Employment Subsidies, Informal Economy and Women's Transition into Work in a Depressed Area: Evidence from a Matching Approach

Manuela Deidda
CRENoS

Adriana Di Liberto*
University of Cagliari and CRENoS

Marta Foddi
CRENoS

Giovanni Sulis
University of Cagliari and CRENoS

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Abstract:

We analyse the effects of an active labour market program for disadvantaged workers recently implemented in an Italian depressed area. Our sample includes 859 workers, mostly women, who entered the program before April 2008 and were subsequently interviewed in 2009-10. We complement the existing administrative data with survey data that enables us to control for numerous individual and labour market characteristics for both treated and non-treated individuals. Using propensity-score matching methods, we do find that the employment subsidy had a positive and significant effect (ATT) on both the probability of finding a job for participants and on their level of income. We also control for effect heterogeneity and find that the outcome of the policy was higher for women and, among them, we also find that the program was more effective on less educated and older female workers. Finally, we exploit unique information on previous contacts between workers and firms and on the use of informal channels for job search activity to explore the role of underground employment relations for the effectiveness of the policy.

Keywords: Active Labour Market Programs, Female Labour-force participation, Employment Subsidies, Propensity Score Matching, Informal networks.

JEL Classification: C14, C83, J64, J16.

*Corresponding author: Adriana Di Liberto, Dipartimento di Scienze Economiche e Aziendali, Università di Cagliari, Viale S. Ignazio 17, 09123 Cagliari, Italy. Tel.+070 6753765, Fax +070 6753760. E-mail: diliberto@unica.it.

1 Introduction

The participation of women in the labour market is a classic topic in the labour economics literature (see for example Killingsworth and Heckman, 1986) and gender issues are also receiving increasing emphasis on the policy agenda. A leading example in this respect is the so called "Lisbon Strategy" of the European Union that set the target of 60% for women's employment rates to be reached by 2010. Still, across European countries we find highly heterogeneous conditions and the EU clearly indicated the need for structural welfare and labour market reforms to reach the target.

As Bergemann and Van den Berg (2008) suggest in their recent survey of the literature on the effects of the active labour market programs (ALMP's henceforth) for women in Europe, impact evaluation analysis is of primary importance to deepen our knowledge of the driving forces between gender differences in participation rates or income levels. Despite that, the literature on the effectiveness of ALMPs on employment and participation outcomes of women is not vast and, as also stressed by Card et al. (2011), "…*few of the programmes are targeted by gender: rather, in cases where gender-specific estimates are available it is because the authors have estimated separate impacts for the same programmes on men and women.*"[1]

Moreover, most of the studies surveyed by Bergemann and Van den Berg (2008) on the effects of various types of programs find higher effects for women than for men with a positive correlation between the magnitude of the positive effects on employment outcomes for women and the gender gap in labour market participation.[2] For example, Gerfin and Lechner (2002) find a more favorable effect of both training and employment subsidies for women, while Gerfin et al. (2005) show that (temporary) employment subsidies for jobs in competitive markets are more effective than standard ones in non-profit or public sector jobs. Interestingly, even if not positive, outcomes of men and women are also differentiated in Kluve et al. (1999): labour market policies have very small but insignificant effects for women, while for men ALMPs are associated with negative employment probabilities after the treatment.[3] Finally, more recently Caliendo and Kunn (2011b) focus on self-employment (start-up subsidies) schemes and find that a general gender gap in terms of effects of these innovative ALPM programs exists but results may vary depending on the choice of the outcome variable. In particular, they find higher employment effects for female than men but smaller income effects, suggesting that in monetary terms men still gain more from program participation than their female counterpart.[4]

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[1] See Card et al. (2011), p. F460. The recent paper by Kluve (2010) conducting meta-analyses on the effectiveness of ALMP in various countries does not even discuss the relevance of gender issues.

[2] The programs discussed are skill training, job search assistance, employment subsidies and monitoring with sanctions, while, typically, the outcome variables are the employment probabilities or transition rates to work, less frequently it is the level of income or duration of unemployment spells.

[3] It should be noted that Kluve et al (1999) study a series of treatments and not a single type of intervention. Lalive et al. (2008) show that (temporary) subsidized jobs can have positive effects on the transition probability out of unemployment, but they do not clearly distinguish between hiring subsidies and pure job creation.

[4] They follow unemployed individuals in Germany for nearly five years after entering one of two different start-up programs that are an integrative part of the national ALMP system.

In principle, active labour market programs directly targeted at increasing the probability of unemployed workers to get back to work could have a relatively larger and positive effect on women especially in a low participation country. As Bergemann and Van den Berg (2008) discuss, this may happen for various reasons. First, women could have a higher elasticity of labour supply, hence increasing their chances in the labour market can result in a relatively stronger effect for this group of workers. Second, they may have higher reservation wages and higher outside options to stay out of the labour market, as long as ALMPs increase the arrival rate of offers, this will result in higher marginal benefits for them. Third, in markets in which discrimination is important, and labour market attachment of women is low, ALMPs can help women to signal their productivity thus increasing their employment probabilities.[5]

The need for gender specific labour market policies is particularly pressing in Italy. Of 135 countries, the Global Gender Gap Report ranks Italy only in 90th place in Economic Participation and Opportunity as women are much more likely to be unemployed (or be out of the labour force) and to earn less than men.[6] Between 2000 and 2010 Italian female participation rates grew from 47 to 52% reaching a percentage more similar to that observed on average in OECD countries a few decades ago than to the 2010 average, which was equal to 64%.[7] Further, the average employment rate for Italian female workers in 2010 was 46%, well below the abovementioned European Lisbon strategy target of 60%. Conversely, the average employment male rate was 69%, that is, the 70% Lisbon strategy target for men was attained in almost every area of the country. Finally, NUTS2 regional data show that only two out of twenty Italian regions reached the 2010 target. In sum, across industrialised countries Italy has one of the lowest employment rate for women and most Italian regions, even the richest and most developed ones, show significant gender differences in participation rates.[8] Despite such evidence, while particular attention to gender differences in the evaluation literature has been devoted to studies concerned with Nordic or Central European countries, very few studies focus on Southern Mediterranean countries.[9] In sum, as Del Boca (2005) suggests, Italy is an interesting case study to investigate the dynamics of low labour force participation of women and therefore to study how ALMP's may impact on them.

In this paper we try to fill this gap by providing some evidence on the effectiveness of an active labour market program called *Interventi di Coesione Sociale* (Interventions for Social Cohesion, ICS, henceforth) which was recently implemented in the Southern Italian region of Sardinia. The main aim of this ALMP was to improve employment probabilities and income for disadvantaged workers using a set of interventions comprising, among others, counselling, employment subsidies and matching services, and it was thought at first as a

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[5] We refer to Bergemann and Van den Berg (2008) for a more detailed formal discussion of the reasons for expecting different effects of active labour market policies on women.

[6] On gender differences in the Italian labour market see Sulis (2011).

[7] More specifically, at the beginning of the 70s, the average OECD women's participation rate was 45%.

[8] Across 307 EU NUTS2 areas, the European region with the lowest participation rate differential between men and women is Stockholm (4,5%) while Emilia Romagna, the Italian region with lowest differential (17%), is only 218th. Most southern areas are among those with the highest gender differences, with Puglia (306 over 307) being the worst with a 32% difference between men and women in participation rates.

[9] See for example the very recent paper by Bosch and Van der Klaauw (2012) for evidence on a tax reform in the Netherlands.

pilot study in the Italian national program for labour market policies. However, the main intervention consisted in a temporary employment subsidy paid to private firms hiring eligible workers.[10] As most ALMPs, even the ICS program was not specifically targeted by gender (see Card et al., 2011) but, more broadly, to disadvantaged workers. However, conditions to be eligible were different depending on the applicant's gender: women needed only to be unemployed, while men had to fulfill more conditions. As a result, 75% of our sample (and of program's participants) is indeed composed by women. This is hardly surprising, since Sardinia is among the less developed Italian regions, and in terms of labour market gender differential, its female labour market participation rates are quite low (50% in 2010).[11]

Our empirical analysis uses propensity score matching methods to investigate the effect of the policy on the full sample of 859 individuals who entered the program in 2006-2008. We match administrative data with a comprehensive survey that provides us with both post-program information on employment status and income outcomes and with detailed pre-program individual and demographic characteristics.

Results on the whole sample of female workers indicate that the policy had a substantial positive effect on both the probability of finding a job for the group of treated (about 43%) and on their level of income (397 euros per month). We also check for the presence of effect heterogeneity for specific sub-groups of female workers. In particular, we find larger effects for more disadvantaged categories: effects for low skilled are higher than for high skilled workers (45% vs. 40%) and for older workers with respect to the younger cohort (43% vs. 37%). Further, although caution should be taken in interpreting this result due to the different eligibility conditions, as found in other papers in this literature, we obtain larger effect for women than for our sample of disadvantaged men.

Together with the standard impact evaluation analysis, during the survey we have also collected detailed information on pre-treatment search activity of unemployed individuals that provides some suggestive evidence on the use of informal networks and family ties and on possible previous contacts between eligible workers and firms that subsequently hired them.[12] First of all, even if not conclusive, this evidence enables us to shed some light on possible informal network effects and find that their role is larger for less educated and older workers. Second, we use the answer received in our survey by the treated sample to the question "Before being hired, did you have the chance to collaborate with the firm that hired you?" to explore the possibility that the program has been ultimately effective into converting informal labour market agreements into formal ones, the latter being an important issue and a relevant policy theme for the Italian economy.[13] Indeed, we do find that the policy was more effective if we consider separately the subsample of women who

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[10] The employment subsidy component of active labour market policies has been previously studied for the Italian case by Felli and Ichino (1988) and, more recently, by Paggiaro et al (2009) in the context of the Mobility Lists program, which is a mixture between active and passive interventions. See Sianesi (2004) for a comprehensive study of the effects of employment subsidies in Sweden.

[11] Note also that the employment rate for men in Sardinia in 2010 was much higher than 70%, meeting the EU Lisbon men target for 2010.

[12] See Cappellari and Tatsiramos (2011).

[13] See Bratti et al. (2005) for the effects of not having a contract on labour market participation of women in Italy; see Schneider (2011) for the size of the shadow economy labour force in Italy compared to other OECD countries; see Boeri and Garibaldi (2001) for a matching model of the shadow economy with unemployment calibrated on Italian regional and sectoral data.

declared they have already (not formally) collaborated before receiving the treatment than for the remaining subsample. Overall, this result suggests that the policy has been more effective in converting black market agreements into formal ones than in creating new employer-employee matches.

The rest of the paper is organised as follows. In the next section we describe the program, while the data and descriptive statistics are presented in section 3. Section 4 is dedicated to the estimation method and the results. After some discussion on the interpretation of the results, in section 5 we conclude.

2   Description of the program

The ICS (Interventions for Social Cohesion) program has been the first comprehensive active labour market program implemented in the Italian region of Sardinia. The program was firstly activated as a pilot program to develop similar policy interventions on a national scale and was supported by the Italian Ministry of Labour and Social Policy, jointly with the regional government, who had the full responsibility of the effective implementation. Although the program has been formally launched in June 2004, it actually started only in 2006.

The ICS program was aimed at reducing unemployment and increasing re-employment probabilities for different groups of disadvantaged workers. In fact, conditions to be eligible to participate in the program were different depending on the applicant's gender. Women only needed to be unemployed, not to receive any unemployment subsidy and to be resident in Sardinia, while men had to fulfill more stringent conditions: being long term unemployed (to have been unemployed for at least 24 months, certified by the local Labour Office) and/or to be older than 44 years of age, and they should not receive any unemployment benefit at the moment of application for the program.[14]

In principle, the ICS program was a composite labour market policy, involving several types of interventions both on the labour demand and supply side, thus directly targeting both firms and unemployed (and non-participating) workers. While the set of interventions specifically directed towards firms have not been eventually implemented, much more attention was dedicated to the labour supply side of the program. In fact, the interventions directed towards unemployed and non-participating workers consisted of a mix of policies including employment subsidies, counseling and tutoring services and matching services.[15] The latter intervention consisted in the possibility for unemployed and non-participating workers of being directly matched to a vacancy of a firm requiring exactly her/his qualification profile.

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[14] Other specific disadvantaged worker categories were eligible, such as drug addicts, alcoholics, detainees being on alternative measures of detention and young workers in families with persistent problems. These categories of workers, besides representing a small percentage of the participants to the ICS program, had peculiar characteristics, and therefore were excluded from our sample from the very beginning.
[15] To some extent, the ICS policy can be considered as the Employment and Relocation services type of policy described by Rodriguez-Planas and Jacob (2010) for the Romanian case, since both include a mix of interventions.

However, the most relevant type of intervention comprised in the ICS policy was a typical hiring subsidy. Firms received an employment subsidy of 460 Euros/month for a maximum period of 12 months. Such subsidy was conditional on eventually hiring workers on a full time contract for a duration of at least 18 months. At the end of the 18 months period, firms received an additional lump sum payment of 2000 Euros if hiring the worker on a permanent contract.[16] Besides these monetary incentives, workers and firms were also entitled to receive counseling support in identifying their occupational needs, with tutoring concerning the workings of the ICS policy and rules governing local and national hiring procedures.

As said above, the ICS program also considered the possibility for workers and firms to use a specifically organized matching service provided by a public employment agency (INSAR) in order to significantly reduce two-sided search costs. At first, the latter was intended as the most innovative intervention of the ICS program as it was the first of this type in Sardinia. However, as we will discuss in more detail in the rest of the paper, such (apparently) appealing feature of the intervention turned to be rarely used by both workers and firms. In fact, bureaucratic and organisational problems turned this part of the policy substantially ineffective: to match workers and firms, the public employment agency developed a database including the occupational needs of the firms and some demographic characteristics of participating workers. However, not only the database development required a much longer time than expected, but in the end it also lacked significant and relevant information. As a result, the final database was unsuitable for performing the employer-employee matching in an efficient way. As a leading example, in the data collected there was no information on workers' educational attainment, which might have possibly helped to match workers to firms.[17] Given all these difficulties, at the end of the program it emerged that matching services have been used by very few participants-firms. In this second case, the ICS services had simply to verify if the worker had particular requirements and providing assistance in order to prepare the documents to participate in the program.

Moreover, the large numbers of applicants (more than 10,000) caused long delays in all bureaucratic procedures and the program implementation had to be postponed. As a result, a significant number of firms originally interested in the program decided to drop out, forcing the regional Government to make a second call. Therefore, we observe two different waves of participants: the first call was opened from June to December 2006, attracting the interest of 533 firms seeking 1258 professional profiles. The second, launched in December 2007 and ended in March 2008, has seen 423 firms applying for the program and seeking 952 job profiles. This second wave of the program explicitly introduced the possibility of a direct call (*chiamata nominativa*) for firms willing to hire one or more particular workers in the pool of participants. However, such possibility was also implicitly allowed during the first wave and, as we will see in the next sections, the explicit direct call

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[16] In this respect the employment subsidy of the ICS policy is quite similar to the one of the Mobility Lists discussed, among others, by Paggiaro et al. (2009).

[17] Firms had to provide information on their name, the sector of activity, the headquarter, the occupational profile and the number of potential workers required. Unemployed applicants had to provide information on gender, age, dwelling, preference for geographic area of work and two professional qualifications. We will return on this point in the next sections.

eventually crowded out the "matching service" offered by the public employment agency discussed above. Overall, ICS beneficiaries enrolled in the program were 877 and, by the end of June 2008, they have all agreed to participate to the program and started to receive the treatment.

3 Data and descriptive statistics

3.1 Data

Our first source of data is the administrative database provided to us by the public employment agency (INSAR) which was in charge for the implementation of the program. As we discussed in the previous section, this dataset only contains few and basic information on personal characteristics (age, gender, place of residence and professional qualifications) for the 7955 individuals who expressed interest and submitted the application form to become eligible to the ICS program in the first place.[18]

Approximately 10% of those who expressed their interest in the ICS program, a sub-sample of 877 beneficiaries, turned to be matched with an employer and eventually treated.[19] For them, we also have administrative data that contains the following additional information: the exact date at which the worker was hired, the attendance of any training, the characteristics of the contract offered to the worker (length, type of contract, hours/week), and the occupational skills profile. Finally, the administrative data also contains information on the employment subsidies and the lump-sum of 2000 euros given to firms hiring workers on a permanent contract.

As the administrative data lacked important information which is relevant for the policy evaluation, we decided to complement the dataset with additional survey data. Hence, the second source of data was collected through computer-assisted telephone interviews (CATI) that took place between December 2009 and March 2010. First, we interviewed a random sample of 462 beneficiaries of the ICS program.[20] In order to avoid likely upward biased estimates due to locking-in effects, we then decided to exclude from our sample all workers who were still under treatment during the observation period. More specifically, we decided to include only those workers who completed the treatment at least 3 months before the interview.[21] Second, we selected the group of non-treated individuals from the sample of those who expressed interest in the program but eventually were not treated. In this case we extracted 1415 individuals to match the distribution of participants

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[18] More precisely, 10408 applications to participate in the program were received, but 2453 were not satisfying the admission criteria.

[19] Of these, only 795 have been eventually hired by firms. The difference between the numbers is due to firms that decided not to hire the worker after the probation period, and to workers that decided to drop out of the program.

[20] During the interview process, only 48 individuals refused to grant the interview, and 25 have arranged to keep in touch with the interviewer, but they eventually didn't show up. Moreover, 188 were non respondent, while 127 contacts were associated with an incorrect telephone number.

[21] We also performed the analysis including workers whose employment subsidy period was not yet finished. Estimated effects were, unsurprisingly, higher and we interpret these results as plagued by upward bias due to locking-in effects.

in terms of gender, age and geographic area of residence.[22] Hence, as we shall see below, the two groups of treated and non-treated are quite similar in terms of the above observed characteristics. After excluding non-respondents and not available individuals, we ended up with a sample of 558 non-treated individuals.[23] This led us with a final sample of 859 individuals, including 351 participants and 558 non-participants.

3.2 Descriptive statistics

The questionnaire used for the interviews enabled us to collect the important additional information on pre-treatment individual characteristics missing in our administrative dataset and essential to perform our matching analysis.[24] We therefore turn to a more accurate description of these variables, with particular attention to those used in the propensity score. Table 1A provides descriptive statistics for the most relevant variables concerning demographics. We distinguish between treated and non-treated individuals and data are also reported separately by gender. In particular, although we also provide information for the whole sample, in what follows we mostly focus on the female group characteristics, as it constitutes about 75% of our final sample and it is the main objective of our study.

Overall, in terms of demographics we do not observe significant differences between the treated and non-treated samples. More heterogeneity is observed between men and women but these are mainly due to the different eligibility criteria employed for the two groups. First, we find that on average female participants are 32 years old and, as expected, they are slightly younger than men as for the latter group the eligibility criterion was long-term unemployment (to be unemployed for at least 24 months) and/or to be more than 44 years old. We have also asked marital status and the presence of children since both characteristics are supposed to influence participation in the labour market, especially for women (see, among others, Killingsworth and Heckman, 1986). Married women include only 41% of our female treated sample and 60% answered to have no children when applied for the ICS policy. Only slight differences are observed among the non-treated female sample, with a higher percentage of married women (+5%) but very similar numbers when we consider the presence of children. When we compare by gender, we find that the percentage of men with children is substantially lower than that of women, about 24% for both treated and non-treated.

Another characteristic that may influence participation in the labour market and mainly for its female component is the presence of elderly and disabled to care in the household.[25] This is particularly true for our sample since the Italian welfare state does not provide adequate services in this field. We find that the proportion of women providing

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[22] A very similar approach can be found in RodriguezPlanas and Jacob (2010). As stressed by those authors (see page 72 of their paper), this procedure does not affect estimation and interpretation strategy but "…it should be kept in mind when interpreting the differences between groups".

[23] Among them, 246 were not respondents, 338 (24% of the total sample) have refused to reply, and 104 did not show up at the telephone appointment. Finally, an incorrect telephone number was associated to 173 contacts.

[24] We have also collected information on job and program satisfaction that will not be used in this specific analysis.

[25] See Leigh (2010).

informal care to the elderly/disabled, is always lower for the treated sample (about 12% against 18%). Moreover, data reported in Table 1A indicate that about 73% of women live in a house that is owned by them (or by their parents), this is true for both participants and non-participants. This variable may be correlated with spatial mobility and, indirectly, labour force participation, and could thus influence participation in the program.

We now turn to the analysis of the educational attainments and self-reported professional skills. Again, we do not identify significant differences between treated and non-treated. Women show, as expected, higher educational attainments with respect to the more disadvantaged sample of men. In particular, with respect to the Italian (and regional) labour market characteristics, the percentage of women that declare to have completed tertiary education is fairly high, respectively just above and just below 20% for the treated and non treated sample.[26] The largest proportion of our female sample has left school at the upper secondary school level (about 46% for both groups) but a relatively high percentage, around 32%, ended formal education before upper secondary. This last evidence should not come as a surprise since Sardinia is characterised by very high percentage of secondary school drop-outs. In particular, secondary school drop-outs rates are higher among boys than girls and this evidence is also confirmed in our disadvantaged workers sample where we observe that almost half of our male (both treated and non-treated) samples finished the formal education acquisition at the lower secondary level. Conversely, men show significantly lower percentage of tertiary education attainments (respectively 11% for treated and 14% for the non-treated) than women.

Finally, Table 1A includes the self-reported specific professional qualification that provides another important measure of skills. The latter variable is also particularly relevant for the purposes of our study as the matching of workers and firms was essentially based on the qualification of workers and on the desired job profiles of firms. In general, we find significant differences between the sample of men and the sample of women also reflecting the lower educational attainments of the former group. In particular, a large part of our women sample, around 73%, certifies professional skills useful for the service sector. For example, the proportion of (treated) women reporting "administrative office" skills is 40%, against 25% for (treated) men. Conversely, the proportion of reported "artisans and farmers" is high for men (about 23% for the sample of participants) and almost nil for women (less than 2% for the sample of participants). Further, we observe that unskilled workers are slightly overrepresented in the sample of treated women (about 17%) against 12% for the non-treated, while the opposite is true for men (13% vs. 22%).

A second set of controls includes measures of previous experience in the labour market. In this case, unlike the previous set of personal characteristics, in Table 1B we do observe significant heterogeneity between treated and non-treated samples. The only exception is possibly observed on unemployment subsidies where we have the same percentage (less than 30%) of subsidies receivers before joining the ICS program in the two groups.

First, heterogeneity arises when female applicants have been asked if they attended some professional training before the ICS policy was implemented: the percentage of treated who replied "yes" to this question is significantly lower (32%) than for the sample

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[26] For more on Italian educational attainments see Di Liberto (2008).

of non-treated (45%). However, the most significant differences are observed in terms of the previous job search history of the individuals. In fact, quite unexpectedly, in this case we find that the treated sample (men and women) has lower probabilities of being job seekers. In particular, for the women subsample we have 70% for treated against 83% of the non-treated. These figures seem to suggest that, among the eligible individuals, the ICS policy was relatively more effective in selecting less motivated individuals and that the program was quite effective to help the transition of such group of workers in the labour force. However, this conclusion needs to be further investigated and the following evidence provides more hints and, possibly, a different picture. In fact, it could also be that these workers were not actively seeking for a job as they already had one, even if an irregular one. To further investigate this issue, we have also asked a series of questions concerning the search behaviour of these individuals and, again, we do find significant differences between treated and non-treated.[27] In particular, when searching for a job, treated are significantly less likely to use the public employment services channel (22% vs. 49% for non treated in the whole sample), and more likely to use personal contacts friends and relatives (33% vs. only 15%). In particular, the latter is the most important search channel for the sample of treated women. We also interestingly find that internet is among the most used job search channels, with about 10 percentage points difference with respect to the non-treated.

Second, more than half of those who actively searched for a job have indeed received a job offer: the percentage is higher for treated rather than untreated ones (47% against 40% in the women sub-sample). On the other hand, quite surprisingly, the percentage of those who accepted such an offer is about 45% against 57% for the sample of non-treated. Third, when asked about the type of pre-treatment job it emerges that more than 10% of both groups report they previously worked without a regular contract with higher percentages for treated than non-treated (12% against 10% for the overall sample) and even higher for participant women (13% against 11%). Finally, in order to control for this possibility we have also asked participants if, before being employed through the ICS-program firm, they already had the chance to collaborate with the firm. Among female treated individuals 64% answer positively to this question. When the same question has been asked to the non-treated individuals who eventually found a job without the program support, this percentage drops to 6%. A similar difference between treated and non-treated holds for the men sample (11% against 53%).

Overall, such evidence seems to suggest a plausible story where the ICS policy could have been used by workers and firms as a channel to convert irregular underground labour agreements into formal employment relationships. We will try to take into account for this possibility in the following analysis.

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[27] As we will explain in the next section, we were not able to include all these variables in the propensity score, as the number of observations drops substantially.

4   Empirical analysis

4.1 Identification strategy

Our analyses on the effects of the ICS active labour market program use the standard framework of the potential outcome approach to causality or Roy-Rubin model that, in the binary case, defines a treatment indicator D_i equals to one if the individual receives treatment and zero if she/he does not (see Roy, 1951 and Rubin, 1974). In our setting we compare two different possibilities faced by unemployed: to participate to the ICS programme (treatment) or continue searching for a job. More precisely, the Roy-Rubin model defines the two potential outcomes for each individual i as $Y_i(D_i)$, while the treatment effect, τ , can be written as:

1) $\quad \tau_i = Y_i(1) - Y_i(0)$ $\hspace{4cm}$ $i = 1, \dots, N$

where N denotes the total population. The fundamental problem in this setting is that each individual may be only observed in one of these two states. In this study we focus on the most important evaluation parameter, the average treatment effect on the treated (ATT), that is, on the effect the treatment shows for individuals that actually participate the program. The ATT is given by:

2) $\quad \tau_{ATT} = E(\tau | D = 1) = E[Y(1)|D = 1] - E[Y(0)|D = 1]$

The last term E[Y(0)│D=1] is the counterfactual mean for the treated and cannot be observed. In experimental data, that is, when assignment to treatment is fully random, to estimate τ_{ATT} one can use the mean outcome of untreated individuals $E[Y(0)|D = 0]$ since the following condition is ensured:

3) $\quad E[Y(0)|D = 1] - E[Y(0)|D = 0] = 0$

With non-experimental data this difference is not zero and the problem of selection bias arises. One therefore needs to rely on some identifying assumption. In the following we briefly describe the identifying assumption of propensity-score matching (PSM henceforth) methods that we apply in our study.[28] PSM methods need to find a group of treated individuals which are similar to the control groups in all relevant pre-treatment characteristics, the only remaining difference being that one group was exposed to the program we would like to evaluate while the other group was not.

This methodology relies on two key assumptions. The first is the Conditional Independence Assumption (CIA), or uncounfoundness, which implies that selection into the treatment is exclusively based on observable characteristics, X, not affected by the treatment. More formally, given X (or set of observable covariates not affected by the treatment) potential outcomes Y are independent of treatment assignment, that is:

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[28] For more details see Caliendo and Kopeinig (2008).

4) $Y(0), Y(1) \amalg D \mid X$

This assumptions implies that in our study we are observing (and controlling for) all variables that simultaneously influence both treatment assignment and potential outcome. The second assumption is the common support condition that implies that:

5) $0 < P(D = 1 \mid X) < 1$

for each individual. This is the region where the balancing score has positive density for both treatment and comparison units. This assumption implies that individuals with the same X values both have a positive probability of receiving the treatment or not. It also implies that no matches can be formed to estimate the average treatment effects on the treated (ATT) parameter when there is no overlap between the treatment and non-treatment groups.

Matching on every covariate is difficult to implement when the set of covariates is large. To solve this dimensionality problem PSM estimate the propensity score $P(D = 1 \mid X) = P(X)$, that is, the probability of participating in a program conditional on X. It can be shown that, holding the CIA assumption, all bias due to observables can be removed by conditioning on the propensity score (Caliendo and Kopening, 2008, p. 36). Given assumptions (4) and (5) the PSM estimator for ATT is then identified by:

6) $$\tau_{ATT}^{PSM} = E_{(P(X) \mid D=1)} \left\{ E \left[Y(1) \mid D=1, P(X) \right] - E \left[Y(0) \mid D=0, P(X) \right] \right\}$$

We omit further details here while refer to the vast literature on this (e.g., Rosenbaum and Rubin, 1983; Heckman et al., 1998; Dehejia and Wahba, 2002; Smith and Todd, 2005; Caliendo and Kopeinig, 2008).

However, as in all program evaluation studies with observational (non-experimental) data one needs to take into account of the issue of selection bias. Selection on unobservables (Nichols, 2007) arises because, in any non-experimental setting, we cannot exclude that policy treatment and outcome are correlated ex-ante through unobservable characteristics that affect, at once, the probability of an individual to be treated and its observed outcome. A second empirical issue concerns the fact that the policy outcome itself may be neither obvious nor easily measurable. In particular, the policy impact can be actually revealed by multiple outcomes each of them expressing a different implication of the policy on former behaviour. A third and, for our study, more worrying complication regards the nature itself of the policy, since it is actually delivered at the individual level not as a single treatment but as a whole menu of measures whose effects may either compensate or interact. Analysing the individual-level impact of the ICS policy, therefore, takes the form of estimating the treatment effect in a non-experimental, multi-treatment environment.

In the following, we claim that it is plausible to assume that our X's, that is, our administrative and survey control variables, enable us to accept that the CIA holds in our

exercise and that the mean effect of treatment can thus be calculated as the mean difference in outcomes over the common support, appropriately weighted by the propensity score of the participants as stated by equation (6) reported above.

4.2 Estimation methodology

We first analyse which are the variables that determine the selection into treatment, and then we estimate the treatment effect on the treated (ATT) through a matching algorithm. Before discussing the propensity score specification, it is important to remind that two characteristics of our dataset should help decrease the possible presence of bias in our estimates.[29] First of all, we use the same source of data (administrative and survey data) for both treated and non-treated. This should ensure that that X's in our probit model are similarly measured across the two groups. Second, we have collected survey data from a sample of eligible non participants as well as participants.

Moreover, we also have a potential rich set of suitable additional covariates and we are therefore able to control for many characteristics that are likely to determine both participation and labour market outcomes. Having said that, in choosing our X variables we will also take into account that the inclusion of too many variables in the propensity score could result in higher standard error for the estimated propensity score, and may also reduce the likelihood of finding a common support.[30] The choice is therefore mainly based on economic theory considerations and previous empirical results. Our final specification of the propensity score that satisfies the balancing property includes the following covariates: gender, age, age squared, and a series of dummies for the presence of children, marital status, home ownership, presence of elderly/disabled to care, educational levels, job search activity, unemployment subsidy and previous training. We also add a series of dummies identifying the occupational profiles reported by individuals when they applied to become eligible for the ICS program.[31] All the above variables represent the pre-treatment socio-demographic characteristics that are supposed to influence the allocation of individuals across the two groups.

We expect demographic characteristics such as the age and gender of participants to have an effect on the probability of treatment, as they also have an effect on labour market outcomes of individuals, as participation and employment status. In the same spirit, the number of children, the presence of elderly/disabled in the household and the marital status are supposed to influence the probability to participate in the program, especially for women. Likewise, dummies for previous occupational profile and education levels are included as they should have an effect on the selection into treatment. Finally, previous job search activity, past participation in training programs, having received an unemployment benefit should also influence the selection into treatment.

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[29] On this, see Heckman et al. (1997) and Heckman et al. (1998).

[30] See Caliendo and Kopening (2008) for advantages and disadvantages of over-parametrisation. See also Khandker et al. (2010) and Byrson et al. (2002) for recommendations against over-parametrised models.

[31] When we apply propensity score matching to the sample of women we exclude the gender dummy. Area dummies are not included since, as said in the previous sections, non-treated individuals match the distribution of participants in terms of the geographic area of residence. Nevertheless, including them does not change the main results.

Table 2 includes the results of the probit-estimation on our main sample of women.[32] Overall, results in the Table are as expected but, possibly, two exceptions. In fact, the pre-treatment dummies on both job search activity and participation in training programs interestingly indicate lower probability of participation and we will return on this possibly puzzling result in next sections. To give a better idea of the quality of our estimates, in Figure 1 we also report the distribution of this estimated propensity scores. This clearly shows that the overlap assumption is satisfied since the propensity scores distribution of the treated clearly overlaps the region of the propensity scores of non-treated.

In the following, we discuss the results on the effect of the ICS policy on employment and income obtained applying the kernel matching method to different samples, together with their corresponding quality measures (see Table 4).[33] We claim that the kernel matching method is the most appropriate in this context, since it includes in the calculation of the outcome the highest possible number of counterfactuals. In particular, when treated and non-treated samples are not large, as in our case, this method enables to obtain efficient estimates of the effect of the policy.[34]

4.3 Main results

As mentioned above, the direct aim of the program was to integrate unemployed individuals into the labour market. In other words, we firstly check if this policy has caused an increase in the probability of getting and maintaining a job for the pool of participants using a standard binary outcome variable to measure employment status of workers after the program was terminated. Secondly, as it is typical in the literature, we also estimate the effect of the ICS program on the annual net income of participants. For the latter dependent variable, we exploit additional information on self-reported assessment of the annual net income of participants obtained from our survey data, which should be more reliable than available administrative data.

To set the scene, before focusing on the sub-sample of women, we investigate the overall effect of the ICS policy and show the results obtained on the full (men and women) sample. In row I of Table 3 we report the estimates of the average treatment effect on the treated (ATT) of our two possible outcomes, employment and income. Considering the full sample of ICS participants, results suggest 42% more probability to be employed for participants than non-participants. Similarly, the analysis also suggests an increase in average earnings for participants of 403 euros with respect to non-participants with identical observable characteristics.

However, as said above, since the eligibility criteria for the two gender groups are different, it is more appropriate to divide the sample by gender. Even if not entirely comparable, it is nevertheless interesting to investigate possible gender differences in program's outcomes. As previously discussed, higher effects for women may arise for at

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[32] Results for the overall sample are available upon request.

[33] In order to assess the matching quality, Table 4 includes the mean standardized bias and Pseudo-R2 results.

[34] In particular, we apply an Epanechnikov Kernel with a bandwidth of 0.06. Bootstrapped confidence intervals have been calculated based on 200 replications. In order to improve on the quality of the matches, estimates are performed imposing the common support condition in the estimation of the propensity score. See Becker and Ichino (2002) and Caliendo and Kopeinig (2008).

least three different reasons. First, this is because of their different elasticity of labour supply: having women more alternatives to paid work, they tend not to participate in the labour market and the positive effect of ALMPs should be higher for them as the policies should have relatively more positive effect on their labour market status. Second, women may have higher reservation wages, hence policies that increase the arrival rate of offers increase the probability of leaving unemployment (and/or increasing participation in the labour market), thus being relatively more effective for them. Third, in countries with low participation of women in the labour market (as Italy), the skill and education level of unemployed women is relatively high, thus the potential for productive participation in ALMPs is higher (see Bergemann and Van den Berg, 2008). On the other hand, recent findings in the literature suggest that ALMPs may be more effective for more disadvantaged categories.[35] In the case of the ICS program, to become eligible for the program men had to belong to genuinely disadvantaged categories, while women had to satisfy much less restrictive conditions, hence in this case we could expect higher effects for men.

Results reported in rows II and III of Table 3 show a larger effect of the program for women (43% more probability to be employed) than men (40%). Likewise, income levels for women are higher than for men, the difference between treated individuals in the two groups being about 50 euros. Again, all estimated effects are statistically significant at conventional levels. Thus, even if results are not directly comparable across groups, they suggest a larger effect for women, as also indicated by the theory and in line with most studies in the literature that look at gender differences.[36]

From now on, we therefore specifically focus on the group of women that constitutes about 75% of our full sample, and further investigate if we find different effects for different subgroups. In fact, since all unemployed women were eligible for the ICS program our female sample is significantly heterogeneous in terms of, for example, educational attainments and experience levels. Therefore, we may expect some relevant differences depending on the type of (female) individuals who benefit from participation and we perform the full estimation procedure previously described for different female subgroups of low/high education and younger/older individuals.

We firstly divide the sample into low (with only lower secondary school attainment level) and high (upper secondary or above) educated individuals. In the descriptive section of the paper we already emphasized the relative importance of highly educated female workers in the sample, and we firstly checked if there were any differences in the effect of the policy for different levels of education. We find (see rows IV and V, Table 3) that the less educated are those who benefit relatively more from the ICS program: the estimated effect on the probability of being employed is equal to 45% against about 40% for those with upper secondary or tertiary attainment levels. Considering income levels, we find that women with lower levels of education earn on average 420 euros per month more than non-treated. This pattern is not surprising, as the ICS policy was mainly targeted towards

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[35] See for example, Caliendo and Kunn (2011a) and Rodriguez-Planas (2010).

[36] As said above, the few studies that compare results by gender (with men and women fulfilling the same eligibility criteria) find that the effect is higher for women than for men. See for example Gerfin and Lechner (2002), Gerfin et al. (2005) and Caliendo and Kunn (2011b). See also Bergemann and Van den Berg (2008) for a general discussion.

more disadvantaged groups, and similar results have been found in the literature (see Caliendo and Kunn, 2011a).

Second, we consider different age groups. In this case (see rows VI and VII of Table 3) we find that the effect of the policy on the probability of being employed is increasing in age: participants with less than 30 years of age have about 37% higher employment probability than non-treated, while such effect increased to 42% for participants in the age group equal or older than 30. There are different possible explanations for this result. On the one hand, as age proxies labour market experience, this indicates that the ICS program was relatively more effective for more experienced women that, for some reason, were out of the labour market, possibly because their reservation wage or outside option was higher (this could be the case for women with children). On the other hand, this group of workers could be the more disadvantaged one in terms of labour market opportunities, possibly because their human and search capital was largely depreciated, thus the ICS policy reached the target of increasing their chances in the labour market.

Finally, in rows VIII and IX of Table 3 we report estimates of the policy effect dividing the sample between the two waves of the policy implementation that we described in previous sections. As said above, a first wave of the ICS program was in fact launched in 2006 but, due to long delays in all bureaucratic procedures, there has been a significant number of firms originally interested in the program that dropped out. A second call was then launched in 2007 and more matching of workers with firms could be done. Thus, across beneficiaries we have two groups of people who entered (and ended) the program in two different periods. As a result, the two waves results may be interpreted as a short-term effect (second wave) and medium-term effect (first wave) of the ICS policy. The effect of the policy in the first wave is significantly lower than that estimated for the second wave: former participants in the program have a 33% higher probability to be still employed compared to non-participants while, as expected, the estimated short-term effect (second wave) is higher and equal to 47%.

Overall, the magnitude of these effects is substantial but not uncommon or new in this literature, especially when considering the effect of employment subsidies to private firms on the probability of being employed. In fact, similar results have been found in different context and countries.[37] Thus, employment subsidies seem to dominate other types of ALMPs in terms of increasing the probability of obtaining a job and the fact that studies for such different countries yield similar positive results suggests the effectiveness of such types of interventions.[38] Our full set of results is robust to the use of alternative matching estimators: we performed the same analysis using nearest neighbors and radius matching estimators and do not find any significant change in estimated effects. Results using radius matching are shown in Table 5. [39]

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[37] For example, these results are in line with those obtained by Rodriguez Planas (2010) who finds that in Romania (Table 9 of her paper) participants to a similar labour market program had between 32 and 57% higher probability of being employed than non-participants. Similarly results are also found in Sianesi (2002) for Sweden. Gerfin and Lechner (2002) find a probability equal to about 10% for Switzerland.

[38] In what follows, we also discuss in more detail other possible causes of such large effect. In fact, the employment outcome of participants is measured just after the participants left the program.

[39] Results using nearest neighbor matching are available upon request

4.4 Further results: informal search channels and black market agreements

As seen above, when we divide the sample between the two waves we find that the estimated effect of the policy was lower in the first than in the second wave. We mainly explain this result as medium vs. short run effect of the policy, but this is likely to tell us only part of the story and, in the following, we explore another complementary (and not necessarily alternative) explanation. In fact, firms in the two waves faced also apparently different formal selection criteria for the hiring of workers: during the first call/wave, firms were supposed to use exclusively the matching service offered by the program to select workers, while during the second wave the direct call of workers from firms to fill vacant jobs was formally allowed. However, even during the first wave the matching service has been rarely used since, as previously discussed, the extremely long delays and firms drop-outs caused a change of procedures. As a result, in both waves firms could informally follow exactly the same selection criteria, that is, they could directly select the unemployed worker to be hired (so called *chiamata nominativa*), or use the matching service offered by INSAR.[40]

This particular design of the ICS policy raises one important consideration. In fact, in this setting firms could choose between the "best" worker-firm match, as identified by the regional government, or choose eligible unemployed workers selecting them from their (informal) network. Our results clearly indicate that the latter effect dominates the former one, as the effect of the policy is stronger once the direct call was explicitly allowed (see also rows IX of Table 3 compared to row VIII). Moreover, the direct call from firms raises the suspicion that at least part of the observed hirings are the emergence of black (informal) labour market agreements. Indeed, the Italian labour market, especially in the less developed areas, is characterised by significant levels of black market agreements and it is likely that, at the very end, this policy has served as a tool to let such agreement be converted into formal employment relationships. [41]

Suggestions that this might have happened emerged when comparing the two groups of treated versus not treated. Results from the propensity score reported in Table 2 show a negative sign and significant coefficient on job search activity on the probability of receiving treatment, while descriptive statistics (see Table 1B) show that participants in the ICS program have lower probabilities of being job seekers before treatment (70% of treated against 83% of the non-treated for women). Further, our descriptive analysis interestingly shows as participants in the ICS program are less likely to use public channels to search for jobs, while they have twice as much probability to look for a job using informal contacts such as personal contacts, friends and relatives.

It is possible to interpret this apparently puzzling evidence in terms of previous black market agreements. That is, treated individuals were less active in searching for a job since they already had one. Hence, exploiting the options offered by the modified ICS program, firms hired the workers they already knew, choosing the direct call channel, that is, they

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[40] This detail has emerged from discussions with the administrative staff who have implemented the policy.

[41] Schneider (2011) reports that the shadow economy is estimated to be around 22% of GDP for 2007 in Italy, this figure being the second highest across OECD countries. On the other hand, the shadow economy participants in percentage of official labour force in 1998 were estimated between 30 and 50%.

were picking up the possibility of converting previous informal black market agreements into formal employment contracts.

To further investigate this issue, one possibility is to estimate the program effect separately for the sample of centrally matched individuals and for the sample of workers hired with a direct call. However, matched individuals are too few (only 36 individuals) to perform a significant in-depth analysis.[42] We only mention here that we do find highly heterogeneous effects: for the former group of centrally matched individuals, the estimate of the effect is still positive and significant but the effect is reduced by less than half (20%) as compared to the latter group hired with the direct call (45%).

As a second strategy we have asked our survey respondents (both treated and non-treated) if she/he has already had the chance to cooperate/collaborate with the firm that eventually hired them. The idea was to control, at least in this very imperfect way, for previous black market agreements between workers and firms. Although we cannot verify if workers and firms already had a previous informal employment relationship, with this information we can check if there was a previous network connection. Moreover, as said above, the size of the informal economy in Sardinia (and Italy in general) suggests that some of the matches created through the ICS program could represent emersion of informal labour contracts.

The analysis leads to further interesting results. In Table 6 we include results for the sub-sample of those who knew and collaborated with the firm before being hired by them (103 treated individuals) separate from those who did not (89 observations). For the former, the effect turns out to be large (45%) and statistically significant. On the other hand, for those who reported no previous contact with the firm that subsequently hired them the effect is smaller (39%). We find a similar pattern for income. This evidence seems thus to confirm our previous analysis, that is, the effects of this program are likely to be affected by the presence of such strong ties in the labour market.

To further investigate this issue, we also estimate a probit model in which the dependent variable is a dummy variable that takes the value of one if the individual has answered using as main job search channel "personal contacts, friends and relatives", that is, a broader definition of informal networks. In particular, the literature on informal networks distinguishes between the effects of "professional contacts" and "family contacts" and our previous analysis could only capture the first channel. The set of controls in our probit regression includes education, age, previous unemployment benefits, previous training, home ownership, marital status, presence of children, professional qualification, and a dummy equal to one in case of previous relationship with the firm. As already found in this literature (see Ponzo and Scoppa, 2011 and Cappellari and Tatsiramos, 2011) our estimates show that informal networks are used more frequently by low educated and older individuals that tend to use exclusively this channel, while, as expected, the dummy on the previous collaboration with the firm shows a positive and significant coefficient.[43]

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[42] More precisely, only 8% of firms/workers used the public matching service, while the remaining 92% of the matches have been realised through a direct call by the firm.

[43] Interestingly, using the same set of controls indicated above, previous collaboration with the firm turns out to be negatively correlated with the probability of actively searching for a job. These results are available upon request.

Finally, our evidence seems also to suggest that informal networks may interact with formal channels of job search activity, as the ones proposed in the ICS policy. In fact, it may be that informal networks reduce the search costs of firms and workers using such search channel, and essentially crowd out formal public employment services provided by the regional government.[44] This issue has been only recently investigated in specific studies dealing with the role of different search channels on labour market outcomes. For example, Fougere et al (2009) study the interaction effects of public employment services on the search effort of unemployed workers with possible crowding out effects of these two search channels. In a similar spirit, Van den Berg and Van der Klaauw (2006) study the effect of counseling and monitoring on the transition rate from unemployment to employment. Interestingly, monitoring shifts job search effort from informal to formal search methods, while counseling may improve the quality of applications and credentials, thus increasing the arrival rate of offers through a reduction in search costs for formal methods. In the case of the ICS program, proper monitoring and counseling were absent from the design of the policy, thus increasing the probability of using informal search channels.

5   Conclusions

In this paper we study the labour market effects of a policy intervention (ICS) which was recently implemented in the Italian region of Sardinia. The program was intended as a pilot before the extension at the national level. The intervention was administered by the regional government and had the objective to increase the probability of employment of specific disadvantaged groups of workers and consisted of various types of interventions as counselling, direct matching and employment subsidies for private firms, the latter being the main intervention implemented. We estimate the effects of such policy using standard propensity score methods and focus in particular on the sample of women, which constitutes about 75% of our sample.

Our estimates indicate that the ICS policy increased the probability of female participants to get a job by about 43% and that effects are stronger for women rather than for men. This result is in line with other finding in the literature, and suggests that women may significantly benefit from participating in this type of programs but the comparability between the two samples is reduced by the different eligibility criteria between men and women. Moreover, when we consider only the female sub-sample we find effect heterogeneity. In particular, the effect was stronger for low educated and older workers. Finally, since the program was implemented in two close but different periods we have also performed a separate analysis for the two waves. In this case, we find that the estimated effect of the policy is higher for those enrolled in the second instead of the first wave and this seems to suggest that the effect of the policy tend to decrease over time. Thus, it is also likely that, with new data on an observation window longer than the period of support we have here, the long term estimated effects would be significantly lower.

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[44] See also Loriga and Naticchioni (2012) for a study dealing with the role of Public Employment Services in Italy.

Moreover, using further information on the implementation of the policy and information on informal contacts between workers and firms which is usually difficult to obtain, we have tried to shed some light on the role that informal networks play in depressed labour markets. We firstly observe that, facing the choice between a matching service offered by the regional government, or eligible unemployed workers as selected from their previous (informal) network, the great majority of firms choose the second option. Such results suggest that the ICS policy had the possible ultimate effect of increasing the probability of getting a job for those who already a previous contact with the firm that subsequently hired them, thus converting possible informal labour agreement into formal employment relationships. Our findings seems to indicate that the expected reduction in costs deriving from information asymmetries concerning the quality of the match allowed by the possibility of direct calls was much greater than the benefit deriving from the centralised matching of workers to firms proposed by the regional government. Finally, as already found in this literature, our evidence shows that informal networks are used more frequently by low educated and disadvantaged individuals that also tend to use this as their exclusive job search channel.

In sum, our main contribution is twofold. First, we provide some direct empirical evidence on the effects of active labour market programs on women, which is particularly relevant in the debate on participation and labour market interventions such as hiring subsidies and public employment services. Second, our data provides some suggestive evidence on the role that informal networks and family/professional ties play in a depressed labour market. Future research avenues include the possibility of a follow-up interview for long term outcomes of workers and more accurate and detailed analysis on firm behaviour in hiring strategies.

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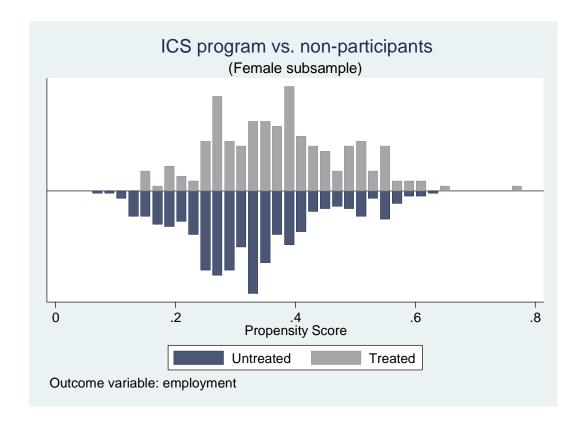World Economic Forum (2011), *The Global Gender Gap Report 2011*, Geneva.

Appendix: Figures and Tables

Figure 1: Propensity score distribution

ICS program vs. non-participants
(Female subsample)

Propensity Score

Untreated   Treated

Outcome variable: employment

Table 1A: Descriptive statistics, Demographics
Values in percentage

	Full sample		Women		Men	
	ICS Policy	Non-participants	ICS Policy	Non-participants	ICS Policy	Non-participants
Sex						
Female	73.75	75.45				
Age						
Age in years	32.72	33.53	32.14	32.59	34.36	36.43
Marital Status						
Single	60.47	65.95	59.46	63.90	63.29	72.26
No. of children						
One or more children	31.56	31.54	34.23	33.97	24.05	24.09
Elderly and disabled care						
Involved in care	11.32	20.80	12.18	18.06	8.82	29.66
Educational attainments						
Lower secondary school or less	35.55	35.30	31.98	32.54	45.57	43.80
Upper secondary school	45.85	46.24	46.85	47.51	43.04	42.34
Tertiary education	18.60	18.46	21.17	19.95	11.39	13.87
Professional qualification						
High skills	2.33	3.41	2.70	3.33	1.27	3.65
Technical skills	6.98	9.50	4.50	8.08	13.92	13.87
Administrative office skills	36.21	39.78	40.09	42.04	25.32	32.85
Commerce and service	29.24	26.16	33.33	31.35	17.72	10.22
Artisans and farmers	7.31	4.84	1.80	2.14	22.78	13.14
Blue collars and drivers	2.33	1.61	0.90	0.71	6.33	4.38
Other unskilled	15.61	14.70	16.67	12.35	12.66	21.90
Home ownership						
Yes	75.42	73.66	72.97	73.16	82.28	75.18

Table 1B: Descriptive statistics, Job Search
Values in percentage

	Full sample		Women		Men	
	ICS Policy	Non-participants	ICS Policy	Non-participants	ICS Policy	Non-participants
Job Search (pre-treatment)						
Active job search activity	72.76	84.41	70.27	82.90	79.75	89.05
Job offers during search						
Received offers	47.95	39.92	46.79	39.83	50.79	40.16
Probability to accept offers						
Accepted offers	44.76	57.45	45.21	53.96	43.75	67.35
Job search methods						
Public employment services	22.37	48.62	21.79	49.00	23.81	47.54
Private employment services	4.57	5.52	4.49	6.02	4.76	4.10
Internet	33.33	23.99	32.69	24.93	34.92	21.31
Personal contacts, friends and relatives	32.88	15.07	32.69	13.75	33.33	18.85
Other	6.85	6.79	8.33	6.30	3.17	8.20
Type of contract						
Short term	49.04	61.35	48.59	63.16	50.00	56.73
Permanent	38.94	28.38	38.03	25.94	40.91	34.62
Irregular	12.02	10.27	13.38	10.90	9.09	8.65
Previous relation with firm						
Have collaborated with the firm that eventually hired them	61.21	7.17	63.69	5.98	52.83	10.83
Unemployment subsidy						
Received subsidy	29.90	31.36	28.83	27.32	32.91	43.80
Professional training						
Received training	32.56	44.62	31.98	45.13	34.18	43.07

Table 2: Estimation of the Propensity Score (Probit model)
Dependent variable: treatment

Variables	Estimates		
	Coefficient	St. error	p-value
Age	-0.172	0.017	0.306
Age squared	0.000	0.000	0.375
Marital status single	0.154	0.071	0.030
Presence of children	-0.085	0.064	0.192
Home ownership	-0.006	0.048	0.907
Involved in care	-0.090	0.054	0.114
Education dummies			
Upper secondary school	0.059	0.054	0.277
Tertiary education	0.116	0.072	0.100
Professional qualification dummies			
Technical skills	-0.202	0.061	0.010
Administrative office	-0.037	0.048	0.433
Commerce and service	-0.075	0.125	0.574
Other unskilled	0.056	0.066	0.390
Job search activity	-0.155	0.052	0.002
Unemployment subsidy	0.004	0.047	0.936
Received training	-0.085	0.042	0.046
Sample size	579		
Log-likelihood	33.02		

Notes:

Reporting marginal effects, see the text for description of variables.

Table 3: Main Results (Kernel Matching)

Sample specification		# Treated	# Controls	OUTCOME VARIABLES	
				Employment	Income (in Euros per month)
I	Full sample	employment / income	265 / 223	500 / 420	0.418 (.033)	403.6 (40.9)
II	Men	employment / income	68 / 58	118 / 97	0.406 (0.086)	345.5 (126.6)
III	Women	employment / income	197 / 165	382 / 323	0.427 (.429)	397.6 (40.3)
	Women subsample					
IV	Low educational level	employment / income	63 / 55	127 / 111	.453 (.088)	420.7 (77.8)
V	High educational level	employment / income	134 / 110	244 / 203	.396 (.049)	368.9 (48.6)
VI	Younger cohort (≤ 30 years)	employment / income	89 / 75	157 / 133	.369 (.073)	345.3 (63.5)
VII	Older cohort (> 30 years)	employment / income	108 / 90	225 / 190	.430 (.057)	406.9 (56.7)
VIII	First wave of ICS	employment / income	75 / 59	382 / 323	.336 (.061)	331.2 (67.4)
IX	Second wave of ICS	employment / income	122 / 106	382 / 323	.468 (.044)	421.7 (45.5)

Notes:

ATT estimates and bootstrapped 95% confidence intervals in parenthesis. We apply an Epanechnikov Kernel with a bandwidth of 0.06. Bootstrapped confidence intervals have been calculated based on 200 replications.

Table 4 Quality of matching estimates

Sample specification	Quality indicators (measured after matching)	OUTCOME VARIABLES	
		Employment	Income
I Full sample	Mean standardized bias	1.75	2.74
	Pseudo-R2	.001	.004
II Men	Mean standardized bias	5.18	10.80
	Pseudo-R2	.016	.03
III Women	Mean standardized bias	1.59	2.54
	Pseudo-R2	.002	.004
Female subsample:			
IV Low educational level	Mean standardized bias	3.28	7.10
	Pseudo-R2	0.004	0.02
V High educational level	Mean standardized bias	2.74	3.56
	Pseudo-R2	.002	.005
VI Younger cohort (≤ 30 years)	Mean standardized bias	3.68	5.34
	Pseudo-R2	.005	.019
VII Older cohort (> 30 years)	Mean standardized bias	3.41	4.36
	Pseudo-R2	.007	.012
VIII First wave	Mean standardized bias	2.01	3.89
	Pseudo-R2	.002	.011
IX Second wave	Mean standardized bias	2.48	3.63
	Pseudo-R2	.005	.009

Table 5 Robustness Analysis (Radius matching)

Sample specification		# Treated	# Controls	Employment	Income (in Euros per month)
				OUTCOME VARIABLES	
I	Full sample — employment / income	264 / 223	500 / 420	0.420 (0.038)	411.2 (40.9)
II	Men — employment / income	68 / 58	118 / 97	0.406 (0.080)	374.3 (108.7)
III	Women — employment / income	196 / 165	382 / 323	0.421 (0.043)	388.8 (42.1)
	Women subsample				
IV	Low educational level — employment / income	63 / 55	127 / 111	0.449 (0.076)	389.05 (74.6)
V	High educational level — employment / income	134 / 110	244 / 203	0.381 (0.05)	364.3 (52.3)
VI	Younger cohort ($\leq$ 30 years) — employment / income	89 / 75	157 / 133	0.375 (0.064)	341.7 (66.03)
VII	Older cohort (> 30 years) — employment / income	108 / 90	225 / 190	0.429 (0.054)	406.2 (53.9)
VIII	First wave of ICS — employment / income	75 / 59	382 / 323	0.351 (0.059)	332.3 (66.75)
IX	Second wave of ICS — employment / income	122 / 106	382 / 323	0.463 (0.044)	424.6 (43.3)

Notes:

ATT estimates and bootstrapped 95% confidence intervals in parenthesis. We apply a Radius-matching estimator with Caliper of 0.1. Bootstrapped confidence intervals have been calculated based on 200 replications.

Table 6 Further results (Kernel matching)

Sample specification (Women subsample)		# Treated	# Controls	Employment	Income (Euros per month)
				OUTCOME VARIABLES	
Workers that have collaborated with the firm that eventually hired them	employment / income	103 / 89	382 / 323	0.451 (0.047)	445.7 (48.3)
Workers that did not collaborate with the firm that eventually hired them	employment / income	93 / 75	382 / 323	0.387 (0.055)	320.9 (52.5)