	-0.096	*	0.053	0.033		0.051
Married	-0.064		0.079	-0.081		0.066	-0.033		0.055	0.126	**	0.054
Divorced	-0.144		0.095	-0.261	***	0.081	-0.120	*	0.068	-0.126	*	0.067
Widow	-0.275		0.252	0.004		0.225	-0.122		0.201	-0.225		0.194
Relation in the household (omitted: no relative)												
Head	0.478	***	0.144	0.362	***	0.132	0.107		0.112	0.568	***	0.112
Spouse	0.328	**	0.161	0.323	**	0.145	0.026		0.123	0.331	***	0.122
Son	0.353	**	0.150	0.392	***	0.138	0.134		0.117	0.539	***	0.115
Grandson	0.210		0.217	0.342		0.187	0.111		0.156	0.421	***	0.153
Other relative	0.276		0.162	0.312	**	0.145	0.041		0.124	0.383	***	0.122
Other household characteristics												
Number of children	0.038	*	0.021	-0.010		0.018	0.014		0.014	-0.028	**	0.014
Unemployed in the family	-0.155		0.098	-0.109		0.080	-0.101		0.067	-0.010		0.066
Wall of brick (omitted: other)	0.220		0.146	0.033		0.117	0.309	***	0.096	0.514	***	0.093
Floor of tile (omitted: other)	-0.188	***	0.050	-0.082	*	0.042	-0.090	**	0.036	0.028		0.035
Apartment (omitted:house)	0.168	***	0.047	0.106	***	0.039	0.094	***	0.033	0.119	***	0.032
Number of rooms	-0.033	*	0.018	-0.039	**	0.016	-0.035	**	0.014	-0.030	**	0.013
Income from rent	0.082		0.121	0.392	***	0.112	0.348	***	0.090	0.229	***	0.088
Other non-labour income	-0.405	***	0.054	-0.233	***	0.049	-0.116	***	0.042	-0.054		0.040
Previous career (omitted: previous domestic worker)												
Unemployment spell	0.028	***	0.002	0.026	***	0.002	0.026	***	0.001	0.017	***	0.001
Previous job duration	-0.002	**	0.001	-0.003	***	0.001	-0.004	***	0.001	-0.005	***	0.001
Previous: private employee	-0.261		0.184	-0.319	**	0.152	-0.277	**	0.139	0.906	***	0.134
Previous: public employee	-0.156		0.230	-0.510	***	0.187	-0.807	***	0.165	0.478	***	0.160
Previous: own account	0.129		0.189	0.150		0.156	0.094		0.142	1.127	***	0.136
Previous: employer	-0.197		0.488	0.085		0.521	-0.002		0.364	0.628		0.352
Previous: family worker	0.882	***	0.284	1.301	***	0.249	0.965	***	0.194	1.514	***	0.180
Previous: other with no remuneration	1.070		0.726	-1.155		1.375	-0.498		0.703	0.525		0.708
Previous: daily worker	-0.320		0.410	-0.543		0.362	-1.338	***	0.357	-0.825	**	0.356
Previous: other with remuneration	-0.012		0.762	0.381		0.687	0.195		0.782	1.092		0.718
Heckman correction	0.597	***	0.207	0.265		0.170	0.349	***	0.134	0.311	**	0.131
Regional dummies	Yes			Yes			Yes			Yes		
Unemployment rate	Yes			Yes			Yes			Yes		
Number observations	13,816			31,107			164,853			541,086		
Pseudo R2	0.212			0.208			0.153			0.178		
Log likelihood	-7292.832			-11415.158			-20220.330			-25034.132		

*/**/*** significant at the 10, 5 and 1 per cent.

Note: Standard errors are clustered at the household level. Results for regional dummies and the unemployment variable are not reported. The estimation of the propensity score that is used in the paper has been computed separately for men and women, while here the results are reported only for the overall sample. Differences of the results between men and women (when significant) are discussed in the text.

Table C.2: Effects of APE participation on the probability of being in a formal job and wages - Control group: Classified Advertisements

	Total				Male				Female			
	Formal employment		Wages		Formal employment		Wages		Formal employment		Wages	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Baseline equation	0.094 ***	0.012	-0.036 *	0.019	0.038 ***	0.014	-0.073 **	0.030	0.126 ***	0.016	-0.057 **	0.027
Control for firm size	0.038 ***	0.011	-0.083 ***	0.020	0.010	0.013	-0.036	0.031	0.070 ***	0.015	-0.108 ***	0.024
Online matches only	0.000	0.017	-0.126 ***	0.035	-0.011	0.022	-0.170 ***	0.051	0.015	0.025	-0.162 ***	0.047
Excluding online matches	0.150 ***	0.016	0.027	0.025	0.088 ***	0.021	-0.014	0.037	0.205 ***	0.021	0.024	0.035
Low-educated	0.238 ***	0.022	0.127 ***	0.031	0.113 ***	0.026	0.042	0.034	0.351 ***	0.033	0.206 ***	0.053
High-educated	0.046 ***	0.011	-0.101 ***	0.023	0.018	0.013	-0.095 ***	0.036	0.066 ***	0.014	-0.140 ***	0.027
Controls												
Personal	Yes		Yes		Yes		Yes		Yes		Yes	
Education	Yes		Yes		Yes		Yes		Yes		Yes	
Household	Yes		Yes		Yes		Yes		Yes		Yes	
Previous career	Yes		Yes		Yes		Yes		Yes		Yes	
Regional dummies and UN rate	Yes		Yes		Yes		Yes		Yes		Yes	
Heckman correction	Yes		Yes		Yes		Yes		Yes		Yes	
Number of observations (baseline)	13,841		13,559		6,559		6,436		7,242		7,123	

*/**/*** significant at the 10, 5 and 1 per cent.

Note: Matching occurs by exact year. The equations are all computed with caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement.

Table C.3: Effects of APE participation on the probability of being in a formal job and wages - Control group: Private Agencies

	Total				Male				Female			
	Formal employment		Wages		Formal employment		Wages		Formal employment		Wages	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Baseline equation	-0.011 *	0.005	-0.022 *	0.013	-0.025 ***	0.007	0.031	0.019	-0.009	0.008	-0.090 ***	0.016
Control for firm size	-0.002	0.006	-0.017	0.013	-0.009	0.007	0.042 **	0.020	0.003	0.008	-0.072 ***	0.016
Online matches only	-0.037 **	0.015	-0.125 ***	0.034	-0.026	0.023	-0.135 **	0.055	-0.022	0.024	-0.172 ***	0.044
Excluding online matches	-0.009	0.006	-0.008	0.014	-0.022 ***	0.007	0.053 **	0.020	-0.007	0.008	-0.082 ***	0.017
Low-educated	-0.028 **	0.012	0.045 ***	0.017	-0.039 ***	0.014	0.096 ***	0.024	-0.012	0.018	-0.025	0.022
High-educated	-0.011 **	0.005	-0.065 ***	0.016	-0.020 ***	0.007	-0.033	0.025	-0.011	0.008	-0.116 ***	0.019
Controls												
Personal	Yes		Yes		Yes		Yes		Yes		Yes	
Education	Yes		Yes		Yes		Yes		Yes		Yes	
Household	Yes		Yes		Yes		Yes		Yes		Yes	
Previous career	Yes		Yes		Yes		Yes		Yes		Yes	
Regional dummies and UN rate	Yes		Yes		Yes		Yes		Yes		Yes	
Heckman correction	Yes		Yes		Yes		Yes		Yes		Yes	
Number of observations (baseline)	31,157		30,081		17,067		16,350		14,090		13,731	

*/**/*** significant at the 10, 5 and 1 per cent.

Note: Matching occurs by exact year. The equations are all computed with caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement.

Table C.4: Effects of APE participation on the probability of being in a formal job and wages - Control group: Employers

	Total				Male				Female			
	Formal employment		Wages		Formal employment		Wages		Formal employment		Wages	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Baseline equation	0.052 ***	0.006	-0.055 ***	0.012	0.037 ***	0.007	0.000	0.017	0.072 ***	0.009	-0.113 ***	0.015
Control for firm size	0.034 ***	0.006	-0.073 ***	0.012	0.017 **	0.007	-0.003	0.017	0.052 ***	0.008	-0.134 ***	0.015
Online matches only	-0.052 ***	0.015	-0.283 ***	0.037	-0.040 *	0.022	-0.244 ***	0.057	-0.046 **	0.022	-0.262 ***	0.051
Excluding online matches	0.057 ***	0.006	-0.052 ***	0.012	0.039 ***	0.008	0.008	0.018	0.085 ***	0.009	-0.102 ***	0.016
Low-educated	0.095 ***	0.015	0.059 ***	0.019	0.068 ***	0.018	0.122 ***	0.026	0.165 ***	0.025	0.023	0.026
High-educated	0.034 ***	0.006	-0.124 ***	0.014	0.027 ***	0.008	-0.072 ***	0.021	0.044 ***	0.008	-0.167 ***	0.017
Controls												
Personal	Yes		Yes		Yes		Yes		Yes		Yes	
Education	Yes		Yes		Yes		Yes		Yes		Yes	
Household	Yes		Yes		Yes		Yes		Yes		Yes	
Previous career	Yes		Yes		Yes		Yes		Yes		Yes	
Regional dummies and UN rate	Yes		Yes		Yes		Yes		Yes		Yes	
Heckman correction	Yes		Yes		Yes		Yes		Yes		Yes	
Number of observations (baseline)	165,228		154,746		89,711		83,328		75,517		71,418	

*/**/*** significant at the 10, 5 and 1 per cent.

Note: Matching occurs by exact year. The equations are all computed with caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement.

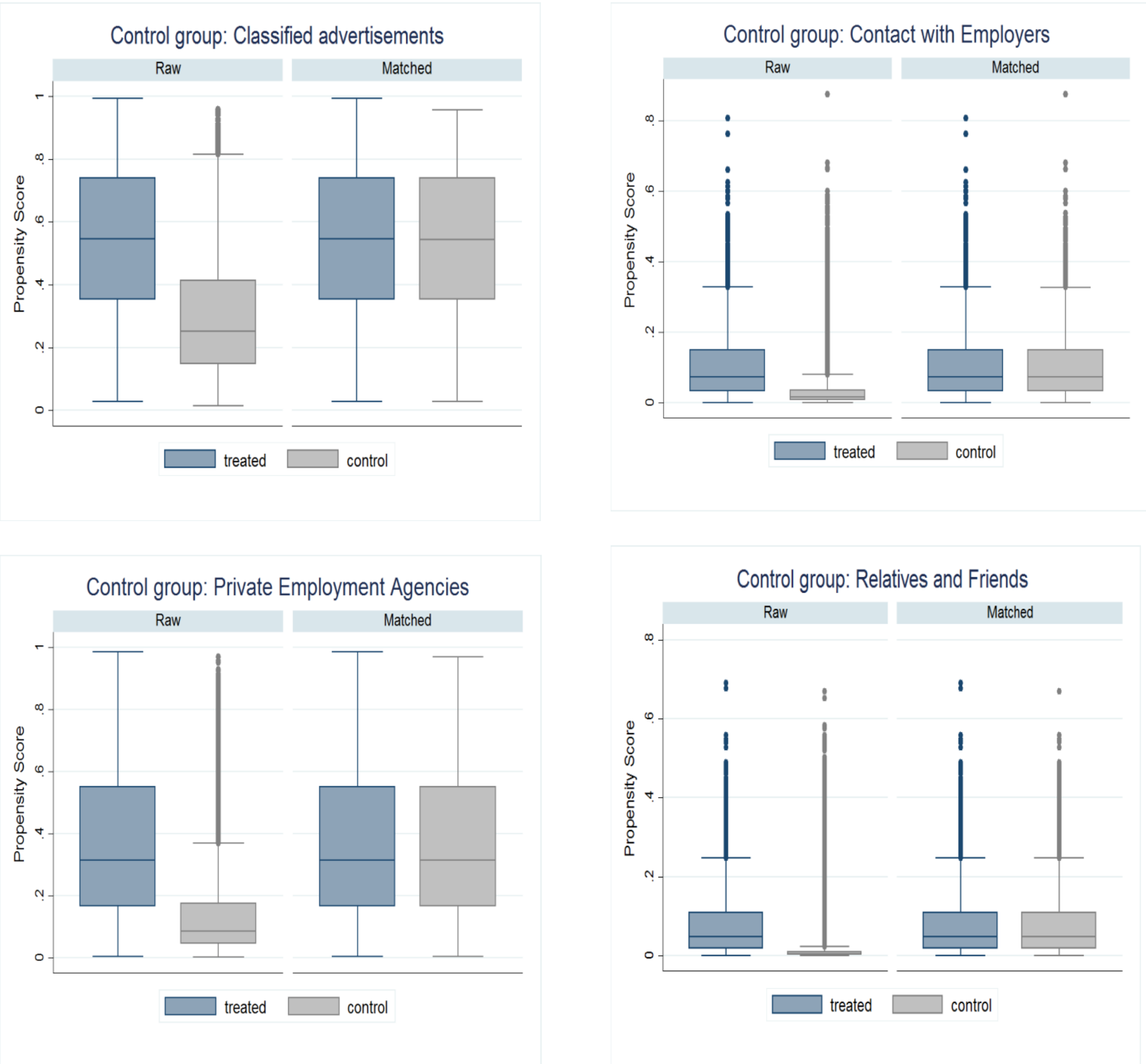
Table C.5: Effects of APE participation on the probability of being in a formal job and wages - Control group: Relatives and Friends

	Total				Male				Female			
	Formal employment		Wages		Formal employment		Wages		Formal employment		Wages	
	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.	Coefficient	S.E.
Baseline equation	0.309 ***	0.008	0.056 ***	0.013	0.275 ***	0.011	0.074 ***	0.019	0.334 ***	0.012	0.011	0.016
Control for firm size	0.107 ***	0.007	-0.053 ***	0.012	0.098 ***	0.009	0.026	0.026	0.115 ***	0.009	-0.130 ***	0.016
Online matches only	0.058 **	0.024	-0.174 ***	0.044	0.062 *	0.032	-0.127 **	0.065	0.062 *	0.037	-0.112 *	0.059
Excluding online matches	0.309 ***	0.009	0.057 ***	0.014	0.277 ***	0.011	0.083 ***	0.020	0.336 ***	0.336	0.009	0.009
Low-educated	0.456 ***	0.019	0.224 ***	0.023	0.379 ***	0.024	0.236 ***	0.029	0.535 ***	0.027	0.205 ***	0.205
High-educated	0.245 ***	0.009	-0.041 ***	0.014	0.219 ***	0.012	-0.048 **	0.022	0.261 ***	0.012	-0.066 ***	0.017
Controls												
Personal	Yes		Yes		Yes		Yes		Yes		Yes	
Education	Yes		Yes		Yes		Yes		Yes		Yes	
Household	Yes		Yes		Yes		Yes		Yes		Yes	
Previous career	Yes		Yes		Yes		Yes		Yes		Yes	
Regional dummies and UN rate	Yes		Yes		Yes		Yes		Yes		Yes	
Heckman correction	Yes		Yes		Yes		Yes		Yes		Yes	
Number of observations (baseline)	541,086		524,892		303,113		293,338		237,973		231,544	

*/**/*** significant at the 10, 5 and 1 per cent.

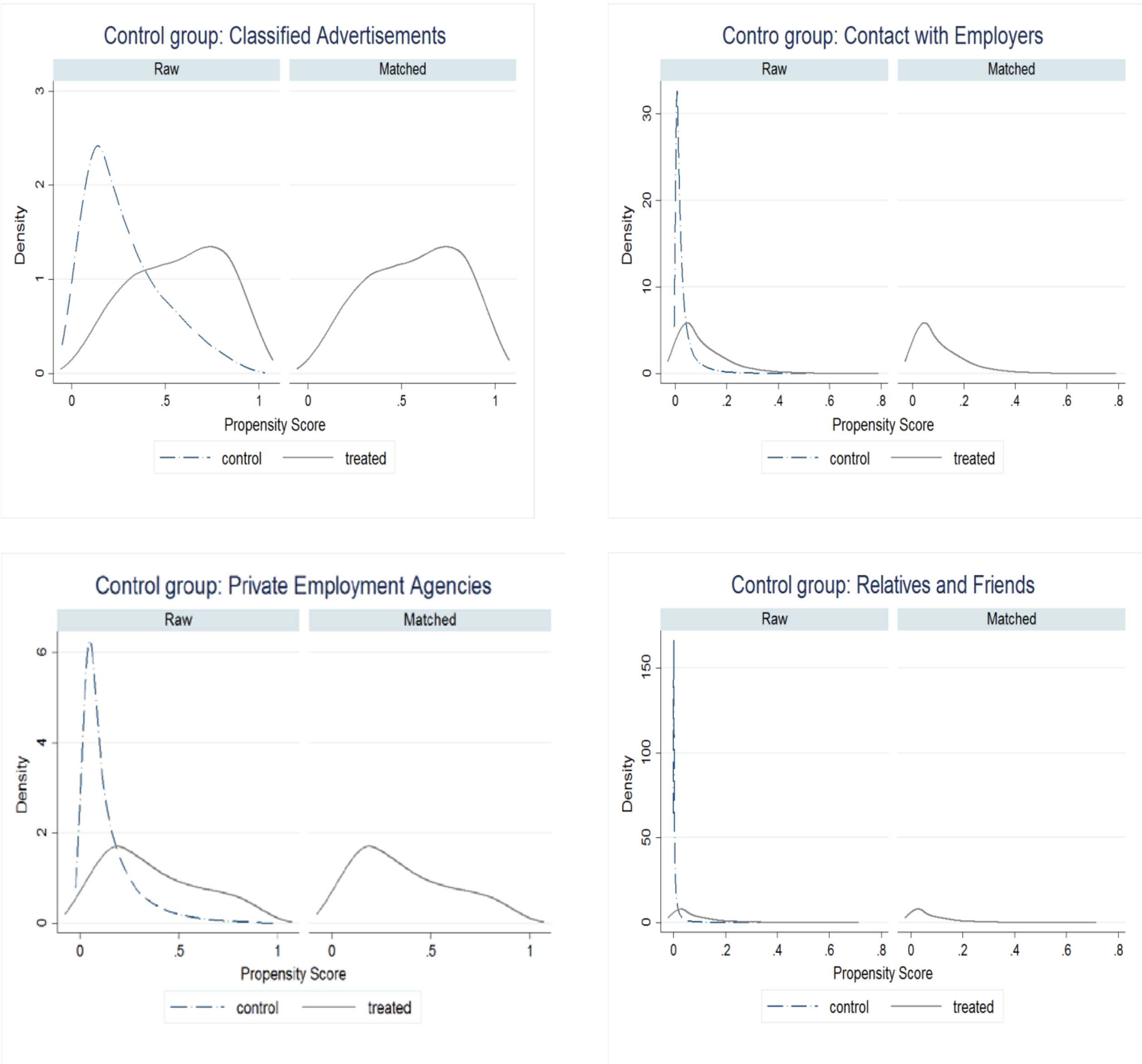
Note: Matching occurs by exact year. The equations are all computed with caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement.

Figure C.1: Box plots of propensity scores between PES and different comparison groups



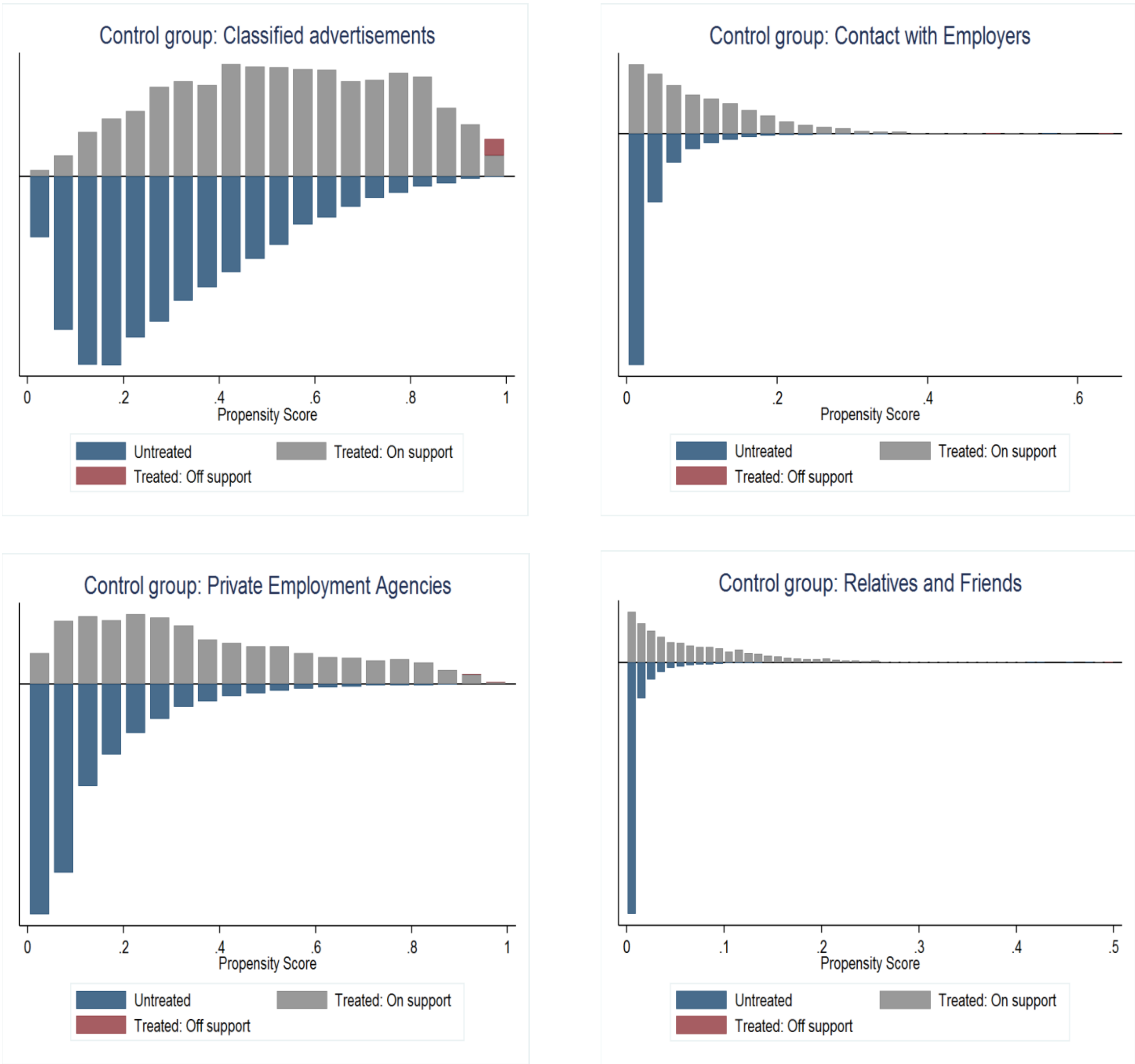
Note: The figures are derived with the post-estimation command *tebalance box* after *tebalance psmatch* run with robust standard errors and the use of one nearest neighbour matching. Outcome variable is the dummy for formal employment.

Figure C.2: Kernel density plots of propensity scores between PES and different comparison groups



Note: The figures are derived with the post-estimation command *tebalance density* after *tebalance psmatch* run with robust standard errors and the use of one nearest neighbour matching. Outcome variable is the dummy for formal employment.

Figure C.3: Propensity score distribution of treated and untreated (different comparison groups)



Note: The figures are derived with the post-estimation command *psgraph* after *psmatch2* run the use of caliper (bandwidth kept constant at 0.01).Outcome variable is the dummy for formal employment

Appendix D: Sensitivity analysis

As a final step, the analysis tests the robustness of the results to changes in the identifying assumptions behind PSM that have been presented earlier in the paper (sections 3 and 4). In particular, the tests will verify the robustness of the results following (i) changes in the matching algorithm; (ii) changes in the area of common support; and (iii) the presence of unobserved heterogeneity between treated and non-treated individuals.[20]

The first set of tests corresponds to verifying the sensitivity of the results to changes in the matching algorithm (see section 4.2). In particular, departures from the baseline equations (for formal employment and wages) are performed using nearest neighbourhood (NN), caliper and kernel matching.[21] Moreover, for each of the three algorithms different choices are made with respect to the comparison group to be taken into account. In particular, NN matching is performed with one neighbour (with and without replacement) as well as oversampling (two and five neighbours). Caliper matching is performed with a caliper equal to 0.2 of the standard deviation of the logit (optimal caliper, used in the analysis) as well as with a caliper of 0.1, 0.2 and 0.5. Finally, kernel matching is done with the optimal choice of 0.06 as well by choosing a much larger (0.2) and smaller (0.002) bandwidth. In all the circumstances, the purpose is to test whether limiting the analysis only to observations very close to the treated individuals or (alternatively) including also those very far away does have an impact on the analysis (Caliendo and Kopeinig, 2008). The results are encouraging, showing that for both formal employment and wages the magnitude of the coefficients as well as their statistical significance does not vary substantially both across different algorithms and within algorithms across different levels of tolerance for the definition of the comparison group – see Table D.1.

As an additional test, the analysis estimates the effects of the programme for different sub-sets of the population where participants and non-participants are more concentrated. Indeed and as reviewed in section 4, the "minima and maxima" method for the definition of the common support does not take into consideration the density of the distribution for different levels of the propensity score. For example, this method does not take into account problems that may arise if the density in the tails of the distribution is very thin; while disregarding observations just outside the bounds even if they correspond to areas of high density in the distribution. To deal with these issues, the literature suggests restricting the area of common support in two different ways (Caliendo and Künn, 2012). First, it is possible to follow Black and Smith (2004) and estimate the effects of the programme only in a region of "thick support"; defined by those individuals whose propensity score lies between 0.33 and 0.67 ($0.33 < \hat{P}(x) < 0.67$). Secondly, the analysis divides the distribution of the propensity score into ten deciles and estimates the effects of the programme only in those deciles for which there is at least five per

[20] The third assumption presented in section 3 (SUTVA) cannot be checked with the available data. That would (for instance) require comparing non-participants in areas where the programme is operating to non-participants in areas where the programme is not implemented (see Blundell et al. 2004).

[21] As specified in section 4.2, the baseline equation has been performed using caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement

cent of the density of the distribution of both participants and non-participants. The results obtained after applying these restrictions largely match those obtained in the baseline equation for formal employment and wages – see Table D.2.

Table D.1: The effects of participation in the PES - Comparison across different matching algorithms

	Classified advertisements				Private Agencies				Employers				Relatives and Friends			
	Formal employment		Wages		Formal employment		Wages		Formal employment		Wages		Formal employment		Wages	
Nearest neighbour matching	**Coefficient**	**S.E.**	**Coefficient**	**S.E.**	**Coefficient**	**S.E.**	**Coefficient**	**S.E.**	**Coefficient**	**S.E.**	**Coefficient**	**S.E.**	**Coefficient**	**S.E.**	**Coefficient**	**S.E.**
NN1	0.092 ***	0.012	-0.035 *	0.200	-0.011 *	0.005	-0.022 *	0.013	0.054 ***	0.006	-0.055 ***	0.012	0.309 ***	0.007	0.056 ***	0.013
NN1 no replacement	0.092 ***	0.006	-0.137 ***	0.012	-0.013 ***	0.004	0.001	0.010	0.053 ***	0.005	-0.060 ***	0.011	0.305 ***	0.007	0.047 ***	0.011
NN2	0.085 ***	0.010	-0.037 *	0.018	-0.015 ***	0.005	-0.019 *	0.011	0.054 ***	0.005	-0.057 ***	0.010	0.295 ***	0.060	0.047 ***	0.011
NN5	0.089 ***	0.009	-0.046 ***	0.017	-0.015 ***	0.004	-0.021 **	0.011	0.055 ***	0.004	-0.053 ***	0.009	0.294 ***	0.005	0.047 ***	0.009
Caliper matching with NN1																
caliper 0.02 stand. deviation	0.094 ***	0.012	-0.036 *	0.019	-0.011 *	0.005	-0.022 *	0.012	0.052 ***	0.006	-0.055 ***	0.012	0.309 ***	0.008	0.054 ***	0.013
caliper 0.01	0.095 ***	0.011	-0.038 **	0.018	-0.011 **	0.005	-0.024 **	0.012	0.053 ***	0.006	-0.055 ***	0.012	0.308 ***	0.008	0.056 ***	0.013
caliper 0.02	0.093 ***	0.011	-0.037 **	0.018	-0.011 **	0.005	-0.023 *	0.012	0.053 ***	0.006	-0.055 ***	0.012	0.308 ***	0.008	0.056 ***	0.013
caliper 0.05	0.094 ***	0.011	-0.036 *	0.019	-0.010 *	0.005	-0.023 *	0.013	0.053 ***	0.006	-0.055 ***	0.012	0.309 ***	0.008	0.056 ***	0.013
Kernel matching																
bandwidth 0.06	0.087 ***	0.008	-0.046 ***	0.016	-0.014 ***	0.004	-0.021 **	0.009	0.056 ***	0.004	-0.079 ***	0.008	0.379 ***	0.003	0.161 ***	0.008
bandwidth 0.06 & bootstrap	0.087 ***	0.005	-0.046 ***	0.009	-0.014 ***	0.004	-0.021 **	0.009	0.056 ***	0.003	-0.079 ***	0.006	0.379 ***	0.003	0.161 ***	0.007
Kernel with bandwidth of 0.2	0.085 ***	0.007	-0.053 ***	0.014	-0.015 ***	0.004	-0.011	0.009	0.054 ***	0.003	-0.118 ***	0.008	0.424 ***	0.003	0.424 ***	0.003
Kernel with bandwidth of 0.002	0.092 ***	0.008	-0.047 ***	0.016	-0.017 ***	0.004	-0.031 ***	0.010	0.054 ***	0.004	-0.056 ***	0.008	0.295 ***	0.004	0.295 ***	0.004
Controls																
Personal	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Education	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Household	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Career	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Regional dummies and UN rate	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Heckman correction	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Number of observations	13,841		13,559		31,157		30,081		165,228		154,746		541,086		524,892	

*/**/*** significant at the 10, 5 and 1 per cent.

Note: Matching occurs by exact year. The bootstrap is computed with 100 replications.

The final test concerns the presence of unobserved heterogeneity between participants and non-participants. Indeed and as reviewed above, the entire estimation strategy is based on the validity of the CIA. If instead treated and untreated individuals differ for some unobserved characteristics that simultaneously affect programme participation and the outcome of interest, then the results would be biased. As this assumption is particularly important for the overall validity of the empirical approach used in the paper, three different tests will be performed. First, a simple ordinary least squares (OLS) regression is run to compare the results with those of the baseline specification. The main differences between the two approaches (i.e. matching versus OLS) refer to the fact that (i) matching is non- or semi-parametric and there is no assumption needed for the functional form of the outcome equation; (ii) matching uses the common support requirement while regression does not; and (iii) if effects of treatment are heterogeneous, matching is a more efficient technique to estimate the ATT (Caliendo, 2006). The results of the OLS are very similar to those of the baseline specification performed with PSM, with no clear upward or downward bias that can be detected (Table D.2). This is also in line with previous results in the literature that found that Colombian job-seekers do not self-select themselves into different job-search channels based on unobservable characteristics (Diaz, 2012)

Secondly, the literature suggests to (indirectly) verifying the CIA by using the bounding approach proposed by Rosenbaum (2002) for testing the sensitivity of the results to the potential presence of unobserved heterogeneity. Indeed, it is possible to determine how strongly an unobserved variable must kick-in for the results to become statistically insignificant (Caliendo et al., 2008; DiPrete and Gangl, 2004). The results of this test when the outcome of interest is the dummy for formal employment report that the critical value at the 5 per cent is equal to 2.65 for the control group of classified advertisements, 1.3 for private employment agencies, 1.8 for employers and 13.6 for relatives and friends. This means that the results discussed above for the effect of the programme on formality would still hold even if participating and non-participating individuals with the same vector of observable X would differ in their odds of participation (due to some unobserved heterogeneity) by a factor of 2.65 (165 per cent), 1.3 (30 per cent), 1.8 (80 per cent) and 13.6 (1,260 per cent) respectively. According to the previous literature, all these results (with the exception of the one obtained with the control group of private agencies) can be considered sufficiently robust to violations of the CIA (Becker and Caliendo, 2007). The critical values for the estimation that investigates the effects of participation on wages are instead equal to 1.35 for classified advertisements, 1.25 for private agencies and 1.55 for both employers and relatives and friends. This would suggest that the results of the analysis with respect to the effects of participation on wages are relatively more sensitive to the presence of unobserved heterogeneity.[22]

For this reason, the analysis performs an additional test of the robustness of the results specifically on wages. This goes beyond the traditional literature on PSM and draws from recent developments in the field of wage decomposition. In particular, Machado and Mata (2005) have proposed a method to extend the Oaxaca-Blinder decomposition to quantile regression. The idea is to decompose the wage gap between the treatment and control groups into observable and unobservable characteristics – the latter possibly accounting for the effect of the treatment (Fortin et al., 2011). The main purpose of this test in the context of the present analysis is to use a significantly different approach with respect to PSM and compare the results obtained with the two methods. These are available in Figure D.1 and show how the "coefficient" variable (i.e. which should account for the effect of treatment) has very similar values compared to those obtained with PSM (both in terms of magnitude and significance). In particular, the effect on wages when the control group is classified announcements is negative, of limited magnitude and significant only at the 10 per cent – as in the baseline equation for PSM. When the control group is composed by those that have found a job through private agencies, the results of the decomposition show that the PES has a negative (and significant) effect at the bottom of the income distribution and a positive (but

[22] However, it should be kept in mind that these are worst-case scenarios. Indeed, this does not mean that unobserved heterogeneity necessarily exists and/or there is no effect of treatment on the outcome variable; but rather that the effect of participation would be statistically non-significant if an unobserved variable caused the probability to participate to differ by the specific factor that is found (Becker and Caliendo, 2007).

non-significant) effect at the top – reflecting the small negative effect obtained by PSM for the overall sample. For the control group relatives and friends, the Machado and Mata decomposition confirms the positive effects of the PES on wages, which is particularly important at the bottom and the top of the income distribution. The only significant difference between the two methods is encountered with the control group of those that found a job through direct contact with the employer. Indeed, in this case PSM reports a negative and significant result while the wage decomposition shows a negative but non-significant result. Overall, however, these tests support the robustness of the results discussed above and provide strong evidence for the use of PSM.

Table D.2: Robustness checks - Dependent variable: Formal employment

	Classified Advertisements			Private Agencies			Employer			Relatives and Friends		
	Coefficient		S.E.	Coefficient		S.E.	Coefficient		S.E.	Coefficient		S.E.
Baseline specification												
	0.094	***	0.012	-0.011	*	0.005	0.052	***	0.006	0.309	***	0.008
Common support												
Thick support 1 // 0.33<P(W)<0.67	0.071	***	0.013	-0.017	***	0.006	0.054	***	0.005	0.390	***	0.005
Thick support 2 // F(P(W)>5%)	0.086	***	0.008	-0.014	***	0.004	0.053	***	0.005	0.379	***	0.003
Unobserved heterogeneity												
Critical value for exp(y)=1 at 5 per cent	2.65			1.3			1.8			13.6		
Alternative estimation strategies												
OLS with clustered SE	0.101	***	0.006	-0.015	***	0.004	0.048	***	0.004	0.293	***	0.004
Controls												
Personal	Yes			Yes			Yes			Yes		
Education	Yes			Yes			Yes			Yes		
Household	Yes			Yes			Yes			Yes		
Previous career	Yes			Yes			Yes			Yes		
Regional dummies and UN rate	Yes			Yes			Yes			Yes		
Heckman correction	Yes			Yes			Yes			Yes		

*/**/*** significant at the 10, 5 and 1 per cent.

Note: Matching occurs by exact year. The baseline equation is computed with caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement.

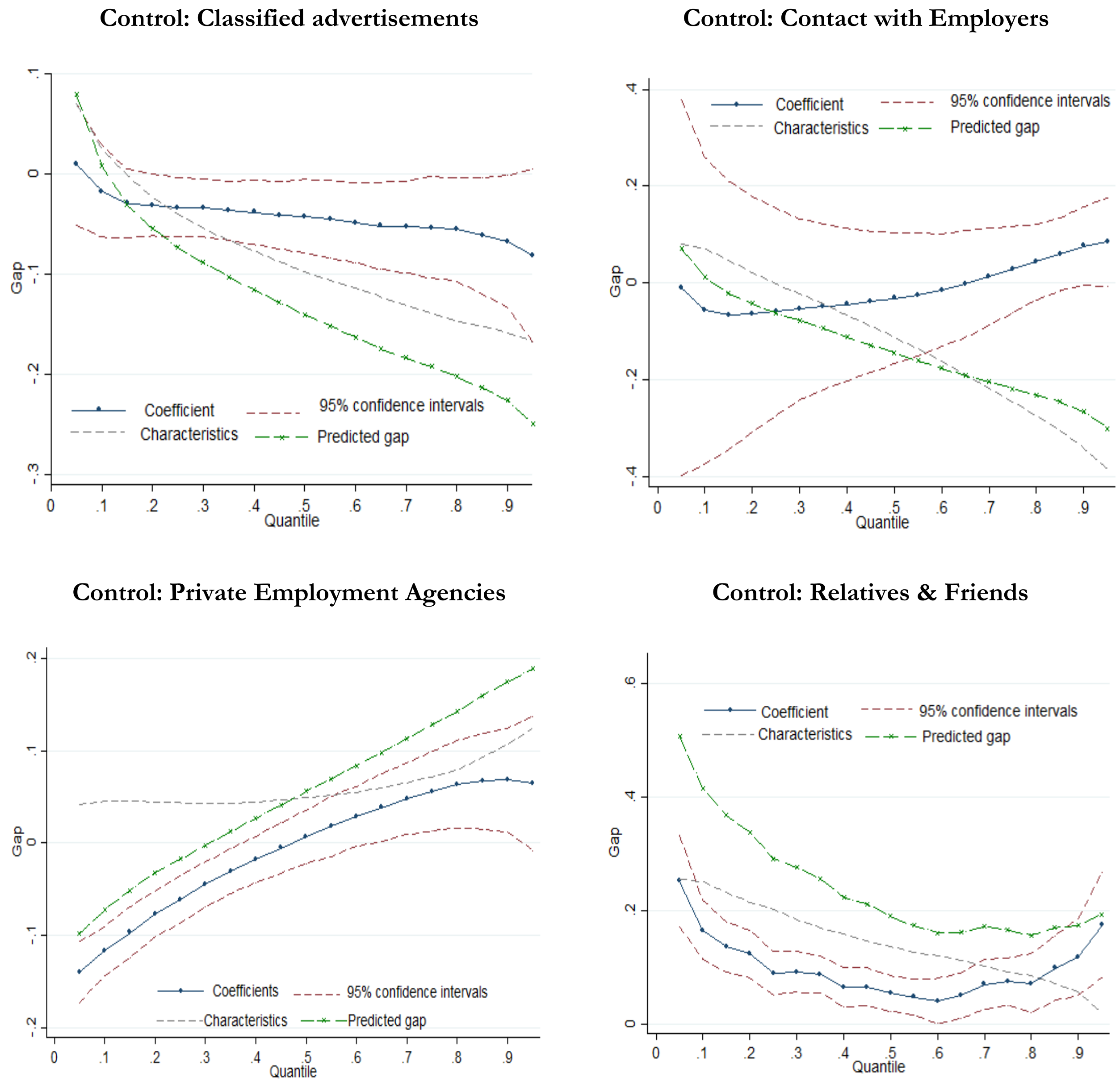
Table D.2 (continued): Robustness checks - Dependent variable: Wages

	Classified Advertisements			Private Agencies			Employer			Relatives and Friends		
	Coefficient		S.E.	Coefficient		S.E.	Coefficient		S.E.	Coefficient		S.E.
Baseline specification												
	-0.036	*	0.019	-0.022	*	0.013	-0.055	***	0.012	0.056	***	0.013
Common support												
Thick support 1 // 0.33<P(W)<0.67	-0.074	***	0.024	-0.050	***	0.014	-0.105	***	0.012	0.153	***	0.011
Thick support 2 // F(P(W)>5%)	-0.073	***	0.023	-0.021	**	0.010	-0.070	***	0.008	0.156	***	0.007
Unobserved heterogeneity												
Critical value for exp(y)=1 at 5 per cent	1.35			1.25			1.55			1.55		
Alternative estimation strategies												
OLS with clustered SE	-0.047	***	0.009	-0.023	***	0.007	-0.054	***	0.007	0.059	***	0.006
Controls												
Personal	Yes			Yes			Yes			Yes		
Education	Yes			Yes			Yes			Yes		
Household	Yes			Yes			Yes			Yes		
Previous career	Yes			Yes			Yes			Yes		
Regional dummies and UN rate	Yes			Yes			Yes			Yes		
Heckman correction	Yes			Yes			Yes			Yes		

*/**/*** significant at the 10, 5 and 1 per cent.

Note: Matching occurs by exact year. The baseline equation is computed with caliper matching with a bandwidth being equal to 0.2 of the standard deviation of the logit of the propensity score combined with nearest neighbour matching with replacement.

Figure D.1: Results of the Machado-Mata quantile wage decomposition



Note: The figures report the results of the Machado and Mata quantile wage decomposition. The "Predicted gap" corresponds to the wage gap between the treatment and control groups, the "Characteristics" line corresponds to the part of the gap that can be explained by observable characteristics (resulting from the wage equation with same covariates as for PSM) and the "Coefficient" accounts for the part of the gap that remains unexplained and can possibly be attributed to the choice of using the PES with respect to alternative job-search channels (with the relevant 95 per cent confidence intervals).