UNIVERSIDAD CARLOS III DE MADRID working papers

Working Paper #05-53 (15)
Business Economics Series
September 2005

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JOB MATCHING QUALITY EFFECTS OF EMPLOYMENT PROMOTION MEASURES FOR PEOPLE WITH DISABILITIES *

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Abstract

In this article, we evaluate the influence that employment promotion measures designed for disabled people have on the latter's job matching quality through the use of matching analysis. We focus on two aspects of quality: the type of contract held (either permanent or temporary) and whether or not the individual is searching for another job. We find that employment promotion measures do not improve the match's job quality. Furthermore, the use of specialized labour market intermediation services by disabled individuals does not affect their job matching quality. As an additional contribution, our definition of disability eludes the self-justification bias.

Keywords: Disability, job matching, employment promotion, evaluation, matching analysis, permanent contract, job-to-job search.
JEL Classification: J480, J40, I120

* Miguel A. Malo acknowledges the financial support received from the 'Junta de Castilla y León' (research project SA092/02). We would like to express our gratitude to participants at the Work Pensions and Labour Economics Conference held at York (United Kingdom, 2005), and at the seminar held at *Instituto de Estudios Fiscales* (Madrid, 2005). The usual disclaimer applies.

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1. Introduction

The objective of this article consists of analysing empirically the effects that employment promotion measures designed for people with disabilities have on the latter's job matching quality.

It is widely known that disabled individuals undergo a situation of wage discrimination, negative prejudices (with degrees depending on the type of disability), and that they have much lower participation rates than non-disabled individuals (which are also dependent on the type of disability). Previous researches supporting these statements are, for example, Baldwin and Johnson (1995), Loprest et al. (1995) or Kidd et al. (2000). Different policies have been implemented in many countries in order to countervail their disadvantaged position in the labour market. There exist two wide groups of policies: on the one hand, income transfer programmes; on the other hand, employment promotion measures. The former have already been previously studied (see, for example, the survey by Bound and Burkhauser, 1999). However, the use of employment promotion measures has been much scarcely analyzed (see Corden and Thornton, 2002, for a review of evaluations from different countries).

In this paper, we present an analysis of the impact of these measures on the quality of the matching associated jobs found by disabled people. Since, up to our knowledge, there is no previous research on this issue, our study constitutes a novelty in the current literature. The interest of our analysis also lies on the fact that in order to ascertain the job quality of disabled individuals, focusing only on the employment or re-employment probability —which follows from employment promotion measures— would offer a partial analysis. This is so to the extent that, for instance, a precarious entry into the labour market is considered a success when compared to unemployment, even in spite of the fact that workers with a poor quality match might be very likely to be dismissed in the first place whenever a crisis begins. That is, not only the entry or re-entry into employment is a subject of concern when analyzing disabled people's labour careers. For this reason, our focus will not consider the effects of employment promotion measures on their job finding success, but on their job quality success. For this purpose, two proxies for the job matching quality are used. On the one hand, the type of contract —either open-ended or temporary— constitutes an 'objective' or 'demand-side' measure to the extent that it is sensible to assume that open-ended contracts are associated to better matching quality than temporary contracts. On the other hand, a

'subjective' or 'supply-side' measure is given by the knowledge of whether or not disabled workers are searching for another job. We will assume that whenever workers are searching for another job, they internally are considering that their job matching quality can be improved, and that, as a result, that their job matching quality is lower.

We use data from a Spanish survey launched in the year 1999 which is especially designed to obtain a representative picture of people with disabilities in Spain, which includes information on employment in a similar way as other labour force surveys collect. This information takes into account whether the current job has been obtained through an employment promotion programme, and whether the disabled worker has used labour market intermediation services specifically designed for people with disabilities. An important characteristic of this survey is that disability is not defined as 'disability to work' but as 'disability to day-to-day activities' —following the recommendations of the World Health Organization— and, therefore, the self-justification bias will not contaminate the results.

The impact of employment promotion measures on the quality of the job match will be estimated through a non-experimental approach, in particular, matching evaluation techniques. The rationale underlying this empirical method is to compare two groups of homogeneous individuals. Our findings show that employment promotion measures do not affect job matching quality of disabled workers. In addition, the use of specialized labour market intermediation services presents no significant impact either on the probability to obtain an open-ended contract or to search for another job, although a negative effect on finding an open-ended contract is obtained with some cases.

The remainder of the paper is as follows. In the next section, we describe the theoretical framework. The third section describes the data set and the main variables of the empirical analysis, especially the definition of disability in the survey. The fourth section explains the details of the evaluation methodology. The fifth section presents and discusses the empirical results. The final section presents the main conclusions.

2. Theoretical framework: employment promotion measures and quality of the job match

Employment promotion measures for disabled people can be considered as a particular case of active labour market policies. The microeconomic effect which is most likely to arise from the latter is an improvement in the probability of exiting from unemployment (or, in general, from non-employment). This result would be attained through the lower

costs of hiring disabled workers due to special contracts with lower social security contributions and, in general, lower non-wage costs of those newly hired workers..

Another way of improvement disabled workers' mobility out of non-employment is to change their order in the 'queue' of unemployment by giving them a sort of preference over non-disabled unemployed individuals. This is the case with a quota system, whereby a percentage of the new vacancies is reserved for people with disabilities. The rationale behind this system is that prejudices or lack of information prevent firm to hire disabled workers even when they have the same productivity as the remainder of workers. The implementation of labour market intermediation services specifically designed for disabled individuals has a similar rationale: to improve their entry or re-entry into the labour market. Given that these intermediation services increase the information available both for firms and for workers, the unemployment level related to the mismatch will presumably be lower. International evidence also shows that assistance with job search and placement planning (i.e., specialized labour market intermediation) is effective in improving placement rates and placement outcomes (Corden and Thornton, 2002). In addition, they can reduce the problems related to the existence of asymmetric information which are relevant for parties to any labour contract, but it can be a particular issue for people with disabilities. As Livermore et al. (2000) remark, disability is another dimension of uncertainty to the hiring decision, which might discourage employers from hiring workers with disabilities. Finally, the promotion of sheltered employment centres pretends to increase the probability of moving towards employment, because in these centres there is a preference to hire workers with disabilities.

However, these policies' impact goes beyond the aforementioned effects already stated. As such, employment promotion measures can potentially improve the quality of the job match held by workers with disabilities. The main reason for this is that those workers with a worse job match will tend to be fired in the first instance during crises. Therefore, should employment promotion measures increase job matching quality, they would also be indirectly lengthening the duration of the underlying labour relationship and improving the stability of their labour careers —with the correspondent consequences in terms of their social inclusion. On the contrary, if a negative effect of these measures on job matching quality is found then labour market integration of disabled individuals will be worsened.

There are many previous works (almost all of them influenced by Jovanovic, 1979) which model the labour market as different processes of job matching between jobs and workers. In spite of the fact that most of these optimal job matching models focus on the individual job search and are very abstract, they provide a theoretical framework which is useful to understand firm behaviour related to the hiring workers with disabilities and the quality of their job match (see Livermore et al., 2000).

As regards the employment promotion measures for people with disabilities, we can deduce the following hypotheses. First, measures which consist of reductions in hiring costs —reduction in non-wage costs, lower social security contributions, etc.— might impact job match quality in two different ways. On the one hand, if there is a problem of prejudices or lack of information, this financial incentive might encourage firms to hire those disabled individuals who are as productive as non-disabled ones, and, therefore, provide them with a high quality job match. On the other hand, the financial incentive might promote hires of those disabled individuals who are less productive than non-disabled ones, given the compensation which underlies the financial incentive of the contract. Presumably, in this latter case, those individual with a poorer match will be fired in the first instance during economic crises.

Second, measures related to protected employment (as, for instance, quota systems) will affect job match quality in as much as they imply a reduction in the amount of workers available for some jobs. Under a quota system, the firm needs to fill a vacancy by considering only a (reduced) subgroup of the labour force (disabled individuals). Even taking into account the fact that these individuals might be negatively affected by discrimination based on prejudices or lack of information, reaching the same quality for the job match at a low cost of candidates' selection will be presumably more difficult. As a result, these measures will be likely to exert a negative impact on job matching quality. A similar problem arises with sheltered employment centres, but even more pronounced, given that a high proportion of the staff needs to be selected among disabled individuals. The expected result will be that these centres' average productivity will be lower. If these centres are not protected from competition in the markets of goods and services, the lower quality of job matching will be fatal for their survival as productive organizations. On the contrary, if they are protected from the free market, they are likely to become an end for disabled individuals' labour careers, given that —

as they are not real firms— they are not able to provide the experience required by 'normal' firms[1].

Third, specialized labour market intermediation for disabled individuals might improve the quality of the match. The main reason is that this intermediation provides expertise and information to both firms and workers, thereby, decreasing unemployment and the duration of vacancies related to mismatch.

To summarize, different effects are expected according to the different measures, to the extent that some measures may exert both positive and negative effects on the quality of the job match. Therefore, only an empirical analysis can offer us useful information on the effects of the employment promotion measures for disabled individuals on the quality of the job match.

3. Data and descriptive analysis

The database used in this article is the Survey on Impairments, Disabilities and Health State (*Encuesta sobre Deficiencias, Discapacidades y Estado de Salud*, EDDES) launched in 1999. This survey was designed to obtain representative data of the Spanish population and the incidence of disabilities in the Spanish population. For this purpose, the sample size was increased up to 79,000 households (around 250,000 individuals).

A similar survey was launched in Spain in 1986. In spite of the fact that this previous survey was very useful to obtain basic information about disabled individuals in Spain, its information on employment (and, in general, on the labour market participation of people with disabilities) was rather limited. However, this experience was useful to include a battery of questions related to employment and labour market in the EDDES-1999. In addition, employment information was obtained (when possible) following the methodology of the Spanish Labour Force Survey. Information on the use of the different measures of employment promotion was obtained through direct questions on the disabled individuals' current job (obviously, this question was not applicable for people without disabilities).

An important characteristic of the survey is its definition of disability. This definition follows the recommendations of the World Health Organization, which defines the

[1] For the Spanish case, Malo and Rodríguez (2000) explain that sheltered employment centres are usually the end of the labour careers of disabled individuals who are working there. There even exist incentives

disability as a consequence of impairments on the functioning and activity of the individual. Therefore, disabilities are related to the individual, while impairments are related to problems of organs or biological systems of the body. In fact, sometimes impairments are compensated by the reception of technical help, in which case, impairments are finally not related to disability thanks to such help[2]. Therefore, the information provided by the EDDES-1999 is comparable to other surveys which follow the international definitions of the World Health Organization.

In addition, this definition of disability eludes the self-justification bias, since people can not self-classify themselves as disabled for work. That is, instead of asking whether the individual is disabled for working, information on disability is attained through questions on different aspects of the individual's day-to-day activities. In particular, the questionnaire asks for some diary activities included in a closed list of items, and whether or not the individual is limited to perform such activities[3]. There are 36 activities aggregated in the following groups: seeing; hearing; communicating; learning and application of knowledge; moving inside the household; using arms and legs; moving outside the household; caring himself/herself; doing housework; and capability to relate to other people. Therefore, the answer to any of these questions is not necessarily related to disability to work, eluding the endogeneity bias of other surveys —as, for instance, the Health and Retirement Survey of the US[4]. The question of the Health and Retirement Survey is the following: *Do you have any impairment or health problem that limits the amount of paid work you can do? If so, does this limitation keep you from working altogether?* Obviously, the results of any analysis on employment of disabled people will be heavily biased by the use of this question to classify individuals as disabled or non-disabled.

Another advantage of the definition used in the EDDES-1999 is that it does not include chronic illness. This distinction is important because the effects of chronic illness and disability may be different. For example, an individual who suffers from diabetes might develop a severe problem of vision related to this chronic illness, but many other

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for sheltered employment centres in order not to promote the transition of their 'best' workers to the 'normal' firms, since average productivity of these centres will then be negatively affected.

[2] The most common example is that glasses compensate the impairment of myopia. Otherwise myopia would affect the normal activity of the individual because of its associated vision problems. In fact, as the case of glasses is an extended situation, they are included in the survey only when the vision problems are very severe.

[3] Therefore, the answers could be considered as subjective mobility.

[4] See, for example, Benítez-Silva et al. (2000) about how to deal with the endogeneity bias created by the definition of disability as disability to work.

diabetic individuals will not have this disability (or even any other disability until they get older). A blind's relationship with the labour market as a consequence of the diabetes will be very similar to the experience of other blinds because of other reasons, and markedly different from other diabetic individuals who are not blind[5].

To sum up, the data base EDDES-1999 provides very reliable information about employment promotion measures for people with disabilities and a definition of disability which is free of the most common problems related in the current literature on disability.

Tables 1 and 2 present some basic information on the most important variables used in the empirical analysis[6]. First of all, the prevalence ratio of disability in Spain is 5 per cent for working age (16 to 64 for males and females) and the distribution by gender is almost fifty-fifty considering working age. Its prevalence, however, is higher for women when including people 65 or more years old (Malo, 2003). As usual, the disability prevalence increases with age, although in the European Southern countries the relative importance of the 55-65 years old group is much higher than in the remainder of EU-15 countries (Malo and García-Serrano, 2001).

It is widely known that the participation rate of disabled individuals is much lower than for the rest of the population. In Spain and for the year 1999, such rate was 32 per cent, while it was 50 per cent for the total population in working age; the employment rate was 24 and 42 per cent, respectively, for such year (Malo, 2003). However, the unemployment rate was much higher for the former group (26 per cent), than for the whole population (16 per cent). Table 1 shows that there are important differences in the number of disabilities suffered among disabled individuals. As expected, inactivity increases with the number of disabilities, while the opposite is true for employment.

Table A.1 shows the description of the main variables of the empirical analysis (considering only wage and salary workers with disabilities). The proxies for the quality of the job match show that 71 per cent of individuals enjoy an open-ended contract, and that 6 per cent are searching for another job. Therefore, there is a clear difference between the demand-side and the supply-side evaluation of the matching quality. In

[5] This is particularly relevant in the special module on disability of the European Labour Force Survey, launched in 2002, where individuals are asked about disability *and* long-term health problems. Therefore, analyses using the European Labour Force Survey will be different from those obtained from surveys using definitions following WHO recommendations (as the Spanish EDDES-1999).

[6] In addition the appendix shows means and standard deviations of all variables.

other words, the subjective (or supply-side) evaluation is better than the objective (or demand-side) one. In addition, there exists a relatively low frequency of the use of employment promotion measures: from 1 to 5 per cent. We have defined a dummy indicating the beneficiary status of any of such measures, which shows that almost one fifth (18 per cent) of the wage and salary disabled workers are benefiting from them. Finally, 9 percent of disabled individuals have used specialized labour market intermediation in order to obtain their current job.

Table 2 offers the distribution of measure beneficiary status and the use of specialized intermediation services by the type of contract and by the fact of searching for another job. Approximately, 80 per cent of disabled individuals with a temporary contract are not recipients of any employment promotion measure, while this percentage raises up to approximately 83 per cent among disabled individuals with open-ended contracts. On the other hand, addressing to specialized intermediation is more likely for frequent among individuals holding temporary contracts (12.32% versus 7.85%). Finally, the use of specialized intermediation does not markedly differ according to whether individuals are searching for another job, while being beneficiary of employment promotion measures is more common among individuals with an open-ended contract.

To sum up, the descriptive approach shows a relative low use of employment promotion measures for people with disabilities, but there seems to be non-negligible differences in the proxies of the quality of the job match.

4. Evaluation methodology

4.1. The evaluation problem and non-experimental evaluation techniques

The main purpose of this paper is to assess the impact of employment promotion measures on the quality of the jobs found by disabled people[7]. Assuming that measure recipients were a random sample of all those eligible, a valid approach to estimating the effect of such measures would be to compare the outcome of the former with those of non-recipients. However, where administrators (or firms) are discriminating between the less and better able as a basis for measure selection, this process would bias estimates of estimated effects if it is unobserved by the evaluator. For instance, if firms are taking the best for the measures (i.e., they are 'cream-skimming') measure effects

[7] As one analyst has recently noted: "The task of evaluation research lies in devising methods to reliably estimate [the impact of policy change], so that informed decisions about programme expansion and termination can be made" (Smith, 2003).

would be over-estimated; similarly, measure effects may be underestimated if the firms are targeting measure resources on the least able. In other words, the effect of employment promotion measures can be characterized by two distinct processes: on the one hand, being recipient of the measure; on the other hand, the process determining the outcome (i.e., job quality). The question of selection bias arises when some component of the recipient process is relevant to the process determining job quality. If the relationship between the two processes can be fully accounted for by observable characteristics, selection bias will simply be avoided by including the relevant variables in the equation explaining outcomes. However, if unobservable characteristics affecting participation can also influence outcomes, controlling for differences in observable characteristics does nothing to address the issue of sample selection, and, therefore, the estimated treatment effect will be biased. This is the essence of the selection problem.

In order to assess the effects of employment promotion measures on a recipient individual, one must compare the observed outcome —i.e., the factual outcome— with the outcome that would have resulted had that person not been recipient of the measure —i.e., the so-called counterfactual outcome. However, only the former is actually observed, and this is the reason for the evaluation problem, which, in essence, is one of missing data. To overcome this problem, all approaches to evaluation attempt to provide an estimate of the counterfactual in order to identify measure effects. There are two questions which evaluations might wish to address. The first is what impact programme participation would have on an individual drawn randomly from the population —the average treatment effect (ATE). The second is what impact participation has on individuals who actually participated —the average effect of treatment on the treated (ATT). These two effects are identical if we assume homogeneous responses. However, where we allow for the more realistic scenario of responses varying across individuals, the measures can likewise differ. Both estimates are of interest. While ATT can indicate the average benefit of participation, ATE would be relevant were the policy interest focused on making a voluntary programme compulsory, for example (see Bryson et al., 2002).

Several alternative approaches exists that take explicit account of the selection issue. These can be grouped under the broad headings of experimental and non-experimental approaches. We focus on non-experimental methods because it is according with our dataset. The non-experimental techniques share one thing in common: in the absence of an observable counterfactual, assumptions have to be made to identify the causal effect

of a policy or programme on the outcome of interest. There are broadly two main categories of non-experimental methods: before-after estimators and cross-section estimators. The first one has been widely used in evaluations, but requires longitudinal or repeat cross-section data, which may not be often available[8] as in our case. Our dataset is suitable for cross section estimators, which use non-participants to derive the counterfactual for participants. The method of matching constitutes one way of doing this, along with instrumental variables technique (IV) and the Heckman selection estimator[9]. The general matching method is a non-parametric approach to the problem of identifying the treatment impact on outcomes, which is based on contrasting the outcome of measure recipients with the outcomes of "comparable" non-recipients —i.e., individuals sharing similar characteristics. It assumes that selection can be explained purely in terms of observable characteristics, since the choice of the match is dictated by observable characteristics.

Although the technique was developed in the 1980s (Rosenbaum and Rubin, 1983) and has its roots in a conceptual framework which dates back even further (Rubin, 1974), its use in labour market policy evaluation only became established in the late 1990s. It gained particular prominence following the work of Dehejia and Wahba (1998, 1999) who, in reanalysing a sub-set of the data used in LaLonde's (1986) seminal work which had established the superiority of experimental estimators, indicated that propensity score matching performed extremely well[10]. As we shall see, the method has an intuitive appeal arising from the way it mimics random assignment through the construction of a control group post hoc.

The parameter of interest can be formally described as follows. For any disabled individual, define random variables representing the job quality he would have attained had he been a recipient of employment promotion measures, and the job quality he would have attained had he not. Denote these two potential outcomes by Y_1 and Y_0, and denote employment promotion measure recipient status by a dummy variable, D. For each individual, we observe only $Y= Y_0+(Y_1- Y_0)D$, so Y_0 is not observed for measure recipients, and Y_1 is not observed for non-measure recipients. We might nevertheless

[8] For details, see Heckman and Smith (1999), and Blundell and MaCurdy (1999)

[9] See Heckman, 1995, or Blundell and Costa Dias, 2000 for the IV estimator, and Goldberger (1983) or Puhani (2000), for the Heckman selection estimator.

[10] This work has been subsequently criticised in studies which show that propensity score matching, like other non-experimental techniques, depend on assumptions about the nature of the process by which participants select into a programme, and the data available to the analyst (Smith and Todd, 2003;

still hope to identify certain averages of Y_1- Y_0. The effect of treatment on the treated (Rubin, 1977) is one such parameter:

$$E[Y_1 - Y_0 \mid D=1] = E[Y_1 \mid D=1] - E[Y_0 \mid D=1] \quad (1)$$

This tells us whether, on average, programme recipients benefited or suffered from participation in the employment promotion programme.

Simple comparisons by programme recipient status can be used to estimate $E[Y_1 - Y_0 \mid D=1]$. However, such comparisons do not control for most of the criteria used by employers to choose which disabled to accept. The job quality of non-accepted disabled individuals might therefore provide a poor indicator of what measure recipients would have earned if they had not enjoyed any employment promotion measure. To explore this point further, note that the comparison by employment promotion measure recipient status can be decomposed as follows:

$$E[Y_1 \mid D=1] - E[Y_0 \mid D=0] = E[Y_1 - Y_0 \mid D=1] + \{E[Y_0 \mid D=1] - E[Y_0 \mid D=0]\} \quad (2)$$

This shows that comparisons of job quality by beneficiary status are equal to $E[Y_1 - Y_0 \mid D=1]$ plus a bias term attributable to the fact that job quality of non-recipients are not necessarily representative of what recipients would have enjoyed had they not enjoyed any employment promotion measure. That is, since measure beneficiary choices are likely to be the result of systematic decisions, the sample of individuals who are assigned to an employment promotion measure will not be random. If this is ignored and individuals who pass through employment measures are simply compared with those who did not, the estimates will suffer from bias[11].

4.2. Selection on observables: propensity score matching

The matching method attempts to mimic *ex post* an experiment by choosing a comparison group from among the non-treated individuals such that they are as similar as possible to the treatment group in terms of their observable characteristics. All the outcome-relevant differences between treated and non-treated individuals are captured in their observable attributes, the only remaining difference between the two groups being their treatment status. In this case, the average outcome of the matched non-

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Heckman et al., 1998; Agodini and Dynarski, 2001). Nevertheless, the technique continues to attract attention as a useful evaluation tool in the absence of random assignment.

[11] On the contrary, if employment promotion measure beneficiary status was randomly assigned, then D would be independent of Y_0 and Y_1, implying $E[Y_0 \mid D=0] = E[Y_0]$, and $E[Y_1 \mid D=1] = E[Y_1]$. In this

treated individuals constitutes the correct sample counterpart for the missing information on the outcomes the treated would have experienced, on average, had they not been treated.

This way of overcoming the missing counterfactual rests on the so-called conditional independence assumption between non-treatment variables and the programme participation status *D*. This implies that if one can control for observable differences in characteristics between the treated and non-treated group, the outcome that would result in the absence of treatment is the same in both cases. It allows the counterfactual outcome for the treatment group to be inferred, and therefore, for any differences between the treated and non-treated to be attributed to the effect of the programme:

$$Y_1, Y_0 \perp D \mid X \tag{3}$$

This assumption of selection on observables requires that, conditional on an appropriate set of observed attributes, the distribution of the (counterfactual) outcome Y_0 in the treated group is the same as the (observed) distribution of Y_0 in the non-treated group. Therefore, if the conditional independence assumption holds, the matching process is analogous to creating an experimental dataset in that, conditional on observed characteristics, the selection process is random. Consequently, the distribution of the counterfactual outcome for the treated is the same as the observed outcomes for the non-treated.

However, when a wide range of *X* variables is in use, finding exact matches can be extremely difficult. This obstacle was overcome thanks to an important result showing that matching on a single index reflecting the probability of participation could achieve consistent estimates of the treatment effect in the same way as matching on all covariates (see Rosenbaum and Rubin, 1983). Following Rosembaum and Rubin (1983)[12], distance can be measured in terms of a balancing score *q(X),* defined as a function of the observables such that $X \perp D \mid q(X)$**.** One of such balancing scores is the propensity score, *p(X),* the probability to receive treatment given the set of pre-treatment characteristics:

$$p(X) \equiv \Pr(D = 1 \mid X) = E(D \mid X) \tag{4}$$

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case, the effect of treatment on the treated is also the average treatment effect in the population subject to randomization and can be estimated by simple comparisons.

By definition, treatment and non-treatment observations with the same value of the propensity score have the same distribution of observable (and unobservables) characteristics independently of the treatment status. In other words, for a given propensity score, exposure to treatment is random and therefore, treated and control units should be on average observationally identical. Propensity score thus reduces the high-dimensionality problem to a one-dimensional one: the estimation of the mean outcome in the non-treated group is a function of the propensity score.

However, this problem of reduced chances of finding a match does not disappear entirely with propensity score matching. It is still possible that there will be nobody in the non-treatment group with a propensity score that is 'similar' to that of a particular treatment group individual. This is known as the 'support' problem. In order to overcome it, one has to identify participants who are poorly-matched and then omitting them from the estimation of treatment effects, so that any combination of characteristics seen among those in the treatment group may also be observed among those in the non-treatment group. That is, when there is no support for the treated individual in the non-treated population, this treated individual is dropped from the analysis. The estimated treatment effect has then to be redefined as the mean treatment effect for those treated falling within the common support.

The explicit acknowledgement of the common support problem is one of the main features distinguishing matching methods from standard parametric regressions. The other main distinguishing feature is that matching is non-parametric. Consequently, it avoids the restrictions involved in models that require the relationship between characteristics and outcomes to be specified.

6. Estimations

6.1. Estimating the propensity score

The effects of employment promotion measures refer in this paper to their impact on job quality of measure recipients through two distinct outcomes — (i) having a permanent contract; (ii) searching for another job— relative to what would have happened to recipients if they had not passed through such measures. Since the propensity to participate is unknown, it needs first to be estimated through any standard probability

[12] Another unit-free metric, is the Mahalanobis one, which assigns weight to each coordinate of X in proportion to the inverse variance of that coordinate (see Blundell et al. 2004). See, also, Abadie and Imbens, 2002, and Zhao, 2004, for alternative matching metrics.

model. Our propensity score models (see Table 3) were fitted as two probit regression models—one for each outcome— where the dependent variable indicates being recipient of the corresponding measure, and the independent variables are the factors thought to influence participation and outcome. In this sense, propensity score matching becomes a semi-parametric approach to the evaluation problem (see Imbens, 2004).

As regards the variables to include in the model when estimating the propensity score, we have chosen demographic, qualification, household and disability characteristics that are expected to influence both participation and outcome[13]. In this sense, among the potential predictors of measure beneficiary and subsequent employment outcomes we include the following ones: whether or not the individual suffers only one disability, whether or not he/she has a certificate of disability, gender, age at the date of being hired, marital status, whether or not he/she is the main person in the household, whether or not he/she receives any sort of financial subsidy or benefit, whether or not he/she belongs to a non-profit organization for disabled people, and, finally, some variables related to his/her qualification (having passed through vocational training courses, and the level of education).

In addition, we have taken into account that only variables that are unaffected by participation, or the anticipation of participation, should be included. To do otherwise would be to mask possibly important programme effects, undermining the interpretability of estimated effects (see Heckman, LaLonde and Smith, 1999). In this sense, the aforementioned variables are either fixed over time (e.g., gender) or were collected at the date of eligibility and, as such are unaffected by measure beneficiary[14]. Fortunately, our dataset offers sufficiently rich information on pre-measure variables, which are likely to be important predictors of measure beneficiary and outcomes. The value of these variables is that they contain pre-measure information which may be

[13] If a variable influences only measure beneficiary status, there is no need to control for the differences between the treatment and the comparison group since the outcome variable of interest is unaffected. Conversely, if a variable influences only the outcome variable, there is no need to control for it since it will not be significantly different between the treatment and comparison groups. In addition, should a variable thought to influence outcomes perfectly predict participation, recipients would have a propensity score of 1 and non-recipients a propensity score of 0. In these instances, it may be difficult to get an unbiased estimate of programme impact using propensity score matching.

[14] For this reason, we have excluded two variables from the set of independent variables in the propensity score estimations that, in spite of being available for our analysis, were likely to be affected by participation: firm size and region. The fact that a disabled worker is benefited by some employment promotion measured is likely to allow his contracting firm to hire more workers, given the savings that this hiring may imply for the firm. Similarly, if employment promotion measures are relatively more extended among certain geographical regions, benefiting from such measures may imply that being hired would more likely in certain geographical zones.

critical in estimating measure beneficiary status and post-measure outcomes. Moreover, they may help capture otherwise unobservable characteristics —such as motivation— which might also influence participation and outcomes. Those variables measured before beneficiary status are divided in the following groups:

- The type of disability suffered by the individual (eyesight, hearing, language, understanding, travelling, physiological needs, not capable to do the housework or relationship difficulties)
- Whether the individual suffering any of such disabilities receives help (either technical or personal help)
- Whether the disability's degree of severity is slight or moderate (very severe will be the reference category)
- Age at the date of disability diagnosis

As shown in Table 3, the propensity to being recipient of employment promotion measures is higher than average if disabled individuals are in possession of a certificate of disability, if they are men, if they receive financial subsidies or benefits, if they belong to a non-profit organization for disabled people or if they have followed vocational training courses. In addition, the type of disability suffered becomes a relevant determinant of measure beneficiary, in the sense that those with language disability are more likely to pass through employment promotion measures (as opposed to those with eyesight disability), as well as those with hearing disability receiving help, and those that have understanding or relationship disability to a slight or moderate degree. On the contrary, those with college degree, with a hearing or relationship disability, and with a language disability detected when they were below 16 years-old are less likely to being beneficiary of employment promotion measures.

As regards the alternative type of treatment we are interested in (i.e., specialized intermediation beneficiary), the fact of having a certificate of disability is a positive determinant of being a recipient of such intermediation services, as well as being the head of the household. Moreover, individuals who have followed vocational training courses, those with a travelling disability who have received help, and the ones suffering from physiological needs disability being detected when they were below 16 years-old, are also more likely to being recipients of specialized intermediation. On the

contrary, such type of intermediation is less likely among disabled with college degree, among those with physiological needs disability, those with relationship disability who have received help, and, finally, the ones suffering from household jobs disability detected when they were below 16 years-old.

5.2. Implementing propensity score matching estimators

An estimate of the propensity score is not enough to estimate the average treatment on the treated (ATT) of interest, given that the probability of observing two units with exactly the same value of the propensity score is in principle zero, since *p(X)* is a continuous variable (see equation [4] above). Various methods have been proposed in the literature to identify the comparator group through propensity score matching. Those methods reach different points on the frontier of the trade-off between quality and quantity of the matches and none of them is a priori superior to the others. Thus, their joint consideration in our empirical analysis offers a way to assess the robustness of the estimates. We have used the publicly available Stata command developed by Leuven and Sianesi (2003) that performs the types of propensity score matching presented in this section. Table 4 shows the estimated ATT for the aggregate employment promotion measure, while Table 4 shows the estimated ATT for the specialised intermediation measure. We have imposed with all these methods the common support restriction. This way, we ensure that any combination of characteristics seen among those in the treatment group may also be observed among those in the non-treatment group (as commented on above)[15].

The traditional and most intuitive form of matching is *nearest-neighbour* (or *one-to-one*) matching, which takes each treated unit and searches for the control unit with the closest propensity score. The resulting set of non-treatment individuals constitutes the comparison group. Although it is not necessary, the method is usually applied *with replacement*, in the sense that a control unit can be a best match for more than one treated unit. Matching with replacement in this way is less demanding in terms of the support requirement since individuals in the comparator group who would provide the closest match to a number of treated individuals remain available[16]. Once each treated

[15] We are conscious of the fact that, dong this, high quality matches may be lost at the boundaries of the common support and the sample may be considerable reduced.(see Lechner, 2001).

[16] Should a certain type of individual be common in the treatment group but relatively uncommon in the comparison group, the pool of comparators able to provide a close match would become exhausted in

unit is matched with a control unit, the difference between the outcome of the treated units and the outcome of the matched control units is computed. The ATT of interest is then obtained by averaging these differences[17]. Estimated nearest-neighbour treatment effects for aggregate employment promotion measure beneficiary (Table 4) indicate a positive effect for the outcome of reaching a permanent contract, and a negative effect for the outcome of searching for another job. However, none of these effects is statistically significant. Equally, the estimated ATT for specialized intermediation are non-significant (Table 5), independently of the quality outcome considered.

Some of the matches found through nearest-neighbour might be fairly poor because for some treated units the nearest neighbour may have a very different propensity score and nevertheless he would contribute to the estimation of the treatment effect independently of this difference. The *radius matching* and *kernel matching* methods offer a solution to this problem. With radius matching, each treated unit is matched only with the control units whose propensity score falls in a predefined neighbourhood of the propensity score of the treated unit. If the dimension of the neighbourhood (i.e., the radius) is set to be very small, it is possible that some treated units are not matched because the neighbourhood does not contain control units. That is, the smaller the radius, the more difficult it is to find a match within that range, resulting in a greater number of cases failing the support requirement. On the other hand, the smaller the size of the neighbourhood, the better is the quality of the matches. Results, therefore, may be sensitive to the size of the radius that is the bases for matching. We tested the sensitivity of our results to three radii: 0.005, 0.01 and 0.02.

In kernel-based matching, all treated are matched with a weighted average of *all* controls with weights that are inversely proportional to the distance between the propensity scores of treated and controls. The 'kernel' is a function that weights the contribution of each comparison group member, usually so that more importance is attached to those comparators providing a better match. The most common approach is

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case that matching were carried out without replacement. This is the reason why the technique is only implemented with replacement in our analysis.

[17] A variant of nearest-neighbour is *caliper matching* (see Cochran and Rubin, 1973 or Dehejia and Wahba, 1999). Its defining characteristic is a tolerance it sets when comparing propensity scores, in other words a 'propensity range' in which a match is deemed acceptable. Where the propensity score of a treated individual falls beyond the bound set for a near comparator, this treated individual will remain unmatched. That is, the "calliper" is used to exclude observations for which there is no close match, thus enforcing common support. The appeal of calliper matching is that it imposes a form of quality control on the match. Any treatment group members left unmatched are discarded from the analysis. However, a practical objection to its use is that it will often not be obvious how to set the tolerance.

to use the normal distribution (with a mean of zero) as a kernel, where the weight attached to a particular comparator is proportional to the frequency of the distribution for the difference in scores observed. This means that exact matches get a large weight, and poor matches get a small weight.

As regards the likelihood of attaining a permanent contract from participation in employment promotion programmes (see Table 4), radius matching and the Epanechinov kernel matching offer an estimated positive ATT. The difference of the former with respect to the remainder matching methods might well come from the fact that radius matching only uses those treated that have control matches within radius *r* — while in the nearest neighbour matching procedure, 166 out of 399 treated units are used, in the radius matching 127 (with calliper 0.005), 141 (with calliper 0.01), and 151 (with calliper 0.02) are used[18]. In any case, in Table 4, neither the estimated results with radius matching nor with kernel matching are statistically significant.

As regards the evaluation of specialized intermediation (Table 5), the probability of achieving a permanent contract is significantly lower for treated individuals under the kernel matching method (except for the biweight kernel). The remainder matching techniques offer non-significant estimators.

Thus, as a summary, the propensity score evaluation indicated that participation in employment promotion measures has no significant effect on the job matching quality of disabled individuals. Better chances to find a permanent job —apparent in the raw data see Section 3 of descriptive results— are no longer apparent once participation in employment promotion measures are compared with "like" non-participants. Thus, participants' better outcomes are attributable to comparative advantages that are independent of the employment promotion measures. Moreover, specialized intermediation presents a significant negative impact on the probability of finding a permanent job, but this effect is non-robust to the matching technique implemented.

A possible interpretation of these results is that disabled workers who are beneficiaries of employment promotion measures or specialized labour market intermediation will enjoy a similar job matching quality than others disabled workers. Therefore, these active labour market policies will have neither an adverse effect on separation probabilities under an economic crisis nor a positive effect. It is important to remark that our results do not say anything about the job matching quality of the disabled

[18] If the radius were chosen to be very small, many treated units would not be matched, and the results would no longer be representative of the population treated (see Smith and Todd, 2003).

workers compared to the average job matching quality of non-disabled workers[19]. Finally, if we regard having an open-ended contract and not searching for another job as signals of a long-term attachment to the labour market (in special the type of contract), we must conclude that these policy measures do not improve the disabled individuals' long-term attachment to the labour market.

5.3. Assessing the quality of the matching

The final step to applying matching should be to test the resulting matching quality in terms of covariate balance in the matched groups. To be effective, matching should balance characteristics across the treatment and matched comparison groups. A measure of the bias can be calculated for each characteristic in order to achieve a standardised indicator of the degree to which the matching has been successful in balancing (see, for example, Sianesi, 2001)[20].

Tables 6 and 7 provide some diagnostics on the performance of the match for employment promotion measures and specialized intermediation. Each cell represents the percent reduction in the bias (between members of the treatment group and those of the comparison group) for significant covariates used to model treatment status. The bias is the difference of the sample means in the treated and non-treated (full or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (formulae from Rosenbaum and Rubin, 1985). Ideally, one would have a 100 percent reduction in every significant covariate. As can be observed, while in Table 6, the bias is not reduced for some covariates in matching techniques 5, 7 and 8 (the corresponding cell figures are negative), in Table 7, this occurs for the first three matching methods. In addition, note also that reduction in covariate imbalance is not especially high in some variables for the nearest-neighbour technique (in fact, the nearest method, as previously underlined, may at times turn out to be quite apart). Therefore, we can conclude that matching quality is better for radius matching concerning evaluation of employment promotion measures, while it is better for kernel matching concerning evaluation of specialized intermediaries.

[19] Blázquez and Malo (2004) analyze the influence of being disabled on educational mismatch using Spanish data of the ECHP and they conclude that disability does not significantly affect to mismatch.

[20] A similar approach can be adopted to assist with specifying the participation model. The balancing test was proposed in Rosenbaum and Rubin (1983) and applied in, for example, Dehejia and Wahba (1998). However, the balancing test does not aid in solving the variable selection problem. It only aids in model specification for a given set of conditioning variables. It cannot provide any indication as to whether the

7. Conclusions

In this article we have presented an analysis about the effect of employment promotion measures for people with disabilities on the quality of their job matching. This is a novelty in current literature because previous research has mainly focused on the disincentive effects on activity of income transfers related to disability. The analysis of the quality of the job matching is linked to the attachment to the labour market of people with disabilities. Those with a poor match will presumably suffer a higher dismissal probability in an economic crisis, worsening their welfare and creating disruptions in their labour career. Anyway, as Jovanovic (1979) explains the job is an experience good and when the quality of the job match is low the separation probability (by either dismissal or quit) will increase. Therefore, it is relevant to know whether policy programmes intended to improve labour market participation of people with disabilities have a positive or a negative effect (or any effect at all) on job match quality.

In addition, our empirical analysis uses a Spanish database specially launched to have an accurate picture on people of disabilities and their relationship with the labour market. Following the recommendations of the World Health Organization, this database uses a definition of disability related to disability to day-to-day activities and not to disability to work, and therefore eliminating the potential bias of self-reported disability measures (which is an endogeneity bias[21]). Therefore, the data are especially suitable for the objective of our empirical analysis.

To evaluate the effects of the employment measures on the quality of job matching of people with disabilities we have applied the non-experimental evaluation methodology called as matching analysis. We have used two variables as proxies of such quality: the type of contract and whether the worker is searching another job or not. The first can be understood as a 'demand-side' valuation of the quality job match, because the type of contract is mainly (and usually) decided by the firm and the worker accepts or not the type of contract offered by the firm. The second one is a 'supply-side' valuation, because workers who search for another job are denoting that there is not a perfect match between their current positions and their optimal one in their current firm.

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conditional independence assumption is plausible, merely whether the matching has been successful in balancing the characteristics included in the model across the treatment and comparison groups.

The main results show that employment promotion measures do not improve the match's job quality (either valuated from the demand or the supply side). Furthermore, the use of specialized labour market intermediation services by disabled individuals does not affect, in general, their job matching quality.

The policy implications of these results are not pessimistic but neither optimistic. These measures have not negative effects on the quality of the job match and, therefore, they are not stepping stones on the labour careers of workers with disabilities. However, they are not increasing the job match quality respect to those workers who are not beneficiaries of these measures. This is especially worrying for specialized services for labour market intermediation which should increase not only the probability of participation into the labour market but the quality of the job match too. Maybe, employment promotion measures and intermediation services are mainly focused to solve short-term problems related to the access to job, and they should add a wider perspective considering the labour career prospects of people with disabilities.

[21] See, for example, Benítez-Silva et al. (2000) for an extensive discussion about self-reported bias and endogeneity in disability research.

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Table 1. Participation in the labour market and number of disabilities

	1	2	3	4 or more	Total
Employment	39.22	30.69	23.47	12.68	23.88
Unemployment	10.67	9.95	9.71	6.05	8.36
Inactivity	50.11	59.37	66.82	81.27	67.76
Total	100.00	100.00	100.00	100.00	100.00

Source: EDDES-1999 and authors' calculations

Table 2. Distribution of being beneficiary of an employment promotion measure or a specialized intermediation service considering the quality of the job matching (type of contract and searching for another job)

	Employment promotion measures			Specialized intermediation services		
Type of contract	**Yes**	**No**	**Total**	**Yes**	**No**	**Total**
Temporary	19.75	80.25	100.00	12.32	87.68	100.00
Open-ended	16.90	83.10	100.00	7.85	92.15	100.00
Total	17.72	82.28	100.00	9.13	90.87	100.00
Searching for another job	**Yes**	**No**	**Total**	**Yes**	**No**	**Total**
Yes	10.72	89.28	100.00	9.89	90.11	100.00
No	18.16	81.84	100.00	9.08	90.92	100.00
Total	17.72	82.28	100.00	9.13	90.87	100.00

Source: EDDES-1999 and authors' calculations

Table 3. Propensity score estimations (probit models)

	Empl. Promotio		Intermediation	
Variables	Coef.	z	Coef.	Z
Only one disability	0.007	0.040	0.038	0.160
Disability certificate	**1.080**	**6.790**	**1.006**	**4.870**
Gender (1=Men)	**0.537**	**3.210**	0.168	0.830
Age	-0.013	-0.330	-0.084	-1.650
Age squared	0.000	-0.010	0.001	1.110
Marital status (1=married)	-0.256	-1.420	-0.169	-0.760
Head of the household (1=Yes)	-0.163	-0.900	**0.496**	**2.170**
Household size	0.020	0.380	0.108	1.740
Financial subsidies or benefits (1=Yes)	**0.386**	**2.610**	0.098	0.540
Belongs to a non-profit organization for disabled people	**0.671**	**3.860**	**0.875**	**4.450**
Has followed vocational training courses	**0.333**	**1.970**	**0.665**	**3.520**
Primary studies	0.105	0.490	0.032	0.130
Middle school	-0.187	-0.620	-0.080	-0.220
Vocational training	-0.395	-1.200	-0.315	-0.790
College degree	**-0.575**	**-1.960**	**-0.907**	**-2.250**
Eyesight disability	-	-	-	-
Hearing disability	**-0.784**	**-2.050**	0.574	1.430
Language disability	**0.657**	**1.780**	-0.053	-0.110
Understanding disability	0.576	0.620	-0.363	-0.360
Travelling disability	0.309	1.180	0.502	1.590
Physiological needs disability	0.197	0.210	**-1.669**	**-1.760**
Household jobs disability	0.437	1.090	0.655	1.470
Relationship disability	**-2.582**	**-2.700**	0.200	0.240
Eyesight disability & receives help	0.475	1.550	0.424	1.220
Hearing diability & receives help	**0.832**	**2.720**	-0.028	-0.070
Language disability & receives help	-0.089	-0.140	0.182	0.280
Understanding disanility & receives help	0.213	0.310	1.015	1.270
Travelling disability & receives help	0.207	1.110	**0.434**	**1.900**
Physiological needs disability & receives help	0.247	0.290	-	-
Household jobs disability & receives help	-0.382	-1.110	-0.291	-0.770
Relationship disability & receives help	-0.902	-0.970	**-2.057**	**-2.110**
Eyesight disability & slight or moderate severity	-0.425	-1.530	-0.141	-0.410
Hearing diability & slight or moderate severity	0.441	1.270	-0.099	-0.270
Language disability & slight or moderate severity	-0.477	-0.870	0.022	0.040
Understanding disability & slight or moderate severity	**1.061**	**2.020**	-0.417	-0.790
Travelling disability & slight or moderate severity	0.059	0.320	-0.306	-1.390
Physiological needs disability & slight or moderate severity	-0.343	-0.520	**1.824**	**1.770**
Household jobs disability & slight or moderate severity	0.544	1.750	0.268	0.800
Relationship disability & slight or moderate severity	**1.978**	**2.380**	-0.233	-0.320
Eyesight disability & under 16	0.211	0.910	0.342	1.260
Hearing diability & under 16	0.385	1.260	-0.490	-1.270
Language disability & under 16	**-0.835**	**-1.770**	0.284	0.510
Understanding disanility & under 16	0.228	0.260	1.425	1.410
Travelling disability & under 16	0.169	0.840	0.027	0.110
Physiological needs disability & under 16	0.208	0.350	0.389	0.570
Household jobs disability & under 16	-0.599	-1.640	**-0.731**	**-1.780**
Relationship disability & under 16	1.668	1.640	-0.031	-0.030
Constant	-1.889	-2.130	-1.403	-1.320

Observations	975	975
LR chi2(46)	394.98	252.29
Log likelihood	-256.766	-176.283
Prob > chi2	0.000	0.000

Table 4. Average treatment effect on the treated. The treatment refers to having been recipient of any employment promotion measures

OUTCOME:	Matching method	Treated	Untreated	Difference
Reaching a permanent contract	Nearest-neighbour matching method with replacement & common support	.6927	.6385	.0542 (.1023)
	Radius matching with common support			
	Caliper 005	.7244	.6888	.0356 (.1032)
	Caliper 0.01	.7234	.6795	.0438 (.1119)
	Caliper 0.02	.7086	.7059	.0026 (.1082)
	Kernel matching with common support			
	Epanechnikov kernel	.6927	.6890	.0037 (.1024)
	Gaussian kernel	.6927	.6983	-.0055 (.1040)
	Biweight kernel	.6927	.6939	-.0011 (.1037)
	Uniform kernel	.6927	.6834	.0092 (.1080)
Searching for another job	Nearest-neighbour matching method with replacement & common support	.0602	.0963	-.0361 (.0518)
	Radius matching with common support			
	Caliper 005	.0708	.1228	-.0519 (.1041)
	Caliper 0.01	.0638	.0694	-.0055 (.0989)
	Caliper 0.02	.0596	.0587	.0008 (.1029)
	Kernel matching with common support			
	Epanechnikov kernel	.0602	.0546	.0055 (.1011)
	Gaussian kernel	.0602	.0566	.0035 (.1056)
	Biweight kernel	.0602	.0548	.0054 (.1049)
	Uniform kernel	.0602	.0537	.0064 (.1085)

Note: The entries in brackets are the bootstrapped standard errors (500 replications)

Table 5. Average treatment effect on the treated. The treatment refers to having access to employment through specialized intermediation services for disabled people

OUTCOME:	Matching method	Treated	Untreated	Difference
Reaching a permanent contract	Nearest-neighbour matching method with replacement & common support	.5909	.6932	-.1023 (.0995)
	Radius matching with common support			
	Caliper 005	.6323	.6908	-.0585 (.0664)
	Caliper 0.01	.6164	.6645	-.0481 (.0659)
	Caliper 0.02	.6266	.7129	-.0863 (.0624)
	Kernel matching with common support			
	Epanechnikov kernel	.6000	.7021	-.1021 (**.0642**)
	Gaussian kernel	.5909	.7015	-.1106 (**.0656**)
	Biweight kernel	.6000	.7037	-.1037 (.0735)
	Uniform kernel	.6000	.6973	-.0973 (**.0587**)
Searching for another job	Nearest-neighbour matching method with replacement & common support	.0795	.1022	-.0227 (.0647)
	Radius matching with common support			
	Caliper 005	.0882	.0898	-.0015 (.0686)
	Caliper 0.01	.0821	.0814	.0007 (.0764)
	Caliper 0.02	.0800	.0787	.0012 (.0678)
	Kernel matching with common support			
	Epanechnikov kernel	.0823	.0652	.0170 (.0670)
	Gaussian kernel	.0795	.0640	.0155 (.0636)
	Biweight kernel	.0823	.0659	.0163 (.0621)
	Uniform kernel	.0823	.0642	.0180 (.0751)

Note: The entries in brackets are the bootstrapped standard errors (500 replications).

Table 6. Percent reduction in covariate imbalance after propensity score matching for employment promotion measures.

	MATCHING METHODS							
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disability certificate	98.0	96.8	97.1	99.9	99.3	96.9	99.6	98.9
Gender (1=Men)	52.5	86.1	56.3	46.7	78.3	69.8	77.7	82.4
Financial subsidies or benefits (1=Yes)	93.6	88.3	92.2	95.5	97.5	95.3	98.7	94.3
Belongs to a non-profit organization for disabled people	100.0	97.7	95.1	93.4	97.2	99.0	97.7	95.8
Has followed vocational training courses	49.2	88.0	61.8	72.1	71.3	80.2	71.3	70.6
Collage degree	62.0	60.7	69.5	73.7	81.4	87.6	79.8	82.0
Hearing disability	85.8	93.0	95.7	98.8	82.3	88.7	80.0	89.1
Language disability	73.6	91.6	96.0	98.7	74.6	80.3	74.4	78.7
Relationship disability	51.3	86.7	71.7	78.3	69.3	77.0	65.7	74.4
Hearing disability & receives help	6.7	81.6	72.6	85.0	-34.4	18.9	-33.2	-25.1
Understanding disability & slight or moderate severity	100.0	99.1	85.8	88.9	84.7	96.2	89.4	76.5
Relationship disability & slight or moderate severity	100.0	52.9	66.1	86.5	86.7	86.6	88.5	84.1
Language disability & under 16	64.4	83.2	89.7	89.4	72.5	82.1	69.0	78.3

Notes: This table shows the convergence of mean values on key variables in the propensity store equation, with non/participant characteristics closely resembling those of participants alter matching. The bias is the difference of the sample means in the treated and non-treated (full or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (formulae from Rosenbaum and Rubin, 1985).

(1) Nearest-neighbour matching method with replacement & common support
(2) Radius matching with common support, caliper 0.005
(3) Radius matching with common support calliper 0.01
(4) Radius matching with common support, calliper 0.02
(5) Epanechnikov kernel
(6) Gaussian kernel
(7) Biweight kernel
(8) Uniform kernel

Table 7. Percent reduction in covariate imbalance after propensity score matching for specialized intermediaries.

	MATCHING METHODS							
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disability certificate	94.0	93.5	97.0	98.9	98.4	91.6	98.3	98.6
Head of the household (1=Yes)	89.1	87.4	87.4	72.6	57.4	73.5	56.4	57.5
Belongs to a non-profit organization for disabled	83.4	96.1	98.0	97.4	95.3	97.7	95.3	95.4
Has followed vocational training courses	93.4	90.2	83.8	89.0	83.2	83.4	86.7	78.5
Collage degree	100.0	98.1	99.2	91.8	86.7	91.2	87.4	86.8
Physiological needs disability	2.3	57.3	49.8	55.9	36.8	37.3	30.6	46.9
Physiological needs disability & receives help	49.2	73.9	71.1	85.7	97.0	98.7	93.5	98.4
Relationship disability & receives help	-75.8	-64.9	-216.1	16.5	83.2	65.3	73.9	92.7
Physiological needs disability & slight or moderate severity	-16.5	82.7	50.1	60.5	53.2	56.5	42.6	69.8
Household jobs disability & under 16	55.0	93.6	94.3	90.1	85.6	92.3	85.1	84.8

Notes: This table shows the convergence of mean values on key variables in the propensity store equation, with non/participant characteristics closely resembling those of participants alter matching. The bias is the difference of the sample means in the treated and non-treated (full or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (formulae from Rosenbaum and Rubin, 1985).

(1) Nearest-neighbour matching method with replacement & common support
(2) Radius matching with common support, caliper 0.005
(3) Radius matching with common support calliper 0.01
(4) Radius matching with common support, calliper 0.02
(5) Epanechnikov kernel
(6) Gaussian kernel
(7) Biweight kernel
(8) Uniform kernel

APPENDIX

Table A.1. Means and Standard Deviations in the sample of wage and salary workers with disabilities

Variable	Weight	Mean	Std, Dev,	Min	Max
Open-ended contract	1021	0.71	0.45	0	1
Searching for another job	1021	0.06	0.24	0	1
Being disabled	1021	0.38	0.48	0	1
Official certificate of being disabled	1021	0.33	0.47	0	1
Gender (1= Male)	1021	0.67	0.47	0	1
Age	1021	42.97	11.81	16	64
Age Squared	1021	1985.50	1007.95	256	4096
Civil Status (1=Married)	1021	0.63	0.48	0	1
Head of the household (1=Yes)	1021	0.59	0.49	0	1
Household size	1021	3.61	1.38	1	10
Receiving any sort of benefit or subsidy (1=Yes)	1021	0.23	0.42	0	1
Being a member of a NGO related to disability	1021	0.15	0.36	0	1
Region: South	1021	0.18	0.39	0	1
Region: East Coast	1021	0.34	0.47	0	1
Region: North Coast	1021	0.16	0.36	0	1
Region: Ebro River	1021	0.04	0.21	0	1
Region: Centre (Castilla and Madrid)	1021	0.22	0.42	0	1
Region: Balearic and Canary Islands	1021	0.05	0.23	0	1
Vocational training courses (1=Yes)	1021	0.17	0.37	0	1
Illiterate or without studies	1021	0.13	0.34	0	1
Primary Level and Compulsory Secondary Level	1021	0.58	0.49	0	1
Non-compulsory Secondary Level	1021	0.09	0.28	0	1
Vocational Training	1021	0.06	0.24	0	1
University or Postgraduate Studies	1021	0.14	0.34	0	1
Firm Size: <10 workers	1021	0.31	0.46	0	1
Firm Size: 10-49 workers	1021	0.26	0.44	0	1
Firm Size: 50-500 workers	1021	0.20	0.40	0	1
Firm Size: 500 workers or more	1021	0.23	0.42	0	1
Empl. Prom.: Training contract (1=Yes)	1021	0.04	0.19	0	1
Empl. Prom.: Fiscal incentives for contracts	1021	0.03	0.17	0	1
Empl. Prom.: Quota in the public sector	1021	0.02	0.12	0	1
Empl. Prom.: Quota in the private sector	1021	0.04	0.19	0	1
Empl. Prom.: Special help for re-entry in the labour market	1021	0.01	0.11	0	1
Empl. Prom.: Subsidies	1021	0.01	0.09	0	1
Empl. Prom.: Sheltered employment centres	1021	0.05	0.22	0	1
Empl. Prom.: Beneficiary of any of the previous measures	1021	0.18	0.38	0	1
User of specialized labour market intermediation to obtain the current job (1=Yes)	1021	0.09	0.29	0	1

Source: EDDES-1999 and authors' calculations