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ADiestRAMIENTO LABORAL, SELECCIÓN Y AUTOCONOCIMIENTO

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RESUMEN

Se seleccionaron aleatoriamente a 1250 hombres y mujeres desempleados para invitarlos, una o dos veces, a participar en un programa de entrenamiento enfocado en desarrollar habilidades para el trabajo, llevado a cabo por una firma multinacional grande como parte de sus esfuerzos de responsabilidad social. Los individuos con el menor costo de oportunidad se autoseleccionaron para atender al programa, lo que implica que la distribución de habilidades de aquellos invitados pero que no se registraron, a partir de los cuales se hizo el segundo grupo de invitaciones, está truncada desde abajo. La primera invitación genera un impacto que disminuye la participación en el mercado laboral, mientras que la segunda invitación aumenta la participación. Se piensa que el programa ayuda a los participantes a conocer su propio tipo (a través de las interacciones con los otros estudiantes), que es relativamente bajo para el primer grupo y relativamente alto para el segundo. Gracias al tratamiento, participantes de la primera invitación perciben que están peor preparados para el mercado laboral de lo que creían originalmente, mientras que los participantes tratados con la segunda invitación se sienten más preparados gracias al programa. Los efectos de socialización y selección pueden ser importantes para los programas de adiestramiento laboral.

Palabras clave: entrenamiento para el trabajo, auto-descubrimiento

Códigos JEL: J24, C21, D83

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ABSTRACT

We randomly assign 1250 unemployed men and women to be invited once or twice to a specific-skills training program offered by a large multinational firm as part of its social responsibility efforts. Individuals with the lowest opportunity cost self-select into attending the program, which implies that the ability distribution of those invited but not registered, from which we draw our second group of invitees, is truncated from below. The impact of the first invitation is to decrease labor market participation, whereas the second invitation increases participation. We suggest that the program helps participants learn their own type (through interactions with other students), which is relatively low on average for the first group and relatively high for the second. First-invite treatments declare themselves to be less prepared for the job market relative to controls whereas second-invites feel better prepared as a result of the program. Socialization and selection effects may be important for job training programs.

Keywords: Job Training, Self-discovery.

JEL Codes: J24, C21, D83

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Job Training, Selection and Self Discovery[†]

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November, 2013
(COMMENTS WELCOME)

Abstract

We randomly assign 1250 unemployed men and women to be invited once or twice to a specific-skills training program offered by a large multinational firm as part of its social responsibility efforts. Individuals with the lowest opportunity cost self-select into attending the program, which implies that the ability distribution of those invited but not registered, from which we draw our second group of invitees, is truncated from below. The impact of the first invitation is to decrease labor market participation, whereas the second invitation increases participation. We suggest that the program helps participants learn their own type (through interactions with other students), which is relatively low on average for the first group and relatively high for the second. First-invite treatments declare themselves to be less prepared for the job market relative to controls whereas second-invites feel better prepared as a result of the program. Socialization and selection effects may be important for job training programs.

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I. Introduction

Job training programs are a widely used and extensively researched form of active labor market policy; hundreds of impact evaluations have been documented in countries around the world (although mostly in OECD), it is probably fair to say that efforts in evaluation of training programs catalyzed the profession's knowledge of experimental methods for policy evaluation in general (Heckman, Lalonde and Smith, 1999). The results of these studies vary across countries but seem to suggest relatively positive employment and earnings effects in developing countries but less so in developed nations (Betcherman et al, 2007). Despite the credibility of the identification strategy of some of these studies, there are still questions about the channels through which the estimated effects operate. This paper uses an experimental evaluation of a job training program in Venezuela to examine one possible channel that could be related to the modest impacts documented in part of the literature. We suggest that training programs are often negatively self-selected, this leads participants to update their priors regarding their place on the distribution of talent and to decrease their expected wage, which in turn can discourage them from participating in the labor market.

This paper is the first experimental evaluation of a job training program in Venezuela, and as far as we know the first to evaluate a privately led and funded Corporate Social Responsibility job training initiative directed towards the less advantaged. 1250 individuals without formal employment were sampled at their homes and their interest in the firm's training program confirmed, then a group was randomly selected to be invited to the training program at one of two locations in the city. Several months later, a fraction of the non-compliers of the first invitation (didn't attend), was again randomly selected to be re-invited to another cohort of the program. This design allows the estimation of impacts for different self-selected groups of compliers. When a group of negatively selected individuals are placed together in a class with a positively selected group, they may adjust their expectations about the labor market and opt out. If the group is positively selected relative to the rest of the class, the opposite effect may occur. We find no effects on employment or income, but find that the first group of invitees reduce their labor market participation, whereas those invited twice increase their participation.

The effects of job training programs may be linked to curricular classroom content, as well as practical learning during internships when applicable, but this paper suggests that interactions with other students can lead to learning about the worker's own type and her relative competitiveness. This is related to a mostly theoretical literature that stresses the individual's uncertainty about her own type and the ways she can learn about it through interactions with other agents such as employers (Ishida, 2006) or potential partners in a marriage market (Maruyama, 2010). This "looking glass self" theory has implications for optimal promotion policies within the firm and for optimal matching in other contexts as well, and this paper is to the best of our knowledge the first to provide empirical evidence that social interactions may be important for the individual's information about her place in the distribution of talent or productivity.

A recent survey of research on the effectiveness of job training programs in Latin America (Ibarrarán and Rosas, 2009) shows that in general, non-experimental evaluations of job training programs produce positive and economically significant impact estimates on employment and earnings, in contrast to some findings in developed countries. Two controlled experimental evaluations, to some extent confirm these findings. First, Card et al (2011) evaluate the program "Juventud y empleo" in Dominican Republic and find a positive but temporary formal/quality employment effect and no effect on earnings, Attanasio, Kugler and Meghir (2011) on the other hand, in their randomized evaluation of Colombia's "Jovenes en acción" find significant positive employment and earnings effects for women and smaller earnings effects for men. There are several differences between those evaluations and the one presented in this paper, first, the targeting of the population invited to the training program in Caracas was somewhat looser than those in Colombia and Dominican Republic: individuals were required to be over 18, with completed 9th grade and being either unemployed or working in the informal sector. Second, although the private firm offered the training, the program did not include an internship component. Third, the program lasts a total of 2 months, compared to 6 months in Colombia (3 in classroom and 3 on the job) and 350 hours in classroom plus 2 month internships in Dominican Republic. Finally, the time between the end of the program and the data collection is quite different; in the Colombia and Dominican Republic studies, approximately 20 months passed after training and before impacts were measured, while in the evaluation described here, only 11

months passed after the first group of trainees finished training and 6 months after the second group.

II. Description of the program and experimental design¹

TPROG is the Corporate Social Responsibility (CSR) platform of a multinational company with longstanding operations in Venezuela and every other country in Latin America. With a focus on adult education, the program has five thematic areas: hospitality, retail, enterprise, bartending and teaching. The program's goal is to train (in a period of 2 months) unemployed or informal workers in skills that enable them to expand their employment opportunities, increase their income and strengthen their civic values. This program is replicated throughout Latin America and the Caribbean, as it is TFIRM's most prominent CSR initiative in the region.

The program's 2010 edition in Venezuela consisted of two courses: Retailing and Bartender. Both courses were completely free and were aimed at people living in Caracas, over 18 years-old, unemployed or in the informal sector, and who at the time of the invitation had completed at least the 3rd year of high school (9th grade of basic education).

Eligible individuals could choose between the two types of training, each offered in different locations: Retailing, offered in "Sabana Grande" a very accessible commercial area in the center of Caracas, and Bartender, located on the southern outskirts of the city on the campus of Simón Bolívar University in the "Sartenejas Valley". The retailing course includes marketing topics, product display in shelves and aisles, customer service and stock administration, and the Bartender course trains participants in the preparation of drinks and spirits, customer service, hospitality, English and Flair practices. Both courses were certified by the Simon Bolivar University (one of the most prestigious universities in the country), for a period of two months, 3 days a week, part time. Both courses included personal growth modules, responsible drinking and job readiness training. Only participants with less than 3 absences could get the course's final certification.

¹ Due to confidentiality, and pending formal authorization, we will not refer to the actual name of the program or the company that sponsors it. Instead, we will refer to TPROG and TFIRM respectively.

Additionally, program attendees enjoyed free transportation to the farther location, refreshments on site and awards for perfect attendance.

First experiment

The first phase consisted in finding eligible candidates for the training program. This was done via regular household surveys based on the polling firm's master sampling framework for the city of Caracas². This process provided the baseline survey as well as the experimental population, which was set at 1250 eligible men and women. 888 individuals were randomly assigned to being invited to the training program, leaving 362 in the control group. The eligibility filters were the following:

- Hometown: Caracas / Metropolitan Area
- Age: over 18 and under 60 years-old
- Education level: at least 9th grade of Basic Education
- Employment status: one of the following options: having odd jobs, unemployed, looking for permanent job, doing housework, participate in state-funded programs
- Declares interest in participating in the training program.

The entire sample was then contacted by phone to inform on their formal invitation status. People who were part of the treatment group were informed about the registration dates and subsequent steps. People who were part of the control group were also contacted by phone and informed that they were part of a second phase program that would begin later and they would be contacted again with information on the new registration dates. In order to ensure the programs' participant quotas were met, there were other selection mechanisms into the program running in parallel to the experiment described here. These mechanisms largely rely on TFIRM's network of retail clients and partners, who select and propose potential trainees, who are generally then invited to participate.

² Initially, the polling firm sampled households via telephone, but the response rate was low (20 calls per eligible respondent), which led them to switch to face to face household surveys, doubling the success rate. 12% of our sample was initially contacted by phone (150 people) while the rest was face to face.

Second experiment

Of the originally invited 888, 772 did not attend the training program, so we randomly selected 623 (80%) of them to be invited a second time. For this second cohort, only the retailing course was available, but this time it was offered in 4 locations throughout the city: “Montalban”, “Caricuao”, “Catia (The Silsa)” and “Sabana Grande”. People who were re-contacted could not choose the venue, but were instead assigned to the one closest to their home.

Follow-up

A year after starting the first course (May 2011), the same market research company tried to contact the original 1250 interviewees for the follow-up survey. Due to administrative considerations, a one month time limit for the follow-up survey was established. The idea was to start tracking the maximum number of participants by phone and proceed with the face-to-face interview (at home) only for those which they could not locate after 3 phone contacts. After two weeks, 411 telephone interviews were obtained, but in the following two, we were able to contact only 189 participants at home. The total follow-up sample contains 600 interviews, 48% of the initial experimental sample. Figure 1 describes the timing of the data collection process as well as the interventions.

[Figure1]

Descriptive statistics

Table 1 shows a number of basic characteristics for the treatment and control groups at baseline, both for the entire sample (1250 obs.) and for the subsample for whom there is follow-up data (600 obs). The most significant difference between treatment and control groups is the fraction female: 57.1% of the treatment group is female whereas this fraction is 66.5% in the control group. Under perfect random assignment, there would have been 61.9% female in both groups, which suggests there may have been imperfections in the implementation of the randomization protocol. This imbalance is associated with a couple other baseline differences between treatment

and control such as the fraction holding occasional jobs and the unemployment rate; labor market participation is higher in the treatment compared to the control group and a lower fraction is dedicated home production. Given these questions regarding the first randomization procedure, we estimate difference in difference models below in order to control for any time invariant unobserved characteristics, as well as present our results separately for women and for men. r

[Table 1]

For the first experiment, of the 888 people invited, 772 (87%) did not attend the registration day despite having expressed an interest and availability to participate in the course. Of the 116 who started the training program (13% take-up), 63 were certified (54% retention). For the second experiment, of the 623 invited twice, 31 were enrolled (5% take-up) and 19 were certified (61% retention). This kind of attrition is common in job-training programs, entrepreneurship, and even micro-financial training programs. It usually represents a major challenge for estimating “treatment on the treated” impacts of the program, so we will initially focus on “intention to treat” effects and then provide IV estimates of a local average treatment effect, a more relevant estimate for policy purposes. Table 2 describes the structure of the 2010 and 2011 datasets across treatment and control groups. Although the re-contact rate was very low, the distribution of the datasets across treatment conditions is very similar, which suggests that the unit non response is orthogonal to the treatment condition.

[Table 2]

III. Program impacts

The program seeks to improve income prospects through employability, which suggests labor market status and income as the key outcomes of interest. We focus on labor market status first and then examine the effects on income. Our baseline model is a simple difference in difference model (in order to control for any remaining unobserved time invariant heterogeneity across treatment groups after randomization), the general specification taking the form:

$$LABORSTATUS_{it} = \beta_0 + \beta_1 INVITED1_{it} + \Gamma X_{it} + \delta_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where $INVITED1_{it} = 1$ if the individual was invited to participate in the program and $t = 2011$ and zero otherwise, δ_i and λ_t are individual and period fixed effects respectively, X_{it} is a vector of individual-level controls, which could be included in the model. Results reported below do not include additional time-varying controls, but doing so does not alter any of the estimated effects (results are readily available from the authors). Since all independent variables are dummy variables, we focus on a linear probability model, results are unchanged if estimated under a Probit or Logit model.

[Tables 3 and 4]

We focus first on the effect of the first invitation (intention to treat) on labor market participation, unemployment and employment status. Table 3 shows that program invitees were 20% less likely to report being unemployed as a consequence of the program, but are not any more likely to have found employment. The entire effect appears to be coming from a decrease in labor market participation. Table 4 reports the same estimates by gender and shows that the average effect is mostly driven by the impact on female labor force participation. As described before, the take-up rate from the first invitation was 13%, which led to a second round of invitations drawn from the non-takers, so these estimated effects include people invited once or twice as part of the treatment group, if the second invitation has any effect, it would be confounded in these estimates. Tables 5 and 6, therefore restrict the treatment group to those invited only once, which includes the 116 takers from the first invitation, plus 150 individuals (of which only 64 were interviewed in the follow-up) that were left as controls in the second invitation round. The control group is the same as in tables 3 and 4 (those never invited). The sign and statistical significance of the effects remains the same, but the size increases to 30% lower probability of being unemployed and 25% lower probability of participating in the labor market (Table 5). Again, the effects are shown to be mainly driven by the impact of the program on women, who report a 36% decline in the probability of unemployment and a 27% decline in labor market participation. Despite the fact that the impacts are more significant for women (economically and statistically), the signs of the coefficients for men are exactly the same as they are for women, which suggests a similar

mechanism may be at work for both groups, but which may require a larger sample to estimate precisely.

[Tables 5 and 6]

Table 7 turns to the impact of the second invitation. In this case the control group includes the 150 individuals who were invited the first time, did not attend, and were not invited a second time. The average impact on unemployment and labor market participation is exactly the opposite from that found for the first invitation. The second invitation increases unemployment by 17% through an increase in labor market participation of 16%, these effects are less precisely estimated, but still remain statistically significant at 90-95% confidence. Despite having achieved a lower take-up than the first invitation, the retention rate was higher. When estimated separately for men and women (Table 8), the direction of the effects for both groups are the same but the size of the coefficients and their statistical significance drops considerably. It still appears to be true that the impacts are larger for women than for men, although here the difference is much smaller than for the first invitation.

[Tables 7 and 8]

In order to get a “treatment on the treated” estimate, we need to find a source of exogenous variation in the decision to participate in the program, conditional on having been invited. Under the assumption that self-selection is related to unobserved gains from participation, using the randomized invitation as an instrument will not resolve the identification problem (Ravallion, 2011). Since we have information on the home address of each interviewee, we are able to calculate the distance from their home to each training center. We construct an instrument for attendance which is the interaction between being invited and the distance between the person’s home and the closest training center, or the center they have been assigned to, or the one they chose to register. This yields the following instrumental variable model:

$$LABORSTATUS_{it} = \alpha_0 + \alpha_1 ATTENDANCE_{it} + BX_{it} + \delta_i + \lambda_t + \varepsilon_{it}$$

where attendance is instrumented with $INSTRUM_{it} = DISTANCE_i * INVITED1_{it}$ and where distance can be measured as the linear distance between the person's home and the training center, the number of subway/metro stations the person needs to ride or the motor vehicle route distance calculated from popular widely available online maps.

[Tables 9 and 10]

Table 9 shows IV estimates of attendance to the first training session on labor market status by gender, while table 10 shows the same but excluding individuals invited a second time from the analysis. The qualitative results are exactly the same: attendance to the training course as predicted by the combination of the randomized invitation and the distance to the training center decreases the probability of labor market participation for women. However, as table 11 shows, the effect of attendance to the second training program due to the second randomized invitation (from a pool of self-selected non compliers to the first invitation) is positive, and much larger for women than for men, although in both cases imprecisely estimated and thus statistically indistinguishable from zero.

[Table 11]

IV. Interpretation

Training programs, despite often being free of charge to participants, do signify a cost to them: the time required and the alternative use they could give it. Individuals with the most valuable uses for that time will be less likely to attend the program, while those with the least options will attend first. It is reasonable to assume that the opportunity cost of an individual's time, as reflected in their decision to participate in the training program or not, is correlated with their potential labor market value. This implies that the takers from the first invitation will be a negatively selected group from the distribution of talent. The same logic applies to the second invitation, although in this case the distribution from which invitees are selected is truncated from below, as illustrated in figure 2.

[Figure 2]

Why would these selected groups drawn from the same distribution of talent, but from different segments of it, exhibit such different responses? We posit that the channel is related to learning about one's own type while interacting in a training environment with other people (who, given the non-experimental selection mechanism, may be a positively selected group). The first group of respondents learn that they are "the worst" in the market and therefore choose not to participate in the labor market after the program, since their expectations of success are corrected downward, while the group that received a second invitation are also negatively selected, but from a truncated distribution, so their updating about themselves relative to the rest, is very positive and are therefore lured into participating in the market more. There is a question in both the baseline and follow up questionnaires about the individual's subjective perception about how ready he or she is to participate in the labor market. Table 12 reports a difference in difference estimate for the first (excluding those invited twice) and second invitations, and shows that individuals invited twice significantly increase their perceived own labor market readiness, according to the two reported measures. Table 13 shows that this observed average effect is driven by changes in women's perceived labor market readiness. The impact of the first invitation on perceived labor market readiness is negative, as predicted by the self-discovery hypothesis, although the coefficients are not statistically significant.

[Tables 12 and 13]

Further suggestion that there is learning about the individual's own type is that the effects can be estimated for groups with different "tenures" in their labor market status in 2010. The impacts are mostly driven by individuals who have little or no labor market experience (results not yet reported).

A simple model of learning about one's own type through interactions in a training program will be sketched along the lines of Maruyama (2010). Applications of learning through the "looking

glass” can be found in Benabou and Tirole (2003) and Ishida (2006). Psychological studies highlighting the role of peer and teacher evaluations on self-perceptions include Cole (1991).

V. Final remarks

Despite an extensive literature evaluating the impact of job training programs, and important recent experimental efforts undergone in Latin America, there is relatively little knowledge about the channels through which such programs exert their effects on individual worker and employer behavior. This paper suggests that one channel that could play a role in these programs is one where individuals learn about their own potential productivity by interacting with others.

One implication is that underlying heterogeneity in ability, which could imply differential effects of training because of better learning, could be magnified by the encouragement of high ability individuals and the discouragement of lower ability trainees.

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Figure 1. Timeline of the data collection and interventions

	2010											2011			
	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May
Baseline	■														
Invitation		■				■									
Enrollment			■	■			■								
Intervention			■	■	■			■	■	■					
Follow up															■

Figure 2. Distribution of talent and self-selection into participation in the training program

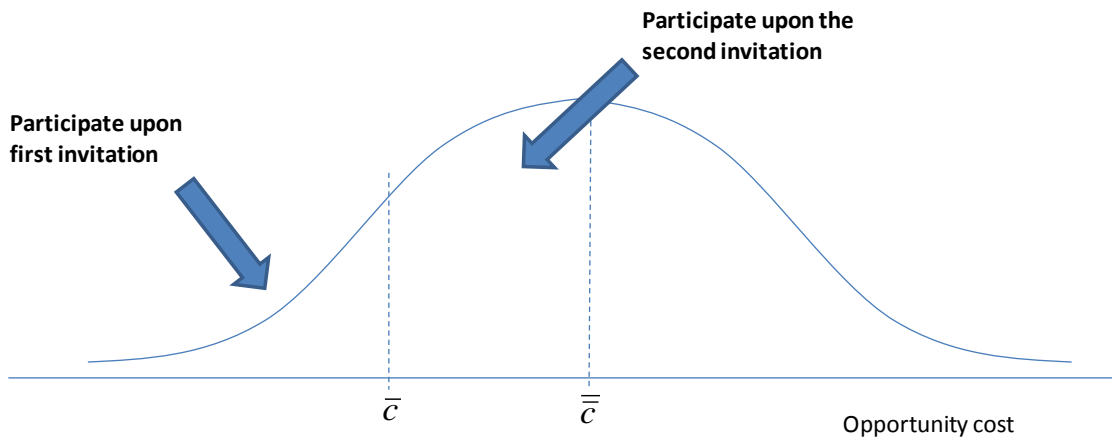


Table 1. Descriptive statistics at baseline (full sample and sample with follow up)

	(Full 2010 sample - 1200 obs)		(Sample with follow up - 600 obs)	
	Treatment	Control	Treatment	Control
Female	57,3%	66,5%	58,7%	68,2%
Employed	27,2%	33,0%	26,4%	34,1%
Occasional workers	19,2%	24,9%	17,7%	25,0%
Informal workers	4,2%	5,3%	4,7%	6,3%
Entrepreneurs	3,8%	2,8%	4,0%	2,8%
Participate in LM	61,9%	56,5%	57,3%	51,1%
Unemployed	34,7%	23,5%	30,9%	17,0%
No participate	38,1%	43,5%	42,7%	48,9%
Students	18,8%	17,2%	18,9%	18,2%
Home production	19,3%	26,3%	23,8%	30,7%
Age 18 - 24 years	47,0%	46,8%	41,0%	42,6%
Age 25 - 34 years	26,5%	27,7%	26,9%	29,0%
Age 35 - 44 years	16,0%	17,7%	18,6%	18,8%
Age 45 - 53 years	8,7%	4,7%	11,1%	6,3%
Age 54 - 60 years	1,8%	3,0%	2,4%	3,4%
High school	80,2%	81,2%	77,8%	79,0%
University	19,8%	18,8%	22,2%	21,0%

Table 2. Distribution of treatment conditions in baseline and follow-up

	2010		2011	
	Obs.	(%)	Obs.	(%)
Treatment 1	888	71.0	424	70.7
Treatment 1 excluding T2	266	21.4	135	22.7
Control 1	362	29.0	176	29.3
Treatment 2	622	49.8	289	48.2
Control 2	150	12.0	64	10.7
Total observations	1250		600	

Note: Treatment 1 refers to the first invitation to participate in the training program, Treatment 1 excluding T2 includes those invited once and registered, plus those invited once and not invited again, Control 1 includes those never invited, Treatment 2 is those who having not responded to the first invitation, were invited a second time, and Control 2 includes those invited once and not invited a second time.

Table 3. Average effect of the first invitation (ITT)

	Participate	Unemployed	Employed
INVITED1	-0.205*** (0.058)	-0.208*** (0.052)	0.003 (0.059)
Y2011	0.358*** (0.049)	0.057 (0.044)	0.301*** (0.050)
Constant	0.555*** (0.019)	0.268*** (0.017)	0.287*** (0.019)
Observations	1,200	1,200	1,200
R-squared	0.12	0.05	0.17
Individuals	600	600	600

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. The treatment group includes everyone invited in the first round, including those invited a second time.

Table 4. Average effect of the first invitation by gender (ITT)

	<i>Female</i>			<i>Male</i>		
	Participate	Unemployed	Employed	Participate	Unemployed	Employed
INVITED1	-0.207*** (0.070)	-0.225*** (0.061)	0.019 (0.070)	-0.099 (0.093)	-0.134 (0.095)	0.034 (0.107)
Y2011	0.500*** (0.058)	0.125** (0.05)	0.375*** (0.058)	0.054 (0.081)	-0.089 (0.082)	0.143 (0.093)
Constant	0.420*** (0.023)	0.206*** (0.020)	0.214*** (0.023)	0.771*** (0.028)	0.368*** (0.029)	0.403*** (0.032)
Observations	738	738	738	462	462	462
R-squared	0.26	0.04	0.28	0.01	0.10	0.06
Individuals	369	369	369	231	231	231

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. The treatment group includes everyone invited in the first round, including those invited a second time.

Table 5. Average effect of the first invitation (ITT), excluding invited twice

	Participate	Unemployed	Employed
INVITED1_EXCL2	-0.2513*** (0.0737)	-0.3071*** (0.0685)	0.0558 (0.0763)
Y2011	0.3543*** (0.0488)	0.0571 (0.0453)	0.2971*** (0.0504)
Constant	0.5723*** (0.0259)	0.2830*** (0.0240)	0.2894*** (0.0268)
Observations	622	622	622
R-squared	0.154	0.076	0.191
Individuals	311	311	311

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. The treatment group includes everyone invited in the first round, except those invited a second time and the control group includes those never invited.

Table 6. Average effect of the first invitation by gender (ITT), excluding invited twice

	<i>Female</i>			<i>Male</i>		
	Participate	Unemployed	Employed	Participate	Unemployed	Employed
INVITED1_EXCL2	-0.2736*** (0.0948)	-0.3622*** (0.0842)	0.0886 (0.0944)	-0.0848 (0.1102)	-0.1763 (0.1180)	0.0915 (0.1287)
Y2011	0.4958*** (0.0582)	0.1261** (0.0517)	0.3697*** (0.0580)	0.0536 (0.0805)	-0.0893 (0.0861)	0.1429 (0.0940)
Constant	0.4398*** (0.0325)	0.2199*** (0.0289)	0.2199*** (0.0324)	0.7833*** (0.0389)	0.3833*** (0.0416)	0.4000*** (0.0454)
Observations	382	382	382	240	240	240
R-squared	0.301	0.089	0.293	0.005	0.092	0.074
Individuals	191	191	191	120	120	120

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. The treatment group includes everyone invited in the first round, except those invited a second time and the control group includes those never invited

Table 7. Average effect of the second invitation (ITT)

	Participate	Unemployed	Employed
INVITED2	0.164*	0.177**	-0.013
	(0.090)	(0.079)	(0.092)
Y2011	0.016	-0.281***	0.297***
	(0.082)	(0.071)	(0.083)
Constant	0.567***	0.292***	0.275***
	(0.025)	(0.021)	(0.025)
Observations	706	706	706
R-squared	0.059	0.067	0.158
Individuals	353	353	353

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. Control group includes only those invited the first time and not the second.

Table 8. Average effect of the second invitation by gender (ITT)

	<i>Female</i>			<i>Male</i>		
	Participate	Unemployed	Employed	Participate	Unemployed	Employed
INVITED2	0.1189	0.1620	-0.1131	0.0888	0.1447	-0.0559
	(0.1265)	(0.1087)	(0.2025)	(0.1208)	(0.1160)	(0.1364)
Y2011	0.2069*	-0.2069**	0.4167**	-0.1429	-0.3429***	0.2000*
	(0.1173)	(0.1008)	(0.1838)	(0.1054)	(0.1011)	(0.1189)
Constant	0.4203***	0.2271***	0.4608***	0.7740***	0.3836***	0.3904***
	(0.0311)	(0.0267)	(0.0572)	(0.0365)	(0.0350)	(0.0412)
Observations	414	414	238	292	292	292
R-squared	0.197	0.026	0.213	0.018	0.141	0.049
Individuals	207	207	170	146	146	146

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. Control group includes only those invited the first time and not the second.

Table 9. IV Estimates of attendance after first invitation, by gender

	Female			Male		
	Participate	Unemployed	Employed	Participate	Unemployed	Employed
ASSIST_INVITED1	-1.240*** (0.446)	-1.149*** (0.378)	-0.091 (0.388)	-0.190 (0.394)	-0.556 (0.411)	0.366 (0.445)
Y2011	0.495*** (0.063)	0.097* (0.053)	0.397*** (0.055)	0.001 (0.063)	-0.121* (0.066)	0.122* (0.072)
Constant	0.423*** (0.027)	0.209*** (0.023)	0.214*** (0.024)	0.771*** (0.029)	0.366*** (0.030)	0.405*** (0.033)
Observations	718	718	718	454	454	454
Individuals	359	359	359	227	227	227

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. Attendance to the training course is instrumented by the interaction of the invitation to the course and a route-measured distance of the person's home to the training center to which he or she was assigned. Treatment group includes those invited once and also those invited twice.

Table 10. IV Estimates of attendance after first invitation, by gender. Excluding those invited twice

	Female			Male		
	Participate	Unemployed	Employed	Participate	Unemployed	Employed
ASSIST_INVITED1	-0.598*** (0.186)	-0.599*** (0.164)	0.001 (0.178)	-0.067 (0.212)	-0.324 (0.232)	0.256 (0.245)
Y2011	0.526*** (0.063)	0.117** (0.055)	0.409*** (0.060)	0.016 (0.075)	-0.118 (0.082)	0.134 (0.087)
Constant	0.441*** (0.034)	0.218*** (0.030)	0.223*** (0.033)	0.788*** (0.039)	0.390*** (0.043)	0.398*** (0.045)
Observations	376	376	376	236	236	236
Individuals	188	188	188	118	118	118

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. Attendance to the training course is instrumented by the interaction of the invitation to the course and a route-measured distance of the person's home to the training center to which he or she was assigned. Treatment group excludes those invited twice.

Table 11. IV estimates of attendance after second invitation, by gender.

	Female			Male		
	Participate	Unemployed	Employed	Participate	Unemployed	Employed
ASSIST_INVITED2	6.736 (10.081)	1.337 (4.183)	5.398 (8.393)	1.271 (4.244)	3.730 (5.120)	-2.459 (5.104)
Y2011	0.030 (0.415)	-0.128 (0.172)	0.158 (0.345)	-0.097 (0.105)	-0.304** (0.127)	0.207 (0.126)
Constant	0.423*** (0.074)	0.234*** (0.031)	0.189*** (0.061)	0.775*** (0.039)	0.380*** (0.047)	0.394*** (0.047)
Observations	402	402	402	284	284	284
Individuals	201	201	201	142	142	142

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual level fixed effects. Attendance to the training course is instrumented by the interaction of the invitation to the course and a route-measured distance of the person's home to the training center to which he or she was assigned.

Table 12. Impact on perceptions on labor market readiness

	High perception	Low perception	Observations / IDs
INVITED1_EXCL2	-0.106 (0.083)	0.034 (0.044)	622 / 311
INVITED 2	0.391*** (0.09)	-0.181*** (0.052)	700 / 350

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual and time fixed effects. “High” perception of labor market readiness is equal to one if respondent says she has “all the necessary skills to enter the labor market” and a “Low” perception equals one if the responds “I have few or none of the skills necessary to enter the labor market”.

Table 13. Impact on perceptions on labor market readiness, by gender

		High perception	Low perception	Observations / IDs
Female	INVITED1_EXCL2	-0.141 (0.108)	0.022 (0.055)	382 / 191
	INVITED 2	0.417*** (0.133)	-0.241*** (0.072)	412 / 206
Male	INVITED1_EXCL2	-0.029 (0.133)	0.040 (0.076)	240 / 120
	INVITED 2	0.358*** (0.125)	-0.123 (0.077)	288 / 144

Note: Statistically significantly different than zero at 99% (***), 95% (**), 90% (*) confidence. Standard errors are in parentheses below each coefficient. All regressions include individual and time fixed effects. “High” perception of labor market readiness is equal to one if respondent says she has “all the necessary skills to enter the labor market” and a “Low” perception equals one if the responds “I have few or none of the skills necessary to enter the labor market”.