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ABSTRACT

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

We analyze the effects of an ALMP for disadvantaged workers implemented in a depressed area of Italy. Using propensity-score matching, we find that a) the employment subsidy had a positive effect for participants on both the probability of finding a job and income, b) the outcome of the policy was more positive for women, and c) the program was more effective for older and less-educated female workers. Using data on previous contacts between workers and firms and on informal channels for job search activity, we ultimately explore the role of the program in promoting the transition from informal to salaried employment.

JEL Classification: C14, C83, J64, J16

Keywords: employment subsidies, female labor-force participation, evaluation, informal economy

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1 Introduction

Women's participation in the labor market is a classic topic in the labor economics literature (see for example Killingsworth and Heckman, 1986), and gender issues are receiving increasing emphasis in the policy agenda. A leading example of this phenomenon is the so-called "Lisbon Strategy" of the European Union, which set a target of a 60% employment rates for women by 2010. Still, across European countries we find highly heterogeneous conditions and the EU has clearly indicated the need for both structural welfare and labor 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 labor market programs (ALMPs) for women in Europe, an impact evaluation analysis is of primary importance if we want to deepen our knowledge of the forces driving gender differences in participation rates and income levels. That notwithstanding, the literature on the effectiveness of ALMPs on employment and participation outcomes for women is not vast and as 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]

In principle, ALMPs directly targeted at increasing the probability of unemployed workers to return to work could have a relatively larger and more 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's labor supply could have higher elasticity and therefore, increasing their chances in the labor market can result in a relatively stronger effect for this group of workers. Second, women may have higher reservation wages and higher outside options to stay out of the labor market: provided ALMPs increase the offer rate, women will obtain higher marginal benefits. Third, in markets in which discrimination is important and women's labor market attachment is low, ALMPs can help women signal their productivity, thus increasing their employment probabilities.

The need for gender-specific labor market policies is particularly pressing in Italy. Of 135 countries, the Global Gender Gap Report ranks Italy in 90th place in Economic Participation and Opportunity, because women are much more likely to be unemployed (or out of the labor force) and to earn less than men.[2] The average employment rate for Italian female workers in 2010 was 46%, well below the abovementioned European Lisbon strategy target of 60%, and only two out of twenty Italian regions reached the 2010 target.[3] The average employment male rate, 69%, was almost on target (70%) in all Italian regions. As Del Boca (2005) suggests, Italy therefore is an interesting case study to investigate both the dynamics of low labor force participation by women and how ALMPs may influence participation. That notwithstanding, although particular attention to gender differences in the evaluation literature has been devoted to studies of Nordic or Central European countries, very few studies focus on Southern Mediterranean countries.

In this paper we attempt to fill the gap by providing evidence of the effectiveness of an ALMP called *Interventi di Coesione Sociale* (Interventions for Social Cohesion, or ICS), which was recently implemented in the Southern Italian region of Sardinia.[4] The main aim of this ALMP was to improve employment probabilities and income for disadvantaged workers using a set of interventions comprising, among other tactics, counseling, employment subsidies and matching services. At first, this ALMP was considered as a pilot study of the Italian national program for labor market policies. However, its primary intervention consisted of a temporary employment subsidy paid to private firms hiring eligible workers. Like most ALMPs, even the ICS program was not specifically targeted by gender but was more broadly targeted to disadvantaged workers. However, eligibility conditions were different depending on gender: women needed only be unemployed, whereas men had to fulfill stricter conditions. As a result, 75% of our sample (and of program participants) are women. This is hardly surprising, because Sardinia is among Italy's least-developed regions, and in terms of the labor market gender differential, LFS data indicate that Sardinia's female labor market participation rates are quite low (50% in 2010).

Our empirical analysis uses propensity score matching methods to investigate the effect of the studied ALMP 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 sample of female workers indicate that the policy had a substantially positive effect on both the probability of finding a job for the group of treated individuals (about 43%) and on their level of income (397 euros per month). We also check for the presence of effect heterogeneity for specific subgroups 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% versus 40%) and for older workers with respect to the younger cohort (43% versus 37%). Furthermore, we obtain a larger effect for women than for our sample of disadvantaged men. However, when comparing these results, we always need to take into account the different eligibility conditions for men and women.

Together with the standard impact evaluation analysis, during the survey we also collected detailed information on pre-treatment search activity, unemployed individuals' previous experience and possible previous contacts between eligible workers and the firms that subsequently hired them. Descriptive statistics show significant differences between treated and non-treated workers. First, and unexpectedly, treated individuals are less active in searching for a new job during the unemployment spell than are non-treated individuals. Second, when searching for a job, treated individuals are significantly less likely to use the public employment services channel and are more likely to use informal search methods such as personal contacts, friends and relatives. Third, using the answers received in our survey to the question "Before being hired, did you have the chance to collaborate with the firm that hired you?" we find that the sample of treated individuals has a significantly higher probability to have had a previous contact with the firm. Overall, these three pieces of evidence are consistent with the hypothesis that at least in part, the ICS program has been ultimately effective not only in creating new employer-employee matches but also in promoting the transition from informal employment to formal jobs. Indeed, compared to other OECD countries, Italy has a significantly sized shadow economy.[5]

2 Description of the program

The ICS (Interventions for Social Cohesion) policy is the first comprehensive ALMP to be implemented in the Italian region of Sardinia. It was first activated as a pilot program to develop similar policy interventions on a national scale and was supported jointly by the Italian Ministry of Labor and Social Policy and the Sardinian regional government, which bore full responsibility for its actual implementation. Although the program was formally launched in June 2004, it did not actually begin until 2006.

The ICS program was aimed at reducing unemployment and increasing re-employment probabilities for different groups of disadvantaged workers. The financial resources allocated to implement the program were 9,536,000 euros: 5,916,000 financed by the Ministry of Labor and Social Policy and the remaining 3,620,000 by the regional government. The program's eligibility conditions varied depending on the applicant's gender. Women only needed to (a) be unemployed, (b) not receive any unemployment subsidy and (c) be resident in Sardinia. Men had to fulfill more stringent conditions: (a) they were required to be long-term unemployed (unemployed for at least 24 months, certified by the local Labor Office) or they were required to (b) be older than 44 years of age, (c) not receive any unemployment benefit at the moment of application for the program, and (d) reside in Sardinia.[6]

In principle, the ICS program was a composite labor market policy that involved several types of interventions on both the labor demand and supply sides, thus directly targeting firms and unemployed workers. The interventions specifically directed toward firms were not eventually implemented and much more attention was paid to the labor supply side of the program through a mixture of policies including employment subsidies, counseling, tutoring and matching services.[7] The latter service consisted of the possibility for unemployed workers to be directly matched to a vacancy in a firm that exactly required her/his qualification profile.

However, the most relevant type of intervention envisaged by the ICS policy was a typical hiring subsidy. Firms received an employment subsidy of 460 euros/month for a maximum period of 12 months. This 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 additional 6-month period, firms also received a additional lump sum payment of 2,000 euros if they hired the worker on a permanent contract. In addition to these monetary incentives, workers and firms were entitled to receive counseling support in identifying their occupational needs, with tutoring related to the workings of the ICS policy and the rules governing local and national hiring procedures.

As noted above, to reduce two-sided search costs, the ICS program also offered the possibility for workers and firms to use a specifically organized matching service provided by a public employment agency, INSAR. At first, this was intended as the most innovative intervention of the ICS program, because effective public employment services were absent in Sardinia. However, bureaucratic and organizational problems made this part of the policy substantially unsuccessful: to match workers and firms, INSAR developed a database that included the firms' occupational needs and some demographic characteristics of participating workers. Indeed, not only did the database development take much longer than expected but also it ultimately lacked significant and relevant information, and it was unsuitable for efficiently matching employers and employees. As a leading example, it is sufficient to say that the data collected contained no information about workers' educational attainment that might have helped match workers to firms.[8] Given all of these difficulties, such an (apparently) appealing feature of the intervention was rarely used.

The unexpectedly large numbers of applicants (more than 10,000) caused long delays in all of the bureaucratic procedures, and the program implementation had to be postponed. Consequently, 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 waves of participants: the first call was opened from June to December 2006, attracting the interest of 533 firms seeking 1,258 professional profiles.[9] The second call was opened in December 2007 and closed in March 2008, with 423 firms applying for the program and seeking 952 job profiles. This second wave of the program explicitly introduced the possibility of direct hiring (*chiamata nominativa*) for firms willing to hire one or more particular workers in the participant pool. However, this possibility was ultimately allowed even during the first wave and as we will see in the following sections, this option eventually crowded out the public placement service offered by INSAR. Overall, 877 ICS beneficiaries enrolled in the program. They agreed and began to participate to the program at different times, and the last worker enrolled at the end of June 2008. From the initial sample of 877 individuals, only 795 were eventually hired by firms, with the difference attributable both to dropouts (27) and to firms that decided not to hire the worker following the probation period (55).

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 of implementing the program. As discussed in the previous section, this dataset only contains basic information on personal characteristics (age, gender, place of residence and professional qualifications) for the 7,955 individuals who expressed interest and submitted the application form to become eligible for the ICS program.[10]

Excluding the dropouts, approximately 10% of those who expressed their interest in the ICS program (the 850 beneficiaries) were matched with an employer and eventually treated.[11] For them, the administrative source contains the following additional information: the exact date on which the worker was hired, participation in training, the characteristics of the contract offered to the worker (length, type of contract, hours/week), occupational skill profile, and information on the employment subsidies and the lump-sum payment of 2,000 euros granted to firms hiring workers on a permanent contract.

Because this administrative dataset lacked important information relevant to the policy evaluation, we decided to complement the dataset with additional survey data. Thus, a second set of data was collected through computer-assisted telephone interviews (CATI) that occurred between December 2009 and March 2010. First, we interviewed a sample of 462 beneficiaries of the ICS program.[12]

To avoid potentially upward-biased estimates due to locking-in effects, we exclude from this sample the 161 workers who were still under treatment or were very close to completing it when interviewed.[13] It is important to stress that for each beneficiary, the ICS treatment was complete after a period of 18 months, i.e., the initial 12 months during which the firms were receiving the monthly hiring subsidy plus the subsequent 6-month period that was necessary for the firms to eventually obtain the lump-sum payment. Overall, our final sample of treated include those 301 workers who completed the treatment (i.e., the employment period of at least 18 months after signing the contract) at least 3 months before the interview.[14]

Second, we selected the group of non-treated individuals from the sample of those who expressed interest in the program but did not receive the treatment. In this case, we extracted 1419 individuals to match the distribution of participants in terms of gender, age and geographic area of residence.[15] Thus, as we shall see below, the treated and non-treated groups are quite similar in terms of the observed characteristics. After excluding non-respondents and unavailable individuals, we ended up with a sample of 558 non-treated individuals.[16] This led us to a final sample of 859 individuals, including 301 participants and 558 non-participants.

3.2 Descriptive statistics

The questionnaire used for the interviews enabled us to collect important additional information on individual pre-treatment characteristics that were missing in our administrative dataset and that were essential to perform our propensity score matching analysis. We therefore turn to a more accurate description of these variables, with particular attention to those used in the propensity score analysis. Table 1A provides descriptive statistics for the most relevant variables concerning demographics, and Table 1B includes data describing previous job experiences and job search activity. In both tables, we distinguish between treated and non-treated individuals and separately report descriptive statistics by gender. For each variable, we also identify whether the difference in the mean values of treated and non-treated individuals is significant. Below, we primarily discuss the data of the female subsample, because those individuals constitute approximately 75% of our final sample (643 observations, including 222 participants and 421 non-participants) and are the focus of our study. In terms of demographics, Table 1A shows significant heterogeneity between men and women, whereas the differences between treated and non-treated are often trivial. Gender differences are easily explained by the diverse eligibility criteria of the ICS program for the two groups. On average, female participants are 32 years old and as expected, they are slightly younger than men, because one condition of eligibility for the latter group is long-term unemployment (i.e., being unemployed for at least 24 months) and/or being more than 44 years old. We have also asked for information concerning marital status and the presence of children, because both variables purportedly influence participation in the labor market, especially for women (see, among others, Killingsworth and Heckman, 1986). Singles represent approximately 60% of the sample of treated females, whereas 34% of the treated sample reports having at least one child when they applied for the ICS treatment. When considering the sample of non-treated, we observe a higher percentage of single women (almost 64%), whereas the percentage of women with children is very similar to that reported for the treated group. When we look at men, we find that the percentage of men with children is substantially lower than that of women at approximately 24% for both treated and non-treated individuals, whereas the percentage of singles is higher (63% for treated and 72% for non-treated men).

Labor-force participation is also related to informal care for the elderly and disabled people in the household (Leigh, 2010). This may be particularly important for our sample of Italian women for two related reasons: the lack of adequate formal care services provided by the Italian welfare system and the traditional role of women as caregivers (Norton, 2000). We find that the percentage of women living in a household in which at least one person is in need of long-term care is always lower for the treated sample (approximately 12% versus 18% for the non-treated sample).

We now turn to an analysis of educational attainments and self-reported professional skills. Again, we do not find significant differences between treated and non-treated individuals. As expected, women have higher educational attainments than the more disadvantaged sample of men. In general, Italian university-level attainment remains below the OECD average: its percentage of the population (25-64 year-olds) that has attained tertiary education in 2009 is 15%, significantly below the OECD average of 31% (OECD, 2012). Moreover, Italy has one of the highest regional dispersions of education attainment.[17] Thus, compared to the Italian average, the percentage of women that completed tertiary education in our sample is high for both treated and untreated individuals at approximately 20%. The largest proportion of our female sample left school at the upper secondary school level (approximately 47% for both groups) but a relatively high percentage, approximately 32%, ended their formal education before upper secondary school. This last piece of evidence should not come as a surprise because Sardinia is characterized by very high percentage of upper secondary school dropouts. In particular, secondary school dropout rates are higher among boys than girls, and this evidence is confirmed in our sample of disadvantaged workers, for whom we observe that almost half of our male (both treated and non-treated) samples ended their formal educations at the lower secondary level. Moreover, men show significantly lower percentages of tertiary education attainments than women (11% for treated and 14% for non-treated individuals, respectively).

Finally, Table 1A includes the distribution of individuals' self-reported professional qualifications, providing a proxy for skills. Professional qualifications are particularly relevant for the purposes of our study because the public placement service provided by the regional government to workers and firms was essentially based on workers' self-reported qualifications and on firms' desired job profiles. As expected, we find significant differences between the sample of men and women, reflecting the lower educational attainments of the former group. In particular, a large portion of the women sampled, approximately 73%, certifies professional skills suited for the service sector. For example, the proportion of (treated) women reporting "administrative office" skills is 40%, versus 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 zero for women (less than 2% for the sample of participants). Furthermore, we observe that unskilled workers are slightly overrepresented in the sample of treated women (approximately 17%) against 12% for the non-treated women.

A second set of controls includes measures of previous experience and job searches (Table 1B). Unlike the set of individual characteristics discussed in Table 1A, we do observe significant differences between the treated and non-treated samples.[18] One exception is found when we ask whether applicants received unemployment benefits pre-treatment: in this case, we observe similar percentages for both groups. Conversely, when applicants were asked whether they received some training before the ICS policy was implemented, differences arise: the 13% of treated individuals who replied "yes" to this question, a rate significantly lower (32%) than that observed for the sample of non-treated individuals (45%).

One of the most significant differences between treated and untreated individuals is observed in terms of the individuals' previous job search histories. We find that the treated samples of both men and women have lower probabilities of being job seekers: the percentage of treated women who actively searched for a job before applying for the ICS program was approximately 70%, versus 83% for the non-treated. Interestingly, more than half of those who actively searched for a job have received a job offer: the percentage is higher for treated than for untreated individuals (47% versus 40% in the women subsample). However, the percentage of those who accepted such an offer is approximately 45% for the female sample of treated individuals versus nearly 54% for the sample of non-treated individuals.

When considering different search methods employed by job seekers, we also find significant differences between treated and non-treated individuals.[19] Table 1B, shows that when searching for a job, treated female individuals are significantly less likely to use the (few) existing public employment services as first option (22% versus 49% for non-treated individuals) and are more likely to use personal contacts, friends and relatives (33% versus 14% for non-treated individuals). Indeed, the latter method is one of the most important search channels for the sample of treated women. Finally, the data suggest that the Internet is among the most-used job search channels among the treated, with more than 30% of respondents using such methods, whereas non-treated individuals seem less likely to exploit the opportunities offered by new technologies. In our survey, we also asked individuals to identify their two most important search channels. Some respondents reported exploiting only one search channel, and this percentage (not included in the tables) is significantly lower, 24%, for the untreated than for the treated individuals, at 48%. Moreover, among the treated individuals, those who exploited only one search channel declare to have almost exclusively (70%) used the informal channel.

At first, these figures suggested that among the eligible individuals, the ICS policy was effective in attracting less-motivated individuals. In principle, it is possible that the program has been successful in helping the transition of this discouraged group of workers into the labor force. However, this evidence is also consistent with a different hypothesis. Given the large share of the informal sector in the Italian economy, it is possible that treated individuals were not actively seeking a job because they were already working under informal employment contracts. Consequently, the ICS program may have increased the probability of transition from the informal to the formal sector.

To further investigate this possibility, in our survey we also asked a series of questions concerning both applicants' overall job experiences and details about previous interactions with firms that ultimately hired them after their application to the program. Remember that to become eligible for the program, individuals had to be formally unemployed. The first set of questions refers to the most significant previous job experience. When asked about the type of job they had before becoming unemployed and applying to the program, respondents faced three possible answers: short term, permanent and irregular contract. Interestingly, whereas almost half of the sample said they had a short-term contract, more than 10% of both groups reported they previously worked without a regular contract, and this percentage is higher for treated than for non-treated individuals, at 13% and 11%, respectively (for the female sample).

We also asked additional questions to individuals who by the time of the interview had eventually obtained a job through the program (treated) or using alternative channels (for the untreated). In particular, we asked whether before applying to the ICS program, they had the opportunity to collaborate with the firm that eventually hired them and in the event of a positive answer, what type of relationship they had. Among the (female) treated, 64% answered positively to the first question; conversely, when the same question was asked to the non-treated individuals who eventually found a job without program support, this percentage drops to 6%. Moreover, when asked about the type of collaboration they had with the firm before applying for the ICS policy, 93% of the interviewed sample refused to answer, whereas among the very few that gave an answer, 5% declared that they had been working under an informal employment contract. Note that the high rejection rate observed for this question has an easy explanation because in theory, it was possible to infer from their answers the firms that had behaved illegally.

Thus, the overall evidence reported on job search behavior and on previous contacts between workers and firms suggests the possibility that the ICS policy was ultimately used by both workers and firms to increase the transition rate from informal to formal employment relationships. This hypothesis will be further investigated in the following sections.

4 Empirical analysis

4.1 Identification strategy

Our analyses of the effects of the ICS ALMP use the standard framework of the potential outcome approach to causality or the Roy-Rubin model, and they focus on the average treatment effect on the treated (ATT), that is, on the effect the treatment shows for individuals who actually participate in the program. Below, we briefly describe the identifying assumption of propensity-score matching (PSM) methods that we apply in our study.[20] PSM methods need to find a group of treated individuals, who are similar to the control group in all relevant pre-treatment characteristics, the only difference being that one group was exposed to the evaluated program whereas the other group was not.

This methodology relies on two key assumptions. The first is the conditional independence assumption (CIA), or unconfoundness, which implies that selection for the treatment is exclusively based on observable characteristics, X, not affected by the treatment. This assumption implies that our study observes (and controls for) all of the variables that simultaneously influence both treatment assignment and potential outcome. The second assumption is the common support condition that implies that individuals with the same X values have equal positive probabilities of either receiving or not receiving the treatment. This 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 estimates the propensity score, that is, the probability of participating in a program conditional on X. It can be shown that under the CIA assumption, all bias due to observables can be removed by conditioning on the propensity score. We omit further details here while referring to the vast literature on the subject (e.g., Rosenbaum and Rubin, 1983; Heckman et al., 1998; Dehejia and Wahba, 2002; Smith and Todd, 2005; Caliendo and Kopeinig, 2008). Below, we claim that it is plausible to assume that our Xs, 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.

We first analyze the variables that determine the selection into treatment, and then we estimate the ATT through a matching algorithm. Before discussing the propensity score specification, it is important to recall that two characteristics of our dataset should help decrease the possible presence of bias in our estimates.[21] First, we use the same source of data (administrative and survey data) for both treated and non-treated individuals. This should ensure that Xs in our probit model are similarly measured across the two groups. Second, we have collected survey data from a sample of eligible nonparticipants and participants.

Moreover, we also have a potential rich set of suitable additional covariates and are therefore able to control for many characteristics that are likely to determine both participation and labor market outcomes. That said, in choosing our X variables we will also consider that the inclusion of too many variables in the propensity score can result in a higher standard error for the estimated propensity score and may reduce the likelihood of finding common support.[22] The choice of excluding some variables is primarily justified by the presence of too many missing observations that would cause a significant reduction in the sample size.[23] 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 the elderly/disabled needing 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.[24] All of 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 participants' age and gender to have an effect on the probability of treatment, because they also have an effect on individuals' labor market outcomes, such as participation and employment status. In the same spirit, the number of children, the presence of elderly/disabled in the household and marital status are supposed to influence the probability of participating in the program, especially for women. Likewise, dummies for previous occupational profile and education levels are included, because they should have an effect on the selection into treatment. Finally, previous job search activity, past participation in training programs, and having received an unemployment benefit should also influence the selection into treatment.

Table 2 includes the results of the probit estimation on our main subsample of women. Because of the presence of missing values on the control set, we are left with 579 observations (197 treated and 382 non-treated).[25] Overall, results in the table are as expected, with two possible exceptions. Indeed, the pre-treatment dummies on both job search activity and participation in training programs show a negative and significant coefficient, and we will return on these somewhat puzzling results in the following sections. To give a better idea of the quality of our estimates, in Figure 1 we also report the distribution of these estimated propensity scores. This distribution clearly shows that the overlap assumption is satisfied, because the propensity scores' distribution of the treated clearly overlaps the region of the propensity scores of the non-treated.

Below, we discuss the results for the effect of the ICS policy on employment and income obtained by applying the kernel matching method to different samples, together with their corresponding quality measures. We claim that the kernel matching method is the most appropriate in this context, because the calculation of the outcome includes the highest possible number of matched non-treated. In particular, when treated and non-treated samples are not large, as in our case, this method enables us to obtain efficient estimates of the policy's effect.[26]

4.2 Main results

As mentioned above, the direct aim of the program was to integrate unemployed individuals into the labor market. In other words, we first check whether this policy has caused an increase in the probability of acquiring and keeping a job for the pool of participants, using a standard binary outcome variable to measure workers' employment status following the termination of the program. Second, as is typical in the literature, we also estimate the effect of the ICS program on participants' incomes. For the latter dependent variable, we do not have access to administrative data on income and exploit the additional information at the self-reported level of net monthly disposable income.[27] Note that unlike the employment status analysis, many interviewed individuals refused to answer specific questions about their income, which explains the observed reduction in the sample size of both the treated and the control groups.

To set the scene, before focusing on the subsample 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 ATT of our two possible outcomes: employment and income. Considering the full sample of ICS participants, our results suggest a 42% higher probability of employment for participants than non-participants. Similarly, the analysis also suggests a 403-euro increase in average monthly earnings for participants compared to non-participants with identical observable characteristics. The matching quality for this sample is analyzed in Table 4, which includes the mean standardized bias and the pseudo-R2 after matching. That table reveals that the resulting pseudo-R2 from the propensity score estimation is low, suggesting a successful match.[28] In general, Table 4 shows reassuring values for both income and employment in most of the subsamples analyzed. Thus, unless necessary, we do not further discuss this issue.

Moreover, because the eligibility criteria for the two gender groups are different, below we appropriately divide the sample by gender. Even if the two subsamples results are not entirely comparable, it is nevertheless interesting to investigate possible gender differences in program outcomes. Given the *a priori* characteristics of our subsamples, we may find either higher or lower effects. As discussed above, we may expect to find higher effects for women for various reasons: a higher elasticity of women's labor supply, higher reservation wages, and previous evidence related to areas such as Sardinia, where women have low participation in the labor market. However, recent findings in the literature also suggest that ALMPs may be more effective for individuals from more disadvantaged categories.[29] In the case of the ICS program, to become eligible for the program, men (unlike women) were required to belong to genuinely disadvantaged categories, and thus we cannot predict whether the effect is higher for women or for (disadvantaged) men.

Overall, even if the difference of the estimated effects between the two groups is not statistically different from zero, the analysis suggests a larger effect of the program for women: the results reported in rows II and III of Table 3 show a 43% higher probability of employment for women and a 40% higher probability for men.[30] Likewise, income levels for women are higher than for men, with the difference between treated individuals in the two groups being approximately 50 euros. Again, all of the estimated effects are statistically significant at conventional levels. However, the matching quality for the small sample of men, as reported in Table 4, is less satisfying and the results must be interpreted with caution.

Accordingly, from now on we specifically focus on the group of women that constitutes approximately 75% of our full sample and further investigate whether we find different effects for different subgroups. Because 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. As found above for the male-female case, the estimated differences between these groups are not statistically significant. Still these results offer some useful suggestions.

We first divide the sample into less (lower secondary school) and more (upper secondary or above) educated individuals. In the descriptive section of the paper, we emphasize the relative importance of highly educated female workers in the sample, and we first checked whether there were any differences in the effect of the policy related to different levels of education. We find (see rows IV and V, Table 3) that less-educated women benefit relatively more from the ICS program: the estimated effect on the probability of being employed is equal to 45% versus approximately 40% for women with upper secondary or tertiary educations. When considering income levels, we find that women with lower levels of education earn an average of 420 euros per month, approximately 50 euros more than more-qualified workers. This pattern is not surprising, because the ICS policy was mainly targeted towards more-disadvantaged groups, and similar results have been found in the literature (see Caliendo and Kunn, 2011).

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 under

30 years of age have an approximately 37% higher employment probability than non-treated individuals, whereas this effect increased to 42% for participants in the age group equal to or older than 30. There are various possible explanations for this result. On the one hand, because age is a proxy for labor market experience, this result may indicate that the ICS program was relatively more effective for more experienced women who were out of the labor market for some reason, 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 more disadvantaged in terms of labor market opportunities, possibly because their human and search capital was largely depreciated and thus, the ICS policy reached its target of increasing their opportunities in the labor 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 noted above, the first wave of the ICS program was launched in 2006 but due to long delays in all bureaucratic procedures, a significant number of firms dropped out of the program. A second call was then launched in 2007 and more matching of workers with firms was performed. Thus, across beneficiaries we have two groups of people who entered (and ended) the program during two different periods. Consequently, the results for the two waves may be interpreted as a short-term effect (second wave) and a 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 remain employed compared to non-participants whereas, as expected, the estimated short-term effect (second wave) is higher and equal to 47%. Unlike the other groups comparisons, we find that the difference in estimated effects between the first and the second wave is statistically significant. This suggests that the effect of the policy tends to fade out over time, and it raises doubts about the possibility of even smaller long-term effects.

Overall, the magnitude of these effects is substantial but neither uncommon nor new in this literature, especially when considering the effect of employment subsidies to private firms on the probability of being employed. Indeed, similar results have been found in different contexts and countries and suggest the effectiveness of such types of interventions.[31]

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 find no significant change in estimated effects. Results using radius matching are shown in Table 5.[32]

4.3 Further results: informal search channels and informal employment

So far, the estimated effects of the policy across all samples have been interpreted in the standard way, that is, as the probability that treated individuals can transform their unemployment status into a salaried position. However, this is likely to tell us only part of the story. Below, we explore another complementary (and not necessarily alternative) explanation and examine whether this policy has also served as a tool to allow the conversion of informal employer-employee agreements into formal employment relationships in an area characterized by high unemployment levels.

Aggregate cross-country data show that with the exception of Greece, Italy has the largest shadow economy (in percentage of official GDP) of all industrialized countries: in 2007, its percentage was estimated at 22.3%, whereas the average value for the 21 OECD countries was below 14%. Furthermore, this phenomenon is highly heterogeneous across regions, with Southern areas, such as Sardinia, having shares that approximately twice those observed in the North.[33] The same pattern is observed for unemployment rates, which for the past three decades have been three to four times higher in Southern Italy than in Northern Italy. The observed high incidence of informal work in Italy (and Sardinia) is explained by both the absence of a universal system with adequate levels of unemployment benefits and the presence of high hiring costs.[34] Below, we therefore investigate whether this has been also the case for the ICS policy.

Suggestions that our estimated policy effects are likely to be not only new matches but also transitions from informal to formal employment are discussed in the previous sections. In particular, in the descriptive section we see that, with respect to the non-treated sample, the percentage of beneficiaries that declared an active job search before the treatment is significantly lower, and results from the propensity score reported in Table 2 confirm this, showing a negative and significant coefficient of job search activity on the probability of receiving treatment. Second, beneficiaries also received less training, and this evidence is largely consistent with the idea that treated individuals were less active in job-searching (and training) activities, because they already had one. Third, as noted in Section 3.2, we find clear-cut evidence that participants in the ICS program are more likely to search for a job using informal methods such as personal contacts, friends and relatives, and also to use this search channel exclusively. Again, this is consistent with the presence of previous informal employment because the informal economy is often based on verbal and illegal contracts and necessarily must be more likely to exploit social relationships based on trust than to use formal agreements.

Following previous recent studies, we further investigate this issue and examine the characteristics of those workers who rely more on informal contacts when searching for a new job; we estimate a probit model in which the dependent variable is a dummy that takes the value of one if the individual has answered using informal channels, that is, "personal contacts, friends and relatives", to find a job. We include a standard set of covariates: age, educational attainments, previous unemployment benefits, previous training, home ownership, marital status, presence of children and professional qualifications. Results on both the full sample (models 1 and 2) and the female subsample (models 3 and 4) , reported in Table 6, are consistent with the evidence found in previous similar empirical analysis.[35] Unlike other studies, we also introduce in our set of controls a dummy on previous contact with the firm (models 2 and 4). More specifically, we use the answers to the question "Before being hired, did you have the chance to collaborate with the firm that hired you?" The idea was for this question to capture the presence of previous connections and possibly, previous black-market agreements between workers and firms. This may also explain why almost 20% of the sample did not answer this question: they were worried about signaling the presence of a previous illegal agreement.[36] As expected, our proxy for informal employment shows a positive and significant coefficient. Overall, our results are also consistent with the evidence found in the literature on social capital.[37] They suggest that informal contacts are used more frequently by less-educated and older individuals, who tend to use this channel exclusively.

From our descriptions in Section 3.2, we also know that the sample of treated individuals has a significantly higher probability of a previous relationship with the firm. We therefore use the possibility offered by our data and run a separate analysis to investigate the policy effects for the subsample of individuals who declared having known and collaborated with the firm before being hired from those who did not. To save space, we do not show the results here; however, they indicate that the program's effect of is associated with negative employment probabilities, at -17%. Conversely, for those who reported no previous contact with the firm that subsequently hired them, the effect is both positive and very large (60%).[38]

A second possibility offered by our data is the ability to exploit the data separately for the sample of individuals who used the program's employment services versus the sample of workers hired directly by the firm (*chiamata nominativa*). In fact, it was impossible for firms to select a specific worker through the employment services offered by the ICS program, whereas this was possible in the latter case. This result implies the potential presence of transitions from informal to formal employment only in the second case. Unfortunately, unlike those selected through the option of *chiamata nominativa*, worker selection through the employment services is rare; all of the matches occurred during the first wave of the ICS program and thus, results could also be driven by medium- versus short-term effects. Nevertheless, our analysis suggests that faced with the choice between a public placement service offered by the regional government or choosing from a list of eligible unemployed workers selected from the previous (informal) agreements, the vast majority of firms choose the second option,[39] and the option of *chiamata nominativa* might also have crowded out the process of job creation. In fact, it is likely that (at least some of the) firms benefited from the employment subsidy to (re)hire workers who already worked for them under either temporary or informal contracts.[40]

Overall, our results are consistent with the idea that the hiring subsidies offered by the ICS program have decreased hiring costs for firms and that the latter took advantage of this possibility, at least in part, to convert previous informal (black market) agreements into formal employment contracts. It is also possible that policy makers saw this as a potential (and positive) side effect of the program. In this case, our analysis implies that the ICS policy has been inefficiently designed. Indeed, the expensive public employment and training services offered by the program, although useful for the unemployed, were unnecessary for undeclared workers. This also raises concerns about the long-term effects of the policy because firms may have primarily exploited temporarily reduced labor costs for individuals who were already working—they did not create new jobs. However, due to small sample size problems, all of these results need to be taken with a grain of salt and are more suggestive than they are conclusive.

5 Conclusions

In this paper, we study the labor market effects of a policy intervention (ICS) that was recently implemented in the Italian region of Sardinia. The intervention was administered by the regional government and had the objective of increasing the probability of employment for specific disadvantaged groups of workers. It consisted of a range of different actions, such as counseling, placement services and employment subsidies for private firms, the latter being the main intervention that was finally implemented.

The ICS program can be seen as a pilot (at least for some parts) of a comprehensive labor market reform currently under discussion in the Italian Parliament. First, because its characteristics, the implementation of this program may offer some useful lessons. More specifically, the failure of the program's matching services provides a warning about the inefficiencies of the Italian public employment services and their possible consequences in terms of policy effectiveness. Likewise, the program's hiring subsidy is similar to the social security break for new employees introduced in the 2015 Italian draft budgetary plan: there are high expectations for this national subsidy boosting labor demand in 2015.[41] Overall, although the differences between the regional hiring subsidy analyzed here and the national subsidy make direct comparison difficult, the results reported in this study can nevertheless offer some guidance to infer the effects of these future interventions.[42]

We estimate the effects of this policy using standard propensity score methods and focus on the sample of women, which constitutes approximately 75% of our total sample. First, our estimates indicate that the ICS policy increased the probability of female participants acquiring a job by about 43% and that effects are generally stronger for the women than for the men in our sample. This result is in line with other findings in the literature and suggests that women may significantly benefit from participating in this type of program. However, comparability between the two samples is reduced by the different eligibility criteria required of men versus women. Unlike women, to be eligible to participate in the program men had to belong to genuinely disadvantaged categories. Thus, because the literature suggests that ALMPs may be more effective for these categories of workers, it is likely that our estimates represent a lower boundary of real gender differences.

Due to these differences in eligibility criteria, we focus on the female subsample. Even considering this more homogeneous sample, we find the more disadvantaged categories showing larger policy effects. More specifically, the estimated effects are stronger for the two categories of low-educated and older female workers. Moreover, because the program was implemented in two distinct (albeit close) 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 wave, which seems to suggest that the policy's effect tends to decrease over time. Thus, it is also likely that with new data for an observation window longer than the period of support we examined here, the long-term estimated effects would be significantly lower.

Moreover, using further information about the implementation of the policy, pre-treatment search activity and informal contacts between workers and firms that is usually difficult to obtain, we have attempted to reveal both the role played by informal search channels in depressed labor markets and their possible interactions with an implemented ALMP. Our data offer many clues that indicate the possibility that the ICS program has been used to convert previously informal employment agreements into formal employment relationships, and they cast some doubt upon how the ICS policy has been designed.

In sum, our primary contribution is threefold. First, we provide some direct empirical evidence on the effects of ALMPs on women, which is particularly relevant to the debate on participation and labor-market interventions such as hiring subsidies and public employment services. Second, whereas the evaluation literature that pays particular attention to gender differences has been devoted to studies of Nordic or Central European countries, this is one of the very few studies to focus on the Southern Mediterranean area. Third, our data provide some suggestive evidence for the role played by informal agreements and family/professional ties in a depressed labor market. Together with the possibility of a follow-up interview for workers' long-term outcomes, future research avenues should further investigate the role of the informal economy in determining labor-market outcomes.

Endnotes

[1] See Card et al. (2011), p. F460. The presence of effects heterogeneity between men and women is found by, among others, Kluve et al. (1999), Gerfin and Lechner (2002), and Bergemann and Van den Berg (2008). More recently, Caliendo and Kunn (2015) focus on self-employment schemes (start-up subsidies) and find a general gender gap in terms of effects of these innovative ALMPs, even though results may vary depending on the choice of the outcome variable.

[2] On gender differences in the Italian labor market see, among others, Sulis (2012).

[3] Across 307 EU NUTS2 areas, the European region with the lowest participation rate differential between men and women is Stockholm (4.5%), whereas Emilia Romagna, the Italian region with lowest differential (17%), is in 218th place. Most Southern areas are included among those regions with the highest gender differences; Puglia (306 over 307) is the worst, with a 32% difference between male and female participation rates.

[4] More details can be found in INSAR (2008a) and INSAR (2008b).

[5] See Schneider (2012). See also Bratti et al. (2005) for the effects of the lack of a formal contract on women's labor market participation in Italy, and Boeri and Garibaldi (2001) for a matching model of the shadow economy with unemployment calibrated on Italian regional and sectoral data.

[6] Among those eligible, the ICS program also included special disadvantaged categories such as former drug addicts and alcoholics. However, because such individuals represent a small percentage of the ICS participants and can be considered as outliers, we have excluded them from the analysis.

[7] A similar policy has been described by Rodriguez-Planas and Jacob (2010), who examined Romania's Employment and Relocation services.

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

[9] The first report provided by INSAR (2008) calculates that during the first wave of the program, firms received the hiring subsidy for only 242 out of the requested 1,258 job profiles (the numbers are 344 out of 952 during the second wave). The main reasons for the high firm-dropout rate observed in the first wave listed in the report are the following: 1) the long delays of the bureaucratic procedures; 2) the skill mismatch between labor demand and supply; and 3) a reassessment made by firms about their financial ability to hire additional workers.

[10] More precisely, 10,408 applications to participate in the program were received, but 2,453 did not satisfy the admission criteria.

[11] The administrative dataset shows that the 27 dropouts quit the program immediately after receiving the proposal. From discussions with the staff that implemented the program, we see that in most cases, the dropouts found a job before receiving the ICS offer, but had not notified the regional administration. We exclude them from the analysis.

[12] During the interview process, approximately 10% of the sample (48 individuals) refused to grant an interview, and 25 arranged a later contact with the interviewer but they failed to appear. Moreover, 188 did not respond, and 127 were associated with an incorrect telephone number. Excluding both these individuals and the 27 dropouts, our interviewed sample consists of 462 treated individuals, with a response rate of 54%.

[13] The locking-in effect arises when a program evaluation is conducted at a point at which individuals still receive the treatment. To obtain unbiased results, the observation window should be longer than the period of support.

[14] We exclude all beneficiaries that enrolled in the program starting in May 2008. We also performed the analysis to include the 161 workers whose employment subsidy period was not yet (or had only recently) finished. Not surprisingly, we find larger effects and we interpret these results as plagued by upward bias due to locking-in effects.

[15] A very similar approach can be found in Rodriguez Planas and Jacob (2010).

[16] Considering the initial sample of 1,419 individuals, during the interview process, 338 (24% of the total sample) refused to grant an interview, 104 did not appear at the telephone appointment, 246 did not respond, and an incorrect telephone number was associated with 173 contacts. For our control group, we therefore have 558 observations and a response rate of 39.3%.

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

[18] In most cases, the mean values reported in Table 1B are based on a smaller sample size because a significant number of individuals refused to answer some of these questions.

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

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

[21] On this point, see Heckman et al. (1997) and Heckman et al. (1999).

[22] See Caliendo and Kopening (2008) for the advantages and disadvantages of over-parametrization. See also Khandker et al. (2010) and Byrson et al. (2002) for recommendations against over-parametrized models.

[23] This is the case for variables related to previous job offers (169 individuals refused to answer) and previous job experiences. We have 281 missing observations (of which 235 relate to the female subsample). We also exclude the job search methods variables: including them does not change the results, but only worsened the quality of the match.

[24] Area dummies are not included because, as noted 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.

[25] The initial female subsample was comprised of 643 observations (222 treated and 421 non-treated). Results for the alternative samples are available upon request.

[26] More specifically, we apply an Epanechnikov Kernel with a bandwidth of 0.06. Bootstrapped standard errors have been calculated based on 200 replications. To improve 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).

[27] More precisely, we do not have access to administrative data on income and we collected data using answers to the following question: "At present, what is your net monthly income (include unemployment benefit)?" In doing so, we obtained a measure of disposable income that includes both wage and non-wage income. On this point, see Lechner and Melly (2007).

[28] A mean standardized bias below 5% denotes the success of the matching approach. For more on this issue, see Caliendo and Kopeinig (2008).

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

[30] We have performed the equality tests for non-matched samples with the assumption of unknown and unequal variances and computing the p-values following the procedure described in Huber, M. Lechner, M. and Steinmayr A. (2014). On this, see also Caliendo and Kunn (2015).

[31] 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 in a similar labor market program had a probability that was from 32 to 57% higher of being employed than non-participants. Sianesi (2004) finds similar results for Sweden. Gerfin and Lechner (2002) find a probability equal to approximately 10% for Switzerland.

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

[33] See Schneider (2012) for cross-country data and Eurispes (2012) for details on Italian regions.

[34] Hiring costs are affected by high taxes, red tape, poor-quality government services and strict employment regulations. See OECD, *Employment Outlook*.

[35] See Ponzo and Scoppa (2011) and Cappellari and Tatsiramos (2011).

[36] We are aware that this is an imperfect proxy of this phenomenon: as noted in Section 3.2, we could not further verify whether workers and firms already had a previous informal employment relationship because a direct question to this issue had a rejection rate of more than 90%.

[37] "…[T]he effect of social capital is more pronounced among less educated people, who need to rely more on trust because of their limited understanding of contracting mechanisms." Guiso et al. (2004), p. 527.

[38] Unfortunately, missing values significantly reduce the sample size: even including both women and men, in the control group we are left with only 31 observations. Thus, these results need to be viewed with caution. Furthermore, results do not significantly change if we exclude men from the analysis, and we also find almost identical but non-significant results when using income as outcome variable.

[39] See Fougere et al. (2009) and Van den Berg and Van der Klaauw (2006) for papers studying the interactions between formal and informal search channels. See Loriga and Naticchioni (2012) for a study dealing with the role of Italy's public employment services.

[40] We thank an anonymous referee for useful comments on this part of the analysis. See Section 2 for more details on the two types of matching processes.

[41] We thank a referee for suggesting this point. The aim of this reform is to improve the governance and effectiveness of active labor market policies in Italy, strengthening social security through a wide unemployment benefits program and improving training and job-finding services. The potential gains of planned interventions in the area of ALMPs are particularly promising, but the improved functioning of public services is a long-awaited precondition.

[42] The new 2015 national employment subsidy is more generous than that examined in this study. Indeed, the former can last for (a maximum of) three years, whereas the ICS subsidy examined in this paper lasts only one year.

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Appendix: Figures and tables

Table 1A. Descriptive statistics: Demographics
Values in percentage

	Full sample		Female subsample		Male subsample	
	ICS Policy	**Non-participants**	**ICS Policy**	**Non-participants**	**ICS Policy**	**Non-participants**
Main demographics						
Female	73.8	75.5				
Age (in years)	32.7	33.5	32.1	32.6	34.4	36.4
Family characteristics						
Marital status single	60.5	66.0	59.5	63.9	63.3	72.3
Presence of children	31.6	31.5	34.2	34.0	24.1	24.1
Household members in need of care (yes)	11.32***	20.8***	12.18*	18.06*	8.82***	29.66***
Home ownership						
Yes	75.4	73.7	73.0	73.2	82.3	75.2
Educational attainments						
Lower secondary school or less	35.6	35.3	32.0	32.5	45.6	43.8
Upper secondary school	45.9	46.2	46.9	47.5	43.0	42.3
Tertiary education	18.6	18.5	21.2	20.0	11.4	13.9
Professional qualification						
High skills	2.3	3.4	2.7	3.3	1.3	3.7
Technical skills	7.0	9.5	4.5*	8.08*	13.9	13.9
Administrative office skills	36.2	39.8	40.1	42.0	25.3	32.9
Retail and service	29.2	26.2	33.3	31.4	17.7	10.2
Artisans and farmers	7.3	4.8	1.8	2.1	22.78*	13.14*
Blue collars	2.3	1.6	0.9	0.7	6.3	4.4
Other unskilled (unspecified)	15.6	14.7	16.7	12.4	12.66*	21.9*

Notes: This table shows the main descriptive statistics on the socio-economic characteristics of our sample of eligible individuals. Data are differentiated both between treated (ICS Policy) and non treated (non participants), and by gender. All variables refer to the pre-treatment period. *Household members in need of care*: refers to families where at least one person needs long term care (elderly, disabled). *Professional qualifications* are self-reported. * Indicates that the difference between treated/non treated is significant at 10%, ** 5%, *** 1%.

Table 1B. Descriptive statistics: Previous employment and job search
Values in percentage

	Full sample		Female subsample		Male subsample	
	ICS Policy	**Non-participants**	**ICS Policy**	**Non-participants**	**ICS Policy**	**Non-participants**
Subsidies and Training						
Received unemployment subsidy	29.9	31.4	28.8	27.3	32.9	43.8
Received training	32.56***	44.62***	31.98***	45.13***	34.2	43.1
Job offers						
Received offers	48.0	39.9	46.8	39.8	50.8	40.2
Accepted offers	44.76**	57.45**	45.2	54.0	43.75**	67.35**
Job Search						
Active job search (yes)	72.76***	84.41***	70.27***	82.9***	79.75*	89.05*
Job search methods						
Public employment services	22.37***	48.62***	21.79***	49.0***	23.81***	47.54***
Private employment services	4.6	5.5	4.5	6.0	4.8	4.1
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	6.9	6.8	8.3	6.3	3.2	8.2
Previous job experiences						
Short term	49.04***	61.35***	48.59***	63.16***	50.0	56.7
Permanent	38.94***	28.38***	38.03**	25.94**	40.9	34.6
Irregular	12.0	10.3	13.4	10.9	9.1	8.7
Previous network with the current employer						
Did you collaborate with this firm before applying to the program? (yes)	61.21***	7.17***	63.69***	5.98***	52.83***	10.83***

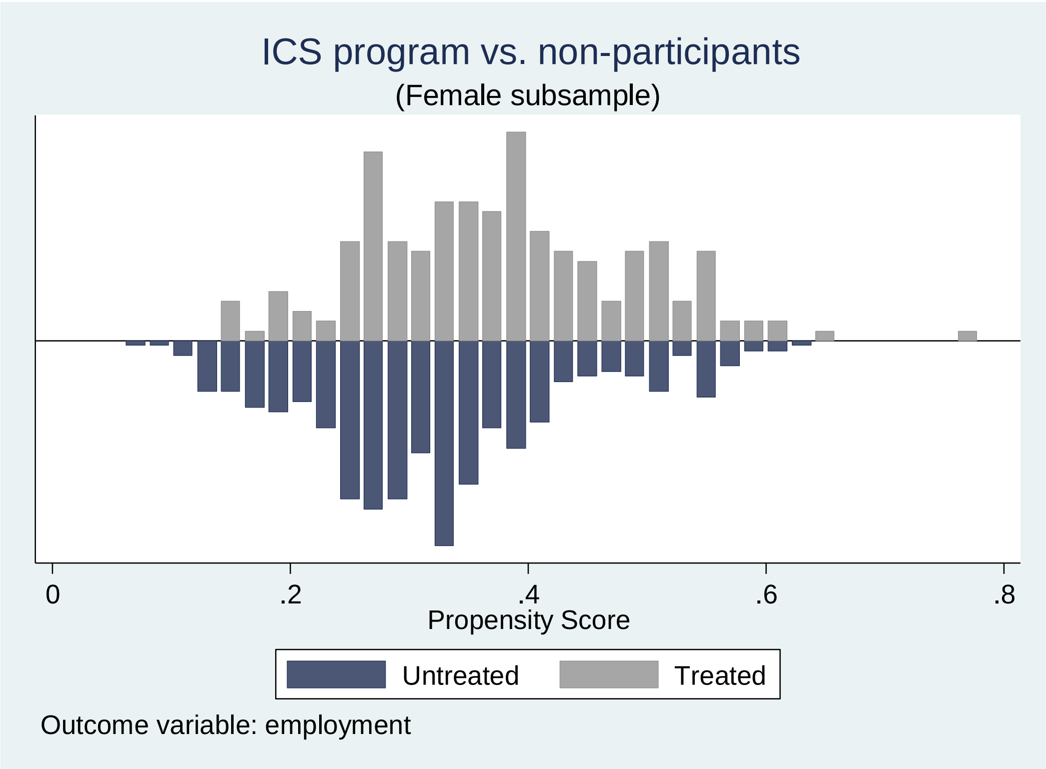
Notes: This table shows descriptive statistics on job experiences and search activity for eligible workers during the pre-treatment period. Data are differentiated both between treated (ICS Policy) and non treated (non participants), and by gender. *Job search methods*: respondents could indicate two different options, ranked by importance. *Previous network with the current employer*: percentages refer to the subsample of individuals that, at the time of the interview declared they had a formal job position (through the ICS program if beneficiaries, otherwise if non beneficiaries). * Indicates that the difference between treated/non treated is significant at 10%, ** 5%, *** 1%.

Table 2. Propensity score estimation: ICS policy versus non-participation. Female subsample

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
Household members in need of care (yes)	-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 skills	-0.037	0.048	0.433
Retail 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
Received 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 Section 3.2 for definition of variables. We focus on the subsample of women, but results on the full sample are available upon request.

Figure 1: Propensity score distribution



Notes: These are propensity score distributions for treated and non-treated individuals based on estimations in Table 2.

Table 3. Main results: kernel matching

Sample specification	# Treated *employment / income*	# Controls *employment / income*	OUTCOME VARIABLES Employment	 Income (in euros per month)
I Full sample	265/223	500/420	0.418 (0.033)	403.6 (40.9)
II Male subsample	68/58	118/97	0.406 (0.086)	345.5 (126.6)
III Female subsample	197/165	382/323	0.427 (0.043)	397.6 (40.3)
Female subsample				
IV Low educational level	63/55	127/111	0.453 (0.088)	420.7 (77.8)
V High educational level	134/110	244/203	0.396 (0.049)	368.9 (48.6)
VI Younger cohort (≤ 30 years)	89/75	157/133	0.369 (0.073)	345.3 (63.5)
VII Older cohort (> 30 years)	108/90	225 / 190	0.430 (0.057)	406.9 (56.7)
VIII First wave	75/59	382/323	0.336* (0.061)	331.2 (67.4)
IX Second wave	122/106	382 / 323	0.468* (0.044)	421.7 (45.5)

Notes: This Table shows the resulting ATT estimates. The 2nd and 3rd columns include the sample sizes for, respectively, the treated and control groups for the two different outcomes (employment and income). Standard errors are in parentheses and are based on bootstrapping with 200 replications. We apply an Epanechnikov Kernel with a bandwidth of 0.06. *Low educational level*: this subsample refers to individuals with, at most, lower secondary school attainment levels. *High educational level* refers to individuals with upper secondary school or above. *First wave* refers to the first call of the ICS program launched in 2006. *Second wave* refers to the second call was launched in 2007 (see also section 2). We performed the equality test comparing the estimated effects of the following groups: male vs female (II vs III), low vs high educational levels (IV vs V), young vs old cohorts (VI vs VII), first vs second wave (VIII vs IX). *Indicates that the difference of the two estimated effects is significant at the 10% level.

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	0.001	0.004
II	*Male subsample*	Mean standardized bias	5.18	10.80
		Pseudo-R2	0.016	0.030
III	*Female subsample*	Mean standardized bias	1.59	2.54
		Pseudo-R2	0.002	0.004
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	0.002	0.005
VI	*Younger cohort (≤ 30 years)*	Mean standardized bias	3.68	5.34
		Pseudo-R2	0.005	0.019
VII	*Older cohort (> 30 years)*	Mean standardized bias	3.41	4.36
		Pseudo-R2	0.007	0.012
VIII	*First wave*	Mean standardized bias	2.01	3.89
		Pseudo-R2	0.002	0.011
IX	*Second wave*	Mean standardized bias	2.48	3.63
		Pseudo-R2	0.005	0.009

Notes: The matching quality indicators are measured after matching. *Low educational level*: this subsample refers to individuals with, at most, lower secondary school attainment levels. *High educational level* refers to individuals with upper secondary or above. *First wave* refers to the first call of the ICS program launched in 2006. *Second wave* refers to the second call was launched in 2007 (see also section 2).

Table 5. Robustness analysis: radius matching

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

Notes: This Table shows the resulting ATT estimates. The 2nd and 3rd columns include the sample sizes for, respectively, the treated and control groups (and for the two different outcomes, employment and income). Standard errors are in parentheses and are based on bootstrapping with 200 replications. We apply a Radius-matching method (caliper of 0.1). *Low educational level*: this subsample refers to individuals with, at most, lower secondary school attainment levels. *High educational level* refers to individuals with upper secondary or above. We performed the equality test comparing the estimated effects of the following groups: male vs female (II vs III), low vs high educational levels (IV vs V), young vs old cohorts (VI vs VII), first vs second wave (VIII vs IX). *First wave* refers to the first call of the ICS program launched in 2006. *Second wave* refers to the second call was launched in 2007 (see also section 2). *Indicates that the difference of the two estimated effects is significant at the 10% level.

Table 6. Probit estimates: informal methods as main job search channel Female subsample

Variables	Full sample		Female subsample	
	(1)	(2)	(3)	(4)
Age	0.004**	0.005**	0.005**	0.006**
	(0.002)	(0.002)	(0.002)	(0.002)
Marital status single	-0.097*	-0.062	-0.106*	-0.090
	(0.052)	(0.055)	(0.058)	(0.062)
Presence of children	-0.065	-0.040	-0.081*	-0.068
	(0.043)	(0.049)	(0.047)	(0.052)
Home ownership	-0.044	-0.038	-0.066	-0.066
	(0.036)	(0.039)	(0.041)	(0.044)
Education dummies				
Upper secondary school	-0.084**	-0.110***	-0.030	-0.044
	(0.036)	(0.039)	(0.042)	(0.045)
Tertiary education	-0.144***	-0.160***	-0.112***	-0.120***
	(0.034)	(0.034)	(0.043)	(0.043)
Professional qualification dummies				
Technical skills	-0.031	0.006	0.021	0.047
	(0.061)	(0.075)	(0.085)	(0.097)
Administrative office skills	-0.063*	-0.053	-0.104***	-0.095**
	(0.038)	(0.042)	(0.040)	(0.042)
Retail and service	0.075	0.050	0.056	0.057
	(0.075)	(0.080)	(0.135)	(0.133)
Other unskilled	0.019	-0.002	0.031	0.011
	(0.044)	(0.047)	(0.052)	(0.054)
Previous contacts with the firm (yes)		0.171***		0.155***
		(0.047)		(0.054)
Received unemployment subsidy	0.038	0.043	0.052	0.044
	(0.034)	(0.037)	(0.040)	(0.043)
Received training	-0.061**	-0.030	-0.044	-0.023
	(0.031)	(0.034)	(0.035)	(0.039)
Sample size	690	578	505	431
Pseudo R-squared	0.107	0.129	0.112	0.132
Log-likelihood	-314.29	-259.51	-221.85	-183.24

***Notes*:** The coefficients represent the marginal effects evaluated at the mean values of the explanatory variables in the sample. The standard errors are reported in parentheses. The dependent variable takes the value of one if the individual has used "personal contacts, friends and relatives" to find a job and zero when other methods are used. ***, **, * indicate that the coefficients are statistically significant, respectively, at 1, 5, and 10 percent levels.