Are (Random) Friends Good for Business? Peer Effects in Training and Entrepreneurship Courses[*]

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Abstract

We study how group composition in a training program that focuses on low-skilled adult women in Chile may impact its effectiveness. We set up an experiment within an existing training program that allows us to investigate whether separating the target population in more homogeneous groups is more efficient than running the program using the traditional allocation process of highly heterogeneous participants. We find that the program only had significant impact on self-perceived benefits and not on objective measures of labor market performance. We further find some evidence that targeting the program did not necessarily improve the benefits provided by it. We finally document significant impact of (randomly-generated) group composition, in particular in the number of classmates interested in self-employment versus formal employment.

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1 Introduction

Training programs are one of the most used strategies to overcome the lack of human capital in relatively unskilled workers, including women, both in developed and developing countries. Unfortunately, research has generally shown that these types of programs have very little –if any- impact in terms of improving employment, job quality or earnings (Heckman et al. 1999; Kluve, 2010; Abramovsky et al., 2011)[1]. One potential cause for this result is that training programs traditionally pool participants with different profiles and backgrounds. This paper estimates the impact of a training program for poor women in Chile and tests whether the peer composition of the student groups that receive the training, groups which composition was altered by our experimental design, can significantly change the impact of the program.

We perform our field experiment in the context of a developing upper middle-income country, Chile, where raising labor supply of the participants has been a high priority policy. As a fact, Chile has the second lowest female labor market participation rate among OECD countries, only ahead of Turkey, and is by far the country with the highest gender labor market participation gap in Latin America (Contreras et al., 2008). In the last quarter of 2011, the unemployment rate is 40% (2.6 percentage points) higher for women than for men, and labor force participation also shows a large gender gap. Unemployment and inactivity are highly concentrated in low-income sectors of the population, a group that traditionally has access only to informal or temporary employment. While 58.7% of women in the richer quintile of the population participate of the labor market, only 25.5% do so in the poorer quintile. Furthermore, since over 40% of the head of households are women in the poor population, while 30% are in the non-poor, enhancing female labor participation in the low income segments of the population becomes a relevant policy in order to reduce gender inequality and vulnerability of families where a woman is the main earner. The closing of both the participation and job quality gap for vulnerable women has been argued as one of the main challenges to public policy in Chile (OECD, 2009).

In many other areas, research has shown that focalization matters. In microfinance programs, some have argued that the most effective programs cannot target the "poorest of the poor" (Murdoch, 1999; Rabbani et al., 2006). Banerjee et al. (2007) evaluate the target efficiency of various assistance programs in India, and find that the methods used generally fail to identify eligible households. They also evaluate a new methodology to identify the poorest households eligible to participate in a program designed to assist the "poorest of the poor" and show that targeting and focusing the program in the most-needed population have positive effects on levels of consumption. The evidence in this area points to the conclusion that efficient targeting of policies can improve their impact and effectiveness. One also can argue that a

[1] See also Dar and Tzannatos, 1999; Martin and Grubb, 2001 or Betcherman et al., 2004.

program can benefit from a diverse population, but in order to improve the efficiency, different subgroups should be treated separately. Duflo et al., (2011) evaluate the impact of tracking (i.e. segregate students according to their performance) in Kenya's elementary schools and find that all subgroups improved their learning experience, even initially low-achievers, when grouped with more similar peers.

In this paper we propose a methodology, similar to the one used in the education literature (Duflo et al. 2011), to test both the impact of focalization and the existence of peer effects in a training program. In order to do so, we intervene a nation-wide training program ran by the Chilean Women's National Service (SERNAM). This program (called Female Worker and Head-of-Household, *Mujer Trabajadora y Jefa de Hogar*, thereafter MTJH) works through municipalities across Chile and currently serves approximately 30,000 women nationwide. MTJH serves a diverse population. As such, one of the main concerns of the program authorities was that their services are not properly targeted. They suspected that the program could be more effective if they could focus the intervention on women who are truly interested in seeking employment.

Our methodology consists of two main steps. First, the program authority needs to define the criteria they want to use to improve their program focalization. In this case, SERNAM chose to explore the option of focusing the program on women with high attachment to the labor market. Attachment to the labor market is measured using an estimated propensity to work index. Second, the pool of participants, randomly chosen among the applicants, was also randomly assigned to two different groups. In one group (mixed group), the program was administered as was done previously. The second group was divided in two groups based on the propensity to work index: a high and a low attachment group. All the participants received the services provided by the program. Thus, the design allows us to investigate whether serving only the target population may be more effective than continuing to serve the entire pool of participants. Note that since the design is based on a random allocation across groups, the results are robust to all problems potentially linked to self-selection into the program. Furthermore, as in our setting the selection criterion is continuous, information is also generated on the most appropriate cut-off.

In addition to the information that this scheme offers in order to improve their service delivery and potentially their targeting, this setting also allows us to explore peer effects within the program. Peer effects are notoriously difficult to estimate (see Manski, 1993 for a description of these problems) but our experiment creates credible exogenous variation in the characteristics of one's classmates, as in Duflo and Saez (2003) and Sacerdote (2001) for example. Some evidence of peer effects in the context of entrepreneurship and executives' choice of firm financial policies has been provided for MBA and business school students exploiting their random assignment into sections at the beginning of graduate school (see Shue, 2013, and Lerner and Malmendier, 2013). We contribute to this literature by considering peer effects in a new context that have been studied separately in other settings: labor market and entrepreneurship training for poor adult women in a developing economy.

We can compare labor and satisfaction outcomes of women who were assigned to high and low classes vs. women in mixed groups. Since the program effectiveness in itself was also something that was of interest to the organization and the academic researchers and the program was oversubscribed, we also randomly selected the initial pool of participants, allowing us for a comparison between women who receive the program (in the mixed group or segregated group format) to women who do not receive it.

We conducted this experiment in the first semester of 2011 in three central regions of Chile using a pool of almost 10,000 female applicants. We divided about 6,000 women who received the program into around 4,000 who received the treatment in a tracking (homogenous) group and 2,000 who received it in a mixed group. We collected data at the moment the postulants applied to the program, when the participants completed the class and 2 years after the program started via a phone interview.

Our results suggest that the training program only significantly improved subjective measures of well being without significantly impacting measures of labor supply, types of employment and wages. We also find that receiving the treatment in tracking groups produced similar results overall to receiving it in a mixed group. We also provide suggestive evidence that little adjustment was made by the monitors to modulate the content of the classes to the class composition, thus potentially explaining the lack of a difference between the two treatments. This would indicate that the potential benefits of tracking have little to do with the interaction with peers that are more similar and more to the adjustment of the teaching methods to a given group.

However, we do find evidence that the characteristics of one's peers matter significantly, both for outcomes in the end survey and for perceptions at the end of the class. We document that both the average propensity to work of peers (and its variance) as well as the average preference for entrepreneurship (and its variance) -- an alternative way the program could be segregated-- influenced a number of outcomes. Overall, a large average and variance preference for entrepreneurship appears to increase labor supply, benefits and wages but it does not do so by increasing entrepreneurship per se. This may suggest that in a context where formal paid employment is potentially more lucrative than self-employment, being faced with more aspiring entrepreneurs could make one realize that paid employment may be a more interesting option.

2 Background and experimental design

Background

The Program *Mujer Trabajadora y Jefa de Hogar* (thereafter MTJH) was created in 2007 as a way to improve employability and employment conditions through a comprehensive program providing women with basic skills for job search and/or self-employment. The program aims at increasing the employment rate of the participants, and the quality of the jobs they can achieve. The program MTJF is a large scale program that works through a large number of municipalities across Chile and serves approximately 30,000 women nationwide each year. The program's beneficiaries are women belonging to the first three quintiles of the income distribution, who are not currently fully employed, between 18 and 65 years old, and who are either head of household or are willing to work. For example in its 2012 version the program had 30.689 beneficiaries. 73% of these women were households head, and 45% and 38% of them belonged to the first and second quintile respectively. One third of the beneficiaries never attended high school, while 20% were high school dropouts. Almost 25% of the participants were unemployed, while 38% were salaried workers.

The program has two main components[2]. The first one, and the only one that is common to all women who participate, consists of a multi-session training course aimed at readying women for the job search process or for starting their own small micro business. In this class they receive knowledge on how to prepare a curriculum, a brief introduction to labor regulations and procedures for starting up a business, and training on how to search for, maintain a job position and learn on-the-job. All this is performed during 12 sessions with classes composed of 8-20 women from a single municipality. The second component of the program consists of skills-upgrading formation sessions that differ from locality to locality depending on the demands of the participants and the needs of the local labor market. The participants may choose two different paths, depending on their interest of becoming salaried or independent workers. There are, however, limited slots in these two components. So, in 2012 only 32% of the participants had access to the entrepreneurship component, while 36% had access to training slots for salaried employment.

As far as we know, there has been only one impact evaluation to the program (in 2009)[3]. This impact evaluation was performed using a matching technique and a difference-in-difference estimator[4]. According to this evaluation the program had impact only on soft labor market variables such as: attitude towards employment, participation in social activities and propensity to engage in more training or

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[2] The program also offers preferential access to dental, health and child care services.

[3] In 2012 SERNAM hired an external firm to conduct a qualitative and quantitative process evaluation of the program. This evaluation was aimed at collecting information on the quality of the services delivered and on the characteristic of beneficiaries. The final report also includes descriptive statistics on labor market outcomes of participants.

[4] This evaluation was performed on the 2007 and 2008 cohorts of participants.

accessing to formal education. The program did not have any meaningful impact on employment outcomes.

One of the main conclusions of this evaluation was that the program was not properly focalized on women with strong attachment to the labor market. For example, among the sample of beneficiaries of the program used to perform the evaluation, 30% of them were not in the labor force, while only 65% were household heads. The recommendation of the evaluating team was to design a better methodology to target women who have strong desire to work. After this recommendation, SERNAM made a priority to explore ways to improve the focalization of the program, as a way to improve the program effectiveness, without affecting the delivery of its training services.

Experimental Design

In this paper we propose an empirical strategy that allows us to simultaneously explore the impact of the program as it is and to explore how it could be improved by altering the target population of the organization. We implemented this strategy in January 2011 with the collaboration of SERNAM.

First, we used the fact that the program was oversubscribed and assigned the limited spots to the numerous applicants to the program randomly. The women who were not randomly selected through this process became the counter-factual measure of what would have happened to a woman had she not received access to the program. Because the women who were receiving the program were, on average, identical to the women who were not, the simple comparison of their mean outcomes would generate a valid estimate of the impact of the treatment on an average applicant.

In addition, each woman selected into the program was randomly allocated to two different groups (homogenous and heterogeneous classes) in proportion 2/3 and 1/3 in order to maintain the same average class size. The second treatment corresponded to the usual delivery method of the classes where women are not assigned to a given set of classmates. In the case of the first treatment, all the women in the group were divided according to a "labor participation index" such that all women above the median were placed in a given class and those below, in a separate one. This index was constructed using a labor force survey from Chile (Encuesta Casen), selecting a population of women similar in characteristics to the target population of the organization and estimating their propensity to be employed at the time of the survey, using as explanatory variables, their educational background, age, family composition and health. Using the baseline characteristics of the applicants, the predicted probability of that woman was obtained and this is what we refer to as the "labor participation index".

This evaluation was conducted in three regions of Chile to minimize the transportation costs of the monitoring team as well as the coordination costs with the supervisors who are all at the regional level. All localities that had at least 60 available spots for new participants in January 2011 were selected as to avoid any sense of favoritism from the central authorities and to ensure that the localities had a sufficiently large number of vacancies to offer 3 different classes, as required by our empirical design. All women who received treatment in these localities were selected in our sampling because the randomization is done at the individual level. The size of the control group was driven by the amount of excess demand for the program, which varied across localities.

In order to achieve sufficient statistical power, we conducted simulation exercises before we started the experiment, which suggested that we would need about 1,570 women per group for a minimum detectable effect size of 0.1 standard deviations, assuming a power of 80% and an intra-group correlation of 0.05.

Because of limits imposed by the program, we stratified the randomization by whether the woman was a head of the household and localities. This was done such that in localities where the fraction of applicants that were heads of household was higher than the objective fraction of the program (70%), that fraction was maintained in our selected sample. In localities where the fraction of the applicants was lower than 70%, we simply randomized the allocation of the treatment to all applicants. This is only relevant when we compare receiving or not the program and not its various alternatives as these alternatives were randomly allocated stratifying only by locality.

Spill-over effects between the treatment and the control groups in this context are likely to be limited. First, the population of women receiving the treatment was not large enough to influence local labor markets in a substantial way. Second, many women in the control group could have obtained employment assistance through some other channels. However, this is part of what the reality would be without the existence of this particular governmental program and thus we wanted to make sure that we were capturing this. We instructed, during the pilot phase, the staff in the local agencies not to specifically target another program to the "control" group but to offer them the range of assistance they would usually offer any woman coming through their door asking for help.

Spillover effects between the two treatment groups could occur as the composition of one group may affect the attitude of the staff towards a particular group. To alleviate these concerns as much as possible, we implemented the following rules. First, local authorities managing the program were given three listings but were not being told which of these lists corresponded to each type of group (heterogeneous, homogeneous high index and homogenous low index). The listings only included a number and that number was randomly allocated to different groups in each locality. This was done so that the teachers of the program did not change their attitudes or focus more on one particular group immediately. On the other hand, this allowed for the class dynamic, the interaction with the instructor and the organization of the group to be potentially influenced by its composition, something that we hoped to capture. Secondly, local authorities were allowed to form multiple classes if they wish to do so but they had to do the same number of classes within their three lists. This rule was implemented because we were very concerned that in some cases, the homogenous groups could have schedules that were more concordant and thus that generating a class schedule for these women would be easier than for the heterogeneous group. This would imply that our randomization would change two characteristics of the treatment at the same time: class composition and class size. This rule ensured that the class size would be, on average, the same between the three groups, avoiding some confusion in the interpretation of the results.

3 Data and estimation strategy

Data

We relied as much as possible on existing surveys and data collection processes in place in the organization. The baseline survey was constructed out of the survey that all applicants must file in order to secure a spot within the program "lottery". This ensured that women who were involved in the experiment experienced a process that was as similar as possible to the ones who weren't (or to women who would apply in subsequent years). Similarly, we relied on the information generated by SERNAM's workers directly in many instances, including measures of assistance to the program, auto-evaluation of the class (to which we added a few questions), desertion questionnaire if they decided not to continue with the program, etc. One benefit of using these existing instruments was that we reduced the sense that investigators were observing the program participants and the program employees.

Our data collection process, then, had three key moments along the progress of the program. First, at the time of the application round, each woman filled a questionnaire. This questionnaire was required by SERNAM to ensure that each applicant qualified for the program based on her income, family structure, etc. The information collected through this questionnaire was also used to build the "labor participation index" of each applicant. The application form included many variables such as age, education, family structure, income, work experience and disabilities. Second, once the training seminars are finished, a number of indicators were collected from the existing SERNAM's data collection process. These included the assistance of all participants to the sessions, a desertion questionnaire for women who left the program during the training sessions and an evaluation questionnaire filled by all participants. From these measures, we obtained indicators regarding the attendance of a woman to the classes, the reasons why she decided to abandon the program as well as quantitative and qualitative measures regarding her experience in the class. Finally, 24 months after the beginning of the training sessions, a follow-up telephone survey was applied to both treated and control subjects. In this survey we collected information on labor outcomes and income, aside of questions regarding household demographic information.

The original sample of treated women 6,348 women, evenly divided among our three treatment groups. The control group has 3,476 women. Table A-1 presents some summary statistics of our sample by the different treatment groups. Given the random assignment, tracking and non-tracking groups have very similar characteristics. As should be expected, the high tracking group has on average younger and more educated women, without a partner present, more likely to be employed, and to be salaried workers and with higher incomes.

Attrition in this program could occur at various stages and some of it is actually an outcome of interest. First, applicants' situation could change between the moment they apply for the program and the moment it was offered. This was simply handled by using the "intention to treat" estimate so as to handle imperfect compliance as it should be orthogonal to the version of the program that was offered to the woman. Second, the group composition could affect the capacity of the program organizer to find a suitable schedule for all participants. Then incapacity to participate in the program was desertion caused by the way the program was offered, an outcome of interest that we wanted to measure. Both of these will be measured directly through the desertion questionnaire. Third, attrition in the evaluation questionnaire could also occur because of absenteeism from the class at which the evaluation was submitted.

And finally, we also face attrition at the time of follow-up survey. Because of budget constraints we apply a telephone survey. We faced a high attrition at this stage, as we found approximately 3,800 women out of the original 9,824. We of course, suffer some selective attrition in our final sample. On average our final sample is biased towards older married women, more educated, more likely to be working and with lower income. Our sample also over represents women with disabilities and women who only hold one job. All characteristic that make more likely to be able to find a given women at home or to make contact to her through another adult present in the household at the time of the interview. Table A-2 presents average differences on selected variables between interviewed and non-interviewed women in the total sample and for the different treatment groups, with and without controlling for individual differences in the participation score.

Empirical Strategy

Using the random variation generated by our experiment, the regression equation that needs to be estimated has the following form:

$$Y_{ij} = \alpha + \beta_1 \mathrm{T1}_j + \beta_2 \mathrm{T2}_{ij} + \beta_4 \mathrm{S}_j + \beta_5 \mathrm{X}_{ij} + \varepsilon_{ij} \qquad (1)$$

where Y_{ij} is the outcome variable of individual *i* in comuna *j*. $\mathrm{T1_j}$ and $\mathrm{T2}_{ij}$ are dummy variables indicating each treatment (homogenous/heterogeneous), S_j are dummy variables indicating the stratification cells (the localities or comunas) and X_{ij} is a vector of baseline variables and potential control variables. Robust standard errors are clustered at the locality level *j*.

The effect of each treatment compared to a control or reference category would then be obtained from the coefficients $\beta_1, \beta_2, \beta_3$. We present intent-to-treat estimate for all the regressions since we do not have the actual treatment for all observations. However, we demonstrate that our random assignment did affect the actual treatment in the cases where it can be observed.

We also can estimate the impact of being assigned to the homogenous group versus the heterogeneous group by estimating the above equation but this time only on individuals that were randomly assigned to receive the treatment.

We can study more directly the role of group composition exploiting the random variation generated by our experiment. First, the participants that are selected to be part of the heterogeneous group will have a random set of peers assigned to them through the formation of the group, conditional on the characteristics of all applicants. This allows us to measure directly the impact of the group composition on outcomes. In particular, we can run the following regression:

$$Y_{ij} = \alpha + \beta_1 \bar{x}_{-ij} + \beta_4 \bar{\mathrm{X}}_{-ij} + \beta_5 \mathrm{x}_{ij} + \varepsilon_{ij} \qquad (2)$$

where the indices are defined as before. In this case, the sample will be restricted only to participants who were assigned to being in a heterogeneous group and the independent variable of interest will be the average characteristics of the group, excluding that of participant *i*, controlling for the average characteristics of all selected individuals. While we can look at our measure of labor market attachment and the role differences in the index plays among one's peers, we can also measure what is the role of having peers more interested in entrepreneurship versus formal employment or of having peers involved in a particular industry. So far the literature has provided very limited evidence of any sort of role for peer interactions within this context and this would allow us to evaluate it very directly. In the context of school children, better peers have been found to increase the performance of other children, as documented by Duflo et al. (2011). We also include in some estimation models the variance in the distribution as it is also randomly generated by our empirical methodology.

We can also directly use the variation generated by our experiment to summarize the impact of our "tracking" using the mean and the variance of the ``index'' for each group within a given locality. In this case, the variation does not stem from the random selection of peers for the heterogeneous group but rather the fact that being assigned to the homogeneous versus heterogeneous group directly affects the mean and the variance of the index of one's peers. Formally, in this case, our estimating equation, for the full sample of individuals ever selected to participate in the program, becomes:

$$Y_{ij} = \alpha + \beta_1 \bar{x}_{-ij} + \beta_2 Var(\mathrm{x}_{-ij}) + \beta_4 \mathrm{S}_j + \beta_5 \mathrm{x}_{ij} + \varepsilon_{ij} \qquad (3)$$

Finally, we have one more way of directly measuring the impact of peer composition on the outcomes of participants. In the homogenous groups, there is a sharp discontinuity in the labor market attachment index of the peers for women in the middle of the distribution. Women who are close to the median can be allocated to the "low group" in which case they will have peers who will have much lower indices than themselves or to the "high group" in which case they will have peers with much higher indices than themselves. Thus, we can estimate the impact of being allocated to the "low" or "high" group using a regression discontinuity design. The assumptions required for the validity of this strategy is that nothing else changes discontinuously around the point of separation between the two groups, which holds in our context. Furthermore, since the value of the index that separates the group in two varied by localities, we can control flexibly for the value of the index of each woman. We will estimate the control function using a 3rd order polynomial on each side of the discontinuity, although similar results were obtained for alternative methods.

The results from this exercise may differ from the ones presented before as being assigned to the "low" or "high" group changes two things simultaneously. One is faced with different peers but one is also in a very different relative position compared to the group. We did not inform the teachers of which group corresponded to which characteristics but they may have readjusted their teaching materials and techniques in front of such different groups. This could imply that being the best of the worse or the worse of the best could not generate large differences in the learning experience since, in both cases, one is far from the average characteristics of the peers. This was found in the case of Duflo et al. (2011) but never before examined in the context of adult labor market training. Comparing both sets of measures of peer effects will thus be informative as to whether it is only the average characteristics of peers which influence the outcome of participants or if the difference between one's characteristics and that of one's peers also matter.

4 Results

First stages

While we will focus on reduced form (intent-to-treat) estimates because of limited availability of variables determining the treatment status for the full sample, we first present indication that the variation in our assignment did translate into a real change in the environment faced by the participants. Table 3 reports a variety of these exercises, all demonstrating a fairly high degree of compliance.

First, we look at whether our random assignment into the program actually impacted the access to the program. We can measure actual access using two different variables. We first code anybody who answered our survey at the end of the class as being a participant and anybody who did not fill such a survey as a non-participant. We find that being randomly allocated to receiving the program increases the probability of having filled the survey by 33 percent. This is a lower bound on the level of compliance as many participants did not fill our first round survey. We are currently matching our records to class rosters and there, the compliance rate seems even larger. We also ask, in our end survey, whether the woman had ever participated in the SERNAM program, which imply that this person could have benefited from in the last 2 years. This indicates that our randomization, in some cases, mostly delayed the receipt of the program but did not impede as, 2 years later, it only increased actual participation by 11 percent. However, in both cases, the relationship is strong and the t-test is very high. Class allocation was even more closely followed as being assigned to a heterogeneous group (among those assigned to treatment) raises the probability of being enrolled in such group by 91%.

Finally, we look at whether our random allocation actually influenced the characteristics of the peers one faced in the class. In column (4), we test whether, controlling for the average score of all individuals assigned to treatment, the average characteristics of one's assigned peers is related to the actual characteristics of the group one faced. The results suggest that being assigned to a heterogeneous group where the average score is 0.1 higher than another one raises the average score of one's actual peers by 0.12. If we instead use all groups and rely on locality fixed effects, we find that being randomly assigned to a group with a 10 percent higher predicted probability to work raises the work probability of actual peers by 9.5 percent. Finally, the last column shows that the marginal individual who was assigned to the "high homogeneous group" instead of the "low marginal group" faced peers with a more than 30 percent higher predicted propensity to work.

Furthermore, we verified that in the case where localities organized more than one class of each "type", the segregation that occurred naturally through, for example, the scheduling choice, did not replicate the one we generated in the experiment. Namely, we can see that a heterogeneous group that was broken into 2 different classes saw those classes still being much more heterogeneous than the homogeneous groups. The heterogeneous classes were then less diverse than the original grouping we had generated but still more diverse than the tracking classes.

Effect of training program

We will now focus on the reduced form effects of our randomized design. We first look at the impact of being allocated to the treatment group. Table 4 presents the first comparison between selected and non-selected individuals in terms of their outcomes 2 years later. The first column corresponds to the estimation of the intent-to-treat of participation, the second and third columns correspond to a model where the impact of being allocated to the tracking and mixed group are estimated separately while the last three columns present the coefficients of a model where the impact of being allocated to each level of tracking group or mixed group is estimated are presented.

The results overall, indicate that only "soft" measures appear to have been impacted by the program. We find little evidence of a treatment effect on measures related to the applicants' labor supply, the type of work they perform and their income and wage levels. We do find that being randomly assigned to participate in the program raises your probability of declaring that you are very happy in our survey by 4 percent. We also find that it raises the probability of stating that you have a better personal situation than 2 years ago by 3 percent. Finally, it appears to make you more likely to claim that such programs are useful.

The results in Columns (2) and (3) differentiate this impact by the type of group one was randomly allocated to. They suggest that being in the tracking group made individuals happier than in the control group but that being in the mixed group increases the probability of having improved their personal situation compared to the control. Individuals assigned to mixed group also appear to have a better opinion of training programs in general. The last three columns indicate that the effect of tracking is independent of which group within the tracking component one is allocated to. No other elements appear to differ between tracking and mixed allocations.

Effect of the type of training

We now restrict our sample to only individuals randomly assigned to receive the treatment and contrast whether, among those individuals, being assigned to one group improves one's performance compared to another. In Table 5, we measure the same outcomes as in Table 4 but this time comparing tracking and mixed treatment among individuals assigned to receive the program. The first column allows a simple comparison between the two groups while the subsequent two compare being assigned to a high or low tracking group. Finally, the last two columns allow the impact of tracking to differ linearly according to the individual's predicted propensity to work. The results do not suggest any strong impact of tracking versus non-tracking in this setting. The few marginally significant differences appear to suggest that tracking may be more helpful for low-propensity to work females than for those with higher propensity but the results are very noisy.

We then repeat the same exercise but this time using our first round outcomes, as obtained in the class evaluation of the participants to explore what potential reasons could explain the lack of a difference between tracking and non-tracking in eventual outcomes. These results are presented in Table 6. We continue finding extremely small and noisy differences in outcomes in this setting. The only significant effect we obtain is that tracking appears to raise the family benefits obtained for low propensity to work women. However, we do observe a pattern that can help us understand the potentially limited impact of tracking in this setting. Two percent fewer of the participants rated the level of the class material as being adequate in tracking versus non-tracking, indicating, if anything, very limited changes or, at worst case, inappropriate readjustments to the curriculum depending on the types of students. This also appears to be orthogonal to the student's propensity to work or the level of the tracking one was assigned. Limited impact also is documented on the perception of the participants about the work of the instructor. However, there is a clear pattern indicating that participants in low-score tracking group did find their classmates much more similar to them, which indicates that the difference was obvious to participants and that the lack of adjustments on the part of the monitor might have been more related to their incapacity to adjust the material being taught than by an incapacity to recognize the difference between the groups. In qualitative interviews with the monitors, many did recognize substantial differences between the groups.

Peer effects

We then turn to evaluate if our previous lack of relationship between tracking and non-tracking group is due to the lack of structure imposed on the estimation. To do so, we estimate, for all participants, equation (3) and present the results in the first four columns of Table 7 and 8. In that case, we estimate the impact of the average and the variance in the propensity of one's peers to work, thus summarizing in these two variables the impact of tracking. Because another great source of diversity in the participants is their interest in entrepreneurship versus paid employment, we also include the average and variance of the propensity of peers to want to start a business in the future. Since the allocation within a locality was random, the variation generated across groups within a locality is exogenous. We can thus interpret the results of both sets of variables as causal.

In the case of our second round outcomes, we find small but more significant evidence than previously that facing peers that are more diverse in terms of their propensity to work increases the perceived benefits of training programs and also the probability of being an independent worker and contributing to social security, 2 years after the beginning of the program. In our mid-line survey, we document that being in a group where peers have a wider range of propensity to work does increase finding the class of an appropriate level but decreases the probability of making friends and the overall perception of the work of the instructor. It thus seems to indicate that the potential benefits of being in a heterogeneous (non-tracking) group does not stem from the capacity to establish better relationships with peers nor with the instructor.

The way we segregated our groups may just not have been as optimal as if we had used a different type of variation. Columns (3) and (4) of Tables 7 and 8 explore this by adding as controls the average and variance in peers' entrepreneurship intents. Being paired with peers who want to start their own business in a larger fraction increases significantly the perceived benefits they obtain from the class, their labor supply 2 years later and their log monthly wage. However, it does not appear to do so by increasing the participants' own entrepreneurship as we find no relationship between the group composition and the likelihood of being an independent worker 2 years later. The impacts on 1st round outcomes are less clear in their overall direction. Having more entrepreneurial peers reduce the personal benefits a participant declares having acquired but greatly increases her learning about labor markets. It appears to have made the class much more participative and the instructor more likely to be on time but also have complicated class management. A larger variance in peer's entrepreneurial desires appear to have a similar impact on outcomes as the mean did, suggesting that while more entrepreneurial activity may influence the outcomes of participants, large deviations from a balanced group would have the opposite effect.

The last 4 columns of Tables 7 and 8 look at estimating the same relationship but this time using not the random allocation across groups within a given locality as the source of variation but rather the fact that the non-tracking group could differ in its attributes from the characteristics of the participants in the locality because they are drawn randomly from such pool. In this case, we find a larger role for being randomly paired with peers with higher average propensity to work: individuals in those groups were more likely to report being happy and less likely to be self-employed. The variance in the propensity to work among peers appears to have similar effects when driven by random draws of the non-tracking group as when we were comparing tracking and non-tracking within localities previously. Facing a more diverse pool of colleagues increases one's perception of the usefulness of training programs, the perception of having improved one's probability to find work, decreases the probability of making friends and appears to complicate the perceived quality of the instructor. Having peers who are more entrepreneurial also seems to have the same impact when measured as deviations from the average stemming from the random drawing of the non-tracking group as it had when comparing the different groups within a locality. It increases the probability of being happier 2 years later, appears to increase labor supply (although this is now noisier). However, it also reduces the perceived family benefits received, the probability of paying social security and being self-employed.

Thus, overall, the impact of being in a group with different attributes during the labor training program appears to have more relevant impacts. In particular, variation in the fraction of peers who want to start a business appears relevant and sometimes more important than the variation we purposefully generated with our propensity to work index.

We finally turn to our last measure of peer effects using the discontinuity in the assignment of the individuals located close to the median of the tracking group. These are presented in Table 9 and 10. In the first column, we report the impact of being just above the 50^{th} percentile, which translates into a much higher probability of being assigned to the "high score" tracking group. We find no significant effect except that individuals just above the 50^{th} percentile were less likely to find their colleagues similar and more likely to find that they had more labor market experience than themselves. This simply suggests that for our marginal participant, the group composition generated a shift in the composition of the peers. The next two columns report the interaction of being at the discontinuity with the value of the propensity to work index at that point. That is, it compares being just placed in the high index group in localities where the median had low propensity to work to those where the median had a higher propensity to work. The results suggest interesting differences depending on the cut-off point. Being just assigned to the high index group increases one's happiness and family income but only when in a locality where the median propensity to work is low. In localities where the participants were already very likely to participate, there was no benefit of being assigned to the high versus the low group. This suggests that the difference may be concentrated in the lower proportion of our propensity to work index. In Table 10, a different conclusion arises in terms of the perception of participants with respect to the benefits they receive. Overall, in this table, the impact of being marginally assigned to the high index group appears to be negative for the localities where the median is at a very low level but growing with that value. Thus, it seems that the evaluation and the perception the participants form after they first finish the class may not be yet be influenced by the actual impact the training program will have on their labor market outcomes.

5 Conclusions

This paper uses a field experiment methodology previously employed in the context of school children to test whether focalization could improve the impact of labor market training programs. We find that while the labor market training improves "soft outcomes", it does little to improve labor supply, job quality and wages. Moreover, it finds little benefit of participants' "tracking" in this context. This is surprising given that the scope for targeting the learning experience to participants' characteristics would appear to be even larger than in the case of school children. However, it appears that there was little effort devoted to adjusting the content of the class to the specific composition of the groups.

At the same time, we find evidence that group composition, both in terms of the students' potential for labor market participation and their self reported preference for entrepreneurship, significantly impacted the outcomes of the participants and, more so, for the average student than for the marginal one who was allocated to better peers around the median of the tracking group. Given the political popularity but disappointing impact evaluations of labor market training programs, we see this paper as providing evidence that peer interactions and "role models" could improve the performance of such programs.

We also think that the use of self-employment by low-income women in Chile as a way of acquiring more flexible work schedules bears additional costs, something that participants with many entrepreneurship-inclined classmates might have been more likely to discover. A better understanding of the trade-off between formality and flexibility that low-income women face in developing countries could provide judicious insight into their labor supply decisions. This would also provide researchers and policymakers with important insights into why these women have low labor force participation and, thus, help in policy design.

This methodology has the potential to be used in a number of other contexts where an institution delivering a particular program would like to evaluate the usefulness of targeting while maintaining coverage of all the initial beneficiaries.

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Table 1. Female participation, employment and unemployment rates by income quintiles and age groups

	15-64			15-24		
	Participation	Employment	Unemployment	Participation	Employment	Unemployment
Q 1	38.5%	28.3%	26.4%	33.2%	15.6%	53.0%
Q 2	50.1%	44.6%	11.1%	41.5%	32.2%	22.3%
Q 3	60.2%	54.8%	8.9%	57.1%	46.4%	18.7%
Q 4	68.9%	66.0%	4.2%	69.7%	63.0%	9.7%
Q 5	78.2%	76.3%	2.5%	75.3%	66.1%	12.1%
Total	58.0%	52.6%	9.3%	50.1%	38.8%	22.5%

Source: Own calculations based on Casen 2011.

Table 2. Female participation, employment and unemployment rates by educational attainment and age groups

	15-64			15-24		
	Participation	Employment	Unemp.	Participation	Employment	Unemp.
<8 yrs	33.2%	24.2%	27.1%	33.2%	15.6%	53.0%
8 yrs	43.9%	38.5%	12.3%	41.5%	32.2%	22.3%
9-12 yrs	53.9%	49.0%	9.2%	57.1%	46.4%	18.7%
12 yrs	60.8%	57.8%	5.0%	69.7%	63.0%	9.7%
13 or more	67.9%	65.7%	3.2%	75.3%	66.1%	12.1%
Total	50.9%	45.9%	10.0%	50.1%	38.8%	22.5%

Source: Own calculations based on Casen 2011.

Table 3: First stage

	Observed in 1st survey	Participated in the program (2 years)	In tracking	Average score of peers		
Assigned to treatment	0.339***	0.107***				
	(0.021)	(0.018)				
Assigned to traking			0.924***			
			(0.041)			
Average score of assigned group				1.206***	0.946***	
				(0.106)	(0.279)	
Assigned to “high” group						0.332***
						(0.008)
Sample	Localities with 1st round surveys	2nd round	1st round	1st round in tracking groups	1st round	All assigned in tracking
Fixed effects	Locality/HH head	Locality/HH head	Locality	None	Locality	Locality
R-square	0.172	0.025	0.862	0.789	0.938	0.964
N	8935	3778	2954	966	2911	4313

Table 4: Effect of program participation

	Treatment	Treatment by type		Treatment by type and level		
		Tracking	Mixed	Tracking	Mixed	High Tracking
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Self-perceived returns			
Very happy	0.043*	0.051*	0.027	0.054*	0.027	-0.008
(N=3,759)	[0.017]	[0.019]	[0.022]	[0.023]	[0.022]	[0.034]
Better personal situation than 2 years ago	0.032+	0.027	0.042*	0.034	0.041*	-0.017
(N=3,761)	[0.018]	[0.020]	[0.019]	[0.023]	[0.019]	[0.029]
Better work situation than 2 years ago	0.009	0.005	0.018	0.009	0.018	-0.010
(N=3,764)	[0.017]	[0.018]	[0.020]	[0.021]	[0.020]	[0.028]
Training programs help a lot	0.029	0.019	0.049*	0.028	0.049*	-0.020
(N=3,229)	[0.018]	[0.020]	[0.022]	[0.021]	[0.022]	[0.032]
Training programs help a little	0.028+	0.023	0.037+	0.032	0.037+	-0.021
(N=3,229)	[0.016]	[0.019]	[0.019]	[0.019]	[0.020]	[0.028]
			Panel B: Labor supply measures			
Worked last month	0.007	0.008	0.005	0.005	0.006	0.005
(N=3,778)	[0.017]	[0.018]	[0.023]	[0.021]	[0.023]	[0.024]
Worked last year	0.017	0.020	0.010	0.024	0.010	-0.009
(N=3,778)	[0.014]	[0.014]	[0.018]	[0.018]	[0.018]	[0.020]
Hours worked per week	0.332	0.273	0.448	0.010	0.466	0.604
(N=3,776)	[0.743]	[0.765]	[1.060]	[0.965]	[1.050]	[1.232]
Works full time	-0.007	-0.015	0.009	-0.028	0.010	0.030
(N=3,776)	[0.018]	[0.019]	[0.024]	[0.023]	[0.024]	[0.030]
Numbers of days worked per week	0.061	0.055	0.072	0.014	0.075	0.095
(N=3,777)	[0.095]	[0.093]	[0.132]	[0.109]	[0.131]	[0.127]
Works more than 5 days a week	0.000	0.000	0.000	-0.014	0.001	0.032
(N=3,777)	[0.018]	[0.018]	[0.025]	[0.021]	[0.025]	[0.025]
Does not work on Sunday	0.015	0.021	0.002	0.024	0.002	-0.007
(N=3,777)	[0.018]	[0.017]	[0.026]	[0.023]	[0.026]	[0.027]
Works at least 2 Sundays per month	-0.011	-0.011	-0.011	-0.003	-0.012	-0.019
(N=3,777)	[0.017]	[0.017]	[0.023]	[0.022]	[0.023]	[0.026]
			Panel C: Work characteristics			
Independent worker	0.002	-0.009	0.024	-0.001	0.023	-0.019
(N=3,125)	[0.020]	[0.021]	[0.028]	[0.028]	[0.027]	[0.035]
Salaried worker	0.002	0.008	-0.011	-0.009	-0.010	0.038
(N=3,125)	[0.021]	[0.023]	[0.026]	[0.025]	[0.027]	[0.030]

Contributes to social security	-0.005	-0.016	0.017	-0.013	0.017	-0.005
(N=3,125)	[0.020]	[0.020]	[0.028]	[0.026]	[0.028]	[0.033]
Formal employment	-0.001	-0.002	0.002	-0.018	0.003	0.036
(N=3,125)	[0.015]	[0.016]	[0.019]	[0.019]	[0.019]	[0.022]
Has one job	0.014	0.016	0.011	0.014	0.011	0.005
(N=3,775)	[0.017]	[0.018]	[0.021]	[0.020]	[0.021]	[0.030]
Has more than one job	-0.007	-0.008	-0.004	-0.009	-0.004	0.001
(N=3,775)	[0.014]	[0.015]	[0.017]	[0.016]	[0.017]	[0.021]
			Panel D: Income and wages			
Log monthly wage	0.029	0.051	-0.011	-0.051	-0.004	0.234
(N=3,742)	[0.185]	[0.187]	[0.266]	[0.241]	[0.263]	[0.275]
Log monthly income (family)	-0.067	-0.140	0.074	-0.204	0.079	0.147
(N=3,777)	[0.131]	[0.152]	[0.144]	[0.177]	[0.144]	[0.208]

Table 5: Effect of tracking on 2nd round outcomes

		Tracking by level		Tracking by score	
	Tracking	Tracking	Low tracking	Tracking	Tracking* score
	(1)	(2)	(3)	(4)	(5)
			Panel A: Self-perceived returns		
Very happy	0.020	0.024	-0.006	0.011	0.014
(N=2,491)	[0.022]	[0.035]	[0.047]	[0.062]	[0.094]
Better personal situation than 2 years ago	-0.010	-0.003	-0.013	0.058	-0.109
(N=2,493)	[0.019]	[0.033]	[0.045]	[0.051]	[0.080]
Better work situation than 2 years ago	-0.013	-0.005	-0.016	-0.002	-0.019
(N=2,495)	[0.019]	[0.032]	[0.040]	[0.055]	[0.087]
Training programs help a lot	-0.029	0.017	-0.081+	-0.043	0.024
(N=2,215)	[0.023]	[0.037]	[0.044]	[0.057]	[0.086]
Training programs help a little	-0.012	0.015	-0.049	-0.003	-0.014
(N=2,215)	[0.019]	[0.033]	[0.041]	[0.050]	[0.082]
			Panel B: Labor supply measures		
Worked last month	0.003	0.021	-0.032	-0.019	0.036
(N=2,503)	[0.019]	[0.029]	[0.034]	[0.047]	[0.074]
Worked last year	0.011	0.022	-0.019	0.002	0.015
(N=2,503)	[0.013]	[0.020]	[0.030]	[0.039]	[0.057]
Hours worked per week	-0.194	0.629	-1.486	-1.043	1.374
(N=2,502)	[0.980]	[1.483]	[1.833]	[2.373]	[3.782]
Works full time	-0.025	-0.017	-0.014	-0.029	0.007
(N=2,502)	[0.023]	[0.032]	[0.047]	[0.065]	[0.096]
Numbers of days worked per week	-0.016	0.149	-0.299	-0.358	0.553
(N=2,503)	[0.111]	[0.156]	[0.189]	[0.269]	[0.419]
Works more than 5 days a week	0.001	0.031	-0.054	-0.088	0.145+
(N=2,503)	[0.021]	[0.028]	[0.040]	[0.057]	[0.081]
Does not work on Sunday	0.017	0.024	-0.012	0.023	-0.009
(N=2,502)	[0.023]	[0.031]	[0.037]	[0.057]	[0.084]
Works at least 2 Sundays per month	0.001	-0.024	0.045	0.024	-0.037
(N=2,502)	[0.020]	[0.027]	[0.034]	[0.052]	[0.078]
			Panel C: Work characteristics		
Independent worker	-0.031	-0.036	0.009	0.024	-0.088
(N=2,098)	[0.027]	[0.041]	[0.049]	[0.071]	[0.115]
Salaried worker	0.022	0.046	-0.045	-0.056	0.124
(N=2,098)	[0.025]	[0.039]	[0.049]	[0.065]	[0.101]
Contributes to social security	-0.03	-0.035	0.009	0.021	-0.081
(N=2,098)	[0.027]	[0.040]	[0.048]	[0.070]	[0.112]

Formal employment	0.001	0.019	-0.034	-0.025	0.042
(N=2,098)	[0.016]	[0.026]	[0.033]	[0.040]	[0.063]
Has one job	0.003	0.039	-0.066+	-0.058	0.100
(N=2,501)	[0.020]	[0.030]	[0.039]	[0.052]	[0.082]
Has more than one job	-0.001	-0.019	0.033	0.037	-0.062
(N=2,501)	[0.014]	[0.024]	[0.031]	[0.034]	[0.054]
			Panel D: Income and wages		
Log monthly wage	0.072	0.223	-0.273	-0.063	0.218
(N=2,472)	[0.249]	[0.336]	[0.413]	[0.592]	[0.841]
Log monthly income (family)	-0.196	-0.149	-0.086	-0.056	-0.227
(N=2,503)	[0.157]	[0.188]	[0.261]	[0.374]	[0.472]

Table 6: Effect of tracking on 1st round outcomes

		Tracking by level		Tracking by score	
	Tracking	Tracking	Low tracking	Tracking	Tracking*score
	(1)	(2)	(3)	(4)	(5)
			Panel A: Self-perceived returns		
Received personal benefits	0.006	0.002	0.008	0.003	0.006
(N=3,594)	[0.004]	[0.007]	[0.011]	[0.012]	[0.018]
Received large family benefits	0.002	-0.003	0.010	-0.018	0.032
(N=3,500)	[0.011]	[0.021]	[0.029]	[0.033]	[0.050]
Received some family benefits	-0.004	-0.017	0.023	0.039+	-0.069+
(N=3,500)	[0.008]	[0.014]	[0.017]	[0.023]	[0.035]
Gained labor market knowledge	0.000	-0.004	0.008	-0.021	0.034
(N=3,533)	[0.007]	[0.010]	[0.012]	[0.017]	[0.025]
Greatly improved chances of finding employment	0.011	0.009	0.004	0.013	-0.002
(N=3,535)	[0.013]	[0.019]	[0.029]	[0.039]	[0.059]
			Panel B: Workshop evaluation		
Workshop was at an appropriate level	-0.022*	-0.025+	0.006	-0.021	-0.001
(N=3,479)	[0.009]	[0.014]	[0.020]	[0.029]	[0.041]
Workshop was too easy	0.012	0.011	0.000	0.006	0.009
(N=3,479)	[0.008]	[0.013]	[0.019]	[0.026]	[0.037]
Never considered deserting	-0.017	-0.013	-0.009	-0.029	0.019
(N=3,551)	[0.011]	[0.018]	[0.022]	[0.024]	[0.038]
Proportion of workshops attended	0.006	0.004	0.004	0.007	-0.001
(N=3,298)	[0.004]	[0.006]	[0.009]	[0.010]	[0.016]
Workshop was fun (1-7)	0.015	0.019	-0.007	0.002	0.022
(N=3,578)	[0.019]	[0.031]	[0.046]	[0.063]	[0.093]
Workshop was understandable (1-7)	-0.014	-0.018	0.009	-0.016	0.004
(N=3,559)	[0.020]	[0.030]	[0.038]	[0.061]	[0.091]
Workshop was participative (1-7)	-0.003	-0.015	0.023	0.021	-0.039
(N=3,575)	[0.025]	[0.028]	[0.037]	[0.053]	[0.078]
Workshop was useful (1-7)	-0.009	-0.022	0.024	0.022	-0.049
(N=3,516)	[0.019]	[0.029]	[0.041]	[0.053]	[0.081]
			Panel C: Relationship with peers		
Colleagues were similar	0.002	-0.027	0.052+	0.073+	-0.115+
(N=2,098)	[0.018]	[0.023]	[0.030]	[0.041]	[0.061]
Colleagues were more experienced	-0.004	0.016	-0.037	-0.064+	0.096+
(N=2,098)	[0.016]	[0.021]	[0.026]	[0.035]	[0.052]
Made friends with colleagues	0.022	0.013	0.016	0.057	-0.056
(N=2,098)	[0.018]	[0.026]	[0.030]	[0.034]	[0.050]

Will work with colleagues in the future	-0.011	-0.004	-0.014	-0.023	0.019
(N=2,098)	[0.016]	[0.021]	[0.026]	[0.038]	[0.056]
May work with colleagues in the future	-0.014	-0.022	0.016	0.016	-0.048
(N=2,501)	[0.020]	[0.028]	[0.036]	[0.051]	[0.074]
Colleagues made the workshop more interesting	-0.002	-0.009	0.012	-0.015	0.020
(N=2,501)	[0.007]	[0.011]	[0.015]	[0.017]	[0.027]
			Panel D: Teacher's evaluation		
Attendance score (1-7)	0.011	0.005	0.011	0.029	-0.028
(N=3,583)	[0.013]	[0.017]	[0.032]	[0.037]	[0.050]
Punctuality score (1-7)	0.017	-0.006	0.041	0.054	-0.060
(N=3,578)	[0.013]	[0.019]	[0.029]	[0.033]	[0.050]
Class management score (1-7)	0.003	-0.007	0.020	0.031	-0.045
(N=3,531)	[0.012]	[0.012]	[0.022]	[0.040]	[0.053]
Attitude towards students score (1-7)	0.000	-0.012	0.023	0.032	-0.050
(N=3,575)	[0.008]	[0.009]	[0.015]	[0.026]	[0.034]
Responsibility score (1-7)	0.011	0.001	0.017	0.046	-0.056
(N=3,564)	[0.010]	[0.012]	[0.024]	[0.032]	[0.043]
Motivation score (1-7)	0.000	-0.026+	0.048*	0.043	-0.068+
(N=3,570)	[0.008]	[0.014]	[0.023]	[0.026]	[0.037]
Helped with her experience (1-7)	0.038	0.041	-0.006	0.010	0.044
(N=3,530)	[0.032]	[0.048]	[0.072]	[0.093]	[0.141]
Respect score (1-7)	0.000	-0.003	0.006	0.032	-0.051
(N=3,503)	[0.019]	[0.030]	[0.043]	[0.062]	[0.089]
Commitment score (1-7)	0.010	0.040	-0.054	-0.061	0.115
(N=3,569)	[0.027]	[0.047]	[0.057]	[0.073]	[0.115]

Table 7: Peer effects in 2nd round outcomes

	All participants				Only heterogenous classes			
	Work propensity		Entrepreneurship		Work propensity		Entrepreneurship	
	Average	Variance	Average	Variance	Average	Variance	Average	Variance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Self-perceived returns				
Very happy	0.014	-0.728	0.368+	0.464	0.747+	3.493	0.959*	2.372*
(N=2,491; 812)	[0.102]	[0.615]	[0.196]	[0.657]	[0.419]	[3.077]	[0.451]	[1.185]
Better personal situation than 2 years ago	-0.038	-0.011	0.073	0.075	0.008	-0.809	-0.178	-0.240
(N=2,493; 813)	[0.100]	[0.560]	[0.162]	[0.483]	[0.390]	[1.939]	[0.424]	[0.996]
Better work situation than 2 years ago	-0.031	0.535	0.017	-0.242	-0.037	1.685	-0.276	-0.194
(N=2,495; 813)	[0.096]	[0.570]	[0.165]	[0.456]	[0.525]	[2.325]	[0.570]	[1.509]
Training programs help a lot	0.029	1.006+	0.111	-0.315	0.469	5.633**	0.819	-0.205
(N=2215; 722)	[0.100]	[0.548]	[0.244]	[0.528]	[0.411]	[1.763]	[0.509]	[1.272]
Training programs help a little	0.070	0.894+	0.234	0.033	-0.001	4.628*	0.359	-0.864
(N=2215; 722)	[0.082]	[0.462]	[0.228]	[0.579]	[0.342]	[1.883]	[0.468]	[1.023]
				Panel B: Labor supply measures				
Worked last month	0.048	-0.526	0.321+	0.460	0.151	0.156	0.000	1.171
(N=2,503; 814)	[0.069]	[0.525]	[0.185]	[0.523]	[0.425]	[2.094]	[0.452]	[1.435]
Worked last year	0.008	-0.527	0.148	0.344	-0.054	-0.186	-0.050	0.597
(N=2,503; 814)	[0.063]	[0.357]	[0.153]	[0.433]	[0.343]	[1.760]	[0.348]	[0.910]
Hours worked per week	2.939	-11.313	11.717	10.280	15.568	69.728	11.853	60.945
(N=2,502; 813)	[3.913]	[26.175]	[8.759]	[24.233]	[20.830]	[94.163]	[21.077]	[62.403]
Works full time	0.108	0.332	0.341+	0.522	0.082	-0.018	0.321	1.620
(N=2,502; 813)	[0.110]	[0.614]	[0.184]	[0.471]	[0.478]	[2.399]	[0.582]	[1.562]
Numbers of days worked per week	0.624	-0.979	1.457	2.394	2.328	13.821	2.211	12.231

(N=2,503; 814)	[0.394]	[3.001]	[0.894]	[2.507]	[2.607]	[11.304]	[2.530]	[7.927]
Works more than 5 days a week	0.119	-0.124	0.411**	0.653	0.449	2.845	0.749	2.704*
(N=2,503; 814)	[0.092]	[0.584]	[0.150]	[0.406]	[0.440]	[2.328]	[0.475]	[1.338]
Does not work on Sunday	-0.063	-0.227	0.172	1.131*	0.367	0.161	-0.249	-0.256
(N=2,502; 814)	[0.080]	[0.631]	[0.198]	[0.481]	[0.578]	[2.235]	[0.409]	[0.871]
Works at least 2 Sundays per month	-0.034	-0.327	-0.126	-0.800	-0.269	-0.641	0.420	0.967
(N=2,502; 814)	[0.077]	[0.546]	[0.186]	[0.510]	[0.520]	[2.145]	[0.462]	[1.045]
				Panel C: Work characteristics				
Independent worker	0.043	1.184+	-0.343	-0.993	-0.805+	0.896	-1.089+	-2.489
(N=2,098; 673)	[0.102]	[0.652]	[0.339]	[0.823]	[0.452]	[1.983]	[0.553]	[1.538]
Salaried worker	-0.013	-0.980	0.190	0.795	0.252	-2.806	0.659	1.838
(N=2,098; 673)	[0.088]	[0.724]	[0.288]	[0.759]	[0.454]	[2.402]	[0.467]	[1.373]
Contributes to social security	0.091	1.125+	-0.409	-1.231	-0.401	1.745	-1.223*	-2.667+
(N=2,098; 673)	[0.089]	[0.639]	[0.343]	[0.812]	[0.473]	[2.128]	[0.574]	[1.522]
Formal employment	0.064	-0.060	-0.220	-0.515	-0.292	-1.457	-0.486	-0.960
(N=2,098; 673)	[0.068]	[0.457]	[0.146]	[0.398]	[0.283]	[1.636]	[0.344]	[0.913]
Has one job	0.063	-0.332	0.389*	0.381	0.393	-1.259	0.046	1.080
(N=2,501; 814)	[0.082]	[0.524]	[0.191]	[0.535]	[0.454]	[2.391]	[0.477]	[1.286]
Has more than one job	-0.016	-0.179	-0.053	0.106	-0.242	1.414	-0.047	0.092
(N=2,501; 814)	[0.057]	[0.382]	[0.172]	[0.453]	[0.383]	[1.620]	[0.472]	[1.123]
				Panel D: Income and wages				
Log monthly wage	0.089	-2.138	2.859*	9.651	-4.357	-15.237	-1.182	12.149
(N=2,472; 809)	[0.855]	[7.029]	[1.364]	[5.873]	[5.023]	[28.692]	[5.607]	[16.811]
Log monthly income (family)	0.093	4.521	0.010	4.242	-1.661	-2.390	-0.529	6.623
(N=2,503; 814)	[0.692]	[4.451]	[1.035]	[3.206]	[2.817]	[13.135]	[2.771]	[6.673]

Table 8: Peer effects in 1st round survey

	All participants				Only heterogenous classes			
	Work propensity		Entrepreneurship plans		Work propensity		Entrepreneurship plans	
	Average	Variance	Average	Variance	Average	Variance	Average	Variance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Self-perceived returns				
Received personal benefits	-0.024	-0.097	-0.152***	-0.379**	0.178	0.438	-0.169	-0.131
(N=3,594; 1,552)	[0.020]	[0.118]	[0.041]	[0.137]	[0.132]	[0.575]	[0.134]	[0.416]
Received large family benefits	-0.018	0.074	-0.047	-0.107	-0.112	0.128	0.396	0.679
(N=3,500; 1,508)	[0.053]	[0.350]	[0.106]	[0.284]	[0.308]	[1.383]	[0.384]	[1.165]
Received some family benefits	-0.057+	-0.017	-0.029	-0.217	-0.069	-1.403	-0.374+	-0.824
(N=3,500; 1,508)	[0.030]	[0.198]	[0.095]	[0.233]	[0.179]	[0.919]	[0.222]	[0.662]
Gained labor market knowledge	-0.043+	0.045	0.222**	0.478*	0.169	1.297	-0.006	-0.048
(N=3,533; 1,525)	[0.025]	[0.170]	[0.077]	[0.225]	[0.149]	[0.915]	[0.166]	[0.604]
Greatly improved chances of employment	-0.044	-0.504	-0.196	-0.744*	0.545	4.417+	0.360	0.385
(N=3,535; 1,537)	[0.079]	[0.431]	[0.122]	[0.293]	[0.436]	[2.223]	[0.465]	[1.357]
				Panel B: Workshop evaluation				
Workshop was at an appropriate level	0.022	0.427+	0.017	0.167	0.201	-0.560	-0.028	-0.357
(N=3,479; 1,514)	[0.043]	[0.220]	[0.162]	[0.406]	[0.201]	[0.898]	[0.244]	[0.618]
Workshop was too easy	-0.015	-0.197	-0.104	-0.315	-0.170	0.379	-0.070	0.071
(N=3,479; 1,514)	[0.043]	[0.194]	[0.156]	[0.386]	[0.179]	[0.833]	[0.218]	[0.553]
Never considered deserting	0.000	0.215	-0.170	0.291	0.066	1.187	0.030	1.516
(N=3,551; 1,531)	[0.056]	[0.268]	[0.164]	[0.444]	[0.274]	[1.274]	[0.416]	[1.268]
Proportion of workshops attended	0.016	-0.102	-0.118	-0.136	-0.124	-1.185	-0.624**	-1.290*
(N=3,298; 1,392)	[0.021]	[0.138]	[0.083]	[0.208]	[0.205]	[0.971]	[0.206]	[0.515]
Workshop was fun (1-7)	0.029	-0.022	0.092	-0.034	-0.144	-0.292	0.294	1.713

(N=3,578; 1,548)	[0.097]	[0.575]	[0.188]	[0.567]	[0.599]	[2.622]	[0.564]	[2.119]
Workshop was understandable (1-7)	0.017	0.156	0.084	0.243	-0.312	0.685	0.287	1.770
(N=3,559; 1,542)	[0.105]	[0.589]	[0.158]	[0.499]	[0.485]	[2.036]	[0.468]	[1.688]
Workshop was participative (1-7)	-0.012	-0.326	0.534**	0.962+	-0.088	-2.324	-0.229	-0.900
(N=3,575; 1,548)	[0.098]	[0.629]	[0.180]	[0.508]	[0.423]	[2.812]	[0.524]	[1.441]
Workshop was useful (1-7)	-0.046	-0.043	0.028	-0.082	0.476	2.562	0.706	1.528
(N=3,516; 1,542)	[0.092]	[0.505]	[0.198]	[0.600]	[0.590]	[2.086]	[0.618]	[2.515]
				Panel C: Relationship with peers				
Colleagues were similar	-0.146*	-0.386	0.171	0.125	0.702	-0.993	-0.102	-0.777
(N=2,098; 1,509)	[0.071]	[0.466]	[0.220]	[0.601]	[0.498]	[2.146]	[0.506]	[1.305]
Colleagues were more experienced	0.132*	0.371	-0.185	-0.030	-0.321	0.415	-0.083	0.252
(N=2,098; 1,509)	[0.059]	[0.417]	[0.161]	[0.454]	[0.430]	[1.915]	[0.443]	[1.077]
Made friends with colleagues	-0.103	-0.937+	0.030	0.448	-0.543	-3.496+	-0.360	-0.345
(N=2,098; 1,520)	[0.075]	[0.481]	[0.160]	[0.441]	[0.389]	[1.825]	[0.486]	[1.321]
Will work with colleagues in the future	-0.011	0.134	-0.019	-0.153	-0.004	-2.604	0.130	0.284
(N=2,098; 1,506)	[0.073]	[0.401]	[0.158]	[0.432]	[0.291]	[1.968]	[0.434]	[0.868]
May work with colleagues in the future	0.043	0.014	0.280	0.960	0.071	-0.351	-0.660	-0.433
(N=2,501; 1,506)	[0.075]	[0.591]	[0.366]	[0.921]	[0.376]	[2.175]	[0.648]	[1.487]
Colleagues made the class more interesting	-0.004	0.091	-0.082	-0.162	0.044	0.079	-0.071	0.119
(N=2,501; 1,520)	[0.032]	[0.188]	[0.053]	[0.150]	[0.134]	[0.792]	[0.230]	[0.549]
				Panel D: Teacher's evaluation				
Attendance score (1-7)	0.047	-0.443	-0.095	-0.360	0.001	-1.224	-0.261	0.170
(N=3,583; 1548)	[0.081]	[0.367]	[0.208]	[0.484]	[0.351]	[1.448]	[0.712]	[1.905]
Punctuality score (1-7)	-0.044	-0.702*	0.292*	0.204	-0.150	-0.704	0.113	0.806
(N=3,578; 1550)	[0.060]	[0.300]	[0.146]	[0.427]	[0.392]	[1.726]	[0.704]	[1.857]
Class management score (1-7)	-0.022	-0.146	-0.222+	-0.612+	0.179	1.679	0.596	1.761
(N=3,531; 1,544)	[0.051]	[0.321]	[0.123]	[0.315]	[0.506]	[1.725]	[0.489]	[2.041]

Attitude towards students score (1-7)	-0.039	0.038	-0.192	-0.536	0.072	0.790	-0.188	-0.362
(N=3,575; 1,548)	[0.034]	[0.234]	[0.143]	[0.369]	[0.269]	[1.295]	[0.302]	[0.831]
Responsibility score (1-7)	-0.027	-0.491*	-0.208	-0.604	0.134	-0.877	-0.202	0.451
(N=3,564; 1,545)	[0.046]	[0.212]	[0.185]	[0.422]	[0.297]	[1.119]	[0.361]	[1.288]
Motivation score (1-7)	-0.044	-0.157	0.033	-0.217	0.114	-0.203	0.427	2.049
(N=3,570; 1,546)	[0.043]	[0.251]	[0.172]	[0.375]	[0.361]	[1.512]	[0.337]	[1.304]
Helped with her experience (1-7)	-0.111	-1.262	-0.173	-1.049	-0.969	-7.867+	-1.062	-3.986
(N=3,530; 1,525)	[0.156]	[0.810]	[0.306]	[0.955]	[0.951]	[4.404]	[1.135]	[2.824]
Respect score (1-7)	0.018	-0.025	-0.261+	-1.061+	-0.129	-0.619	-0.449	-3.296**
(N=3,503; 1,524)	[0.105]	[0.470]	[0.152]	[0.633]	[0.406]	[2.246]	[0.481]	[1.242]
Commitment score (1-7)	0.071	-1.118	0.029	-0.115	-0.327	-4.225	-0.505	-2.295
(N=3,569; 1,547)	[0.117]	[0.689]	[0.349]	[0.938]	[0.993]	[5.232]	[1.092]	[2.747]

Table 8: Peer effects for marginal individual in 2nd round outcomes

	RD by cut-off		
	Above 50th percentile	Above 50th percentile	Above 50th percentile*Propensity
	(1)	(2)	(3)
		Panel A: Self-perceived returns	
Very happy	0.029	0.325+	-0.436+
(N=2,491)	[0.080]	[0.181]	[0.249]
Better personal situation than 2 years ago	-0.031	-0.089	0.085
(N=2,493)	[0.063]	[0.196]	[0.266]
Better work situation than 2 years ago	-0.076	0.012	-0.13
(N=2,495)	[0.072]	[0.175]	[0.231]
Training programs help a lot	-0.083	0.029	-0.165
(N=2,215)	[0.071]	[0.196]	[0.266]
Training programs help a little	-0.07	0.031	-0.149
(N=2,215)	[0.063]	[0.173]	[0.250]
		Panel B: Labor supply measures	
Worked last month	0.046	0.097	-0.075
(N=2,503)	[0.057]	[0.160]	[0.231]
Worked last year	0.015	0.06	-0.067
(N=2,503)	[0.045]	[0.116]	[0.160]
Hours worked per week	0.615	-5.16	8.51
(N=2,502)	[2.604]	[8.942]	[12.558]
Works full time	0.019	-0.073	0.136
(N=2,502)	[0.053]	[0.171]	[0.245]
Numbers of days worked per week	0.095	0.032	0.093
(N=2,503)	[0.316]	[1.009]	[1.409]
Works more than 5 days a week	-0.02	-0.055	0.051
(N=2,503)	[0.063]	[0.190]	[0.261]
Does not work on Sunday	-0.033	-0.012	-0.031
(N=2,502)	[0.063]	[0.196]	[0.263]
Works at least 2 Sundays per month	-0.005	-0.132	0.186
(N=2,502)	[0.062]	[0.187]	[0.252]
		Panel C: Work characteristics	
Independent worker	0.032	0.232	-0.295
(N=2,098)	[0.070]	[0.237]	[0.341]
Salaried worker	0.015	-0.18	0.288
(N=2,098)	[0.068]	[0.194]	[0.263]
Contributes to social security	0.037	0.179	-0.208

(N=2,098)	[0.066]	[0.236]	[0.329]
Formal employment	0.059	0.01	0.072
(N=2,098)	[0.053]	[0.128]	[0.173]
Has one job	0.049	0.13	-0.12
(N=2,501)	[0.065]	[0.171]	[0.243]
Has more than one job	-0.004	-0.034	0.044
(N=2,501)	[0.046]	[0.118]	[0.168]
		Panel D: Income and wages	
Log monthly wage	-0.234	2.957	-4.717
(N=2,472)	[0.639]	[2.437]	[3.251]
Log monthly income (family)	0.429	2.342+	-2.82
(N=2,503)	[0.433]	[1.311]	[1.761]

Table 11: Peer effect for marginal individual in 1st round survey

	RD by cut-off		
	Above 50th percentile	Above 50th percentile	Above 50th percentile*Propensity
	(1)	(2)	(3)
		Panel A: Self-perceived returns	
Received personal benefits	-0.025	-0.098	0.108
(N=2,491)	[0.020]	[0.069]	[0.088]
Received large family benefits	-0.030	-0.143	0.166
(N=2,493)	[0.047]	[0.149]	[0.202]
Received some family benefits	-0.017	0.065	-0.121+
(N=2,495)	[0.025]	[0.052]	[0.064]
Gained labor market knowledge	-0.031	0.030	-0.089
(N=2,215)	[0.022]	[0.075]	[0.102]
Greatly improved chances of employment	0.039	-0.260*	0.442**
(N=2,215)	[0.052]	[0.108]	[0.164]
		Panel B: Workshop evaluation	
Workshop was at an appropriate level	-0.029	-0.076	0.070
(N=2,503)	[0.039]	[0.081]	[0.114]
Workshop was too easy	0.034	0.079	-0.066
(N=2,503)	[0.037]	[0.077]	[0.108]
Never considered deserting	0.018	-0.243	0.382+
(N=2,502)	[0.041]	[0.170]	[0.229]
Proportion of workshops attended	0.009	-0.051	0.086
(N=2,502)	[0.015]	[0.034]	[0.052]
Workshop was fun (1-7)	0.024	0.037	-0.019
(N=2,503)	[0.067]	[0.205]	[0.270]
Workshop was understandable (1-7)	0.025	-0.517**	0.799**
(N=2,503)	[0.062]	[0.187]	[0.280]
Workshop was participative (1-7)	0.040	-0.332+	0.547*
(N=2,502)	[0.069]	[0.199]	[0.248]
Workshop was useful (1-7)	-0.016	-0.089	0.108
(N=2,502)	[0.059]	[0.178]	[0.247]
		Panel C: Relationship with peers	
Colleagues were similar	-0.105+	-0.301*	0.289
(N=2,098)	[0.056]	[0.142]	[0.215]
Colleagues were more experienced	0.094+	0.221+	-0.188
(N=2,098)	[0.049]	[0.129]	[0.184]
Made friends with colleagues	0.044	-0.059	0.153
(N=2,098)	[0.053]	[0.125]	[0.160]

Will work with colleagues in the future	0.032	0.037	-0.007
(N=2,098)	[0.047]	[0.125]	[0.159]
May work with colleagues in the future	-0.033	-0.169	0.201
(N=2,501)	[0.061]	[0.136]	[0.191]
Colleagues made the class more interesting	0.002	-0.055	0.085
(N=2,501)	[0.031]	[0.103]	[0.137]
		Panel D: Teacher's evaluation	
Attendance score (1-7)	0.013	-0.107	0.175
	[0.059]	[0.252]	[0.338]
Punctuality score (1-7)	0.039	-0.218	0.378
	[0.060]	[0.257]	[0.360]
Class management score (1-7)	-0.003	-0.096	0.137
	[0.034]	[0.073]	[0.099]
Interaction score (1-7)	0.016	0.053	-0.053
	[0.026]	[0.042]	[0.065]
Responsibility score (1-7)	0.033	-0.203+	0.346+
	[0.044]	[0.115]	[0.204]
Motivation score (1-7)	0.012	0.008	0.005
	[0.033]	[0.126]	[0.172]
Helped with her experience (1-7)	-0.003	-0.499*	0.730+
(N=2,472)	[0.098]	[0.249]	[0.366]
Respect score (1-7)	0.083	0.062	0.031
	[0.063]	[0.165]	[0.228]
Commitment score (1-7)	0.029	-0.526**	0.815**
(N=2,503)	[0.083]	[0.193]	[0.266]

Table A-1: Characteristics of samples by treatment group. Baseline, ex-ante balance.

	Raw averages				**Controlling for own score**					**Without control**
	Control	Mixed	Tracking-high	Tracking-low	Treated-Controls	Mixed-Controls	Tracking-Controls	Tracking-Mixed	High-Low Tracking	High-Low Tracking
	(1)	**(2)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
Age	40.20	40.25	37.89	41.47	-0.18	0.169	-0.358	-0.228	0.54	-3.491***
					(0.273)	(0.332)	(0.276)	(0.286)	(0.674)	(0.466)
Married	0.34	0.29	0.17	0.37	-0.015	-0.013	-0.016	-0.009	0.03	-0.195***
					(0.010)	(0.012)	(0.011)	(0.010)	(0.024)	(0.017)
With Partner, not married	0.08	0.08	0.06	0.11	0.014+	0.009	0.017*	0.007	-0.017	-0.051***
					(0.007)	(0.008)	(0.008)	(0.007)	(0.017)	(0.010)
Single	0.32	0.33	0.44	0.27	0.002	-0.008	0.007	0.014	0.044	0.167***
					(0.011)	(0.014)	(0.013)	(0.014)	(0.031)	(0.016)
Divorced	0.22	0.26	0.29	0.21	-0.003	0.014	-0.011	-0.018	-0.053*	0.079***
					(0.010)	(0.011)	(0.011)	(0.012)	(0.025)	(0.013)
Widow	0.03	0.03	0.04	0.04	0.002	-0.002	0.003	0.007	-0.004	0
					(0.003)	(0.004)	(0.004)	(0.006)	(0.010)	(0.007)
Primary education	0.29	0.29	0.22	0.33	0.000	0.008	-0.004	-0.004	0.033	-0.105***
					(0.010)	(0.012)	(0.011)	(0.011)	(0.025)	(0.015)
Secondary education	0.61	0.60	0.69	0.56	0.003	-0.009	0.009	0.009	-0.061*	0.123***
					(0.011)	(0.014)	(0.012)	(0.013)	(0.030)	(0.017)
Tertiary education	0.10	0.10	0.09	0.10	-0.002	0.002	-0.004	-0.005	0.028	-0.015
					(0.005)	(0.008)	(0.006)	(0.009)	(0.017)	(0.010)
With disability	0.49	0.41	0.18	0.60	-0.002	-0.005	0	0.02	-0.097+	-0.425***
					(0.009)	(0.011)	(0.009)	(0.012)	(0.058)	(0.030)
Working	0.57	0.60	0.71	0.57	-0.007	-0.016	-0.002	0.021+	0.078**	0.144***
					(0.015)	(0.016)	(0.016)	(0.011)	(0.027)	(0.019)

Unemployed	0.34	0.32	0.24	0.33	0.000	0.009	-0.005	-0.020+	-0.079**	-0.097***
					(0.013)	(0.015)	(0.014)	(0.011)	(0.026)	(0.017)
Salaried worker	0.47	0.52	0.53	0.49	0.021	0.030*	0.017	-0.006	-0.017	0.046**
					(0.013)	(0.014)	(0.014)	(0.011)	(0.026)	(0.015)
Formal worker	0.37	0.39	0.41	0.35	0.008	0.015	0.004	-0.008	-0.007	0.053**
					(0.011)	(0.013)	(0.012)	(0.011)	(0.027)	(0.018)
Income up to U$200	0.47	0.43	0.38	0.46	-0.012	-0.009	-0.014	-0.001	0.022	-0.070***
					(0.013)	(0.016)	(0.015)	(0.015)	(0.028)	(0.019)
Income U$200-U$600	0.42	0.48	0.54	0.44	0.022+	0.028+	0.018	-0.005	-0.021	0.098***
					(0.013)	(0.016)	(0.014)	(0.014)	(0.027)	(0.016)
Income U$600-U$1000	0.01	0.01	0.02	0.01	0.000	0.000	0.001	0.001	-0.008+	0.006
					(0.003)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)
Income more than U$1000	0.00	0.00	0.00	0.00	-0.002	-0.001	-0.003+	-0.001	0	-0.002+
					(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
One job	0.48	0.49	0.51	0.49	0.009	0.003	0.012	0.013	-0.009	0.014
					(0.014)	(0.016)	(0.015)	(0.013)	(0.027)	(0.016)
Two Jobs	0.24	0.26	0.25	0.22	-0.011	0.007	-0.020+	-0.026*	0.019	0.031*
					(0.009)	(0.011)	(0.010)	(0.011)	(0.022)	(0.013)
Three jobs	0.05	0.06	0.08	0.07	0.009+	-0.001	0.014*	0.015*	0.004	0.009
					(0.005)	(0.007)	(0.006)	(0.007)	(0.015)	(0.007)
Sample	3,476	2,068	2,117	2,163						

Table A-2: Ex-post balance. Attrition at the endline survey.

	Found	Found*treat	Found* Tracking	Found	Found*treat	Found* Tracking
	Without controlling for ownscore			Controlling for ownscore		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Age	2.153***	-0.513	-0.57	1.963***	-0.09	-0.632
	(0.224)	(0.372)	(0.679)	(0.190)	(0.342)	0.066
Married	0.024*	-0.031	-0.011	0.022*	-0.026	-0.025
	(0.011)	(0.020)	(0.027)	(0.011)	(0.020)	0.127
With Partner, not married	-0.018**	-0.002	-0.015	-0.017**	-0.004	-0.014
	(0.006)	(0.013)	(0.014)	(0.006)	(0.013)	0.032
Single	-0.029**	0.015	0.021	-0.027**	0.009	-0.026
	(0.009)	(0.022)	(0.026)	(0.009)	(0.021)	0.043
Divorced	0.023*	0.015	0.002	0.023*	0.015	-0.022
	(0.011)	(0.019)	(0.023)	(0.011)	(0.019)	0.060
Widow	0	0.004	0.004	-0.001	0.005	-0.01
	(0.003)	(0.006)	(0.010)	(0.003)	(0.006)	0.006
Primary education	-0.027***	-0.033+	-0.053+	-0.032***	-0.021	-0.026
	(0.008)	(0.019)	(0.027)	(0.007)	(0.017)	0.047
Secondary education	0.015	0.058*	0.038	0.021*	0.044*	-0.024
	(0.010)	(0.022)	(0.026)	(0.009)	(0.020)	0.055
Tertiary education	0.011+	-0.026*	0.014	0.010+	-0.024+	-0.015
	(0.006)	(0.013)	(0.015)	(0.006)	(0.013)	0.021
With disability	0.034**	-0.01	-0.018	0.018**	0.025+	-0.021
	(0.011)	(0.023)	(0.029)	(0.006)	(0.014)	0.365
Working	0.049***	-0.035+	0.002	0.049***	-0.034+	-0.025
	(0.009)	(0.020)	(0.025)	(0.009)	(0.020)	0.128
Unemployed	-0.054***	0.033+	0.031	-0.053***	0.031	-0.026
	(0.008)	(0.020)	(0.026)	(0.008)	(0.019)	0.069
Salaried worker	-0.030*	0.033*	0.003	-0.027*	0.027+	-0.021
	(0.012)	(0.016)	(0.022)	(0.012)	(0.016)	0.094
Formal worker	-0.013	0.012	-0.048+	-0.011	0.008	-0.027
	(0.011)	(0.021)	(0.027)	(0.011)	(0.021)	0.050
Income up to U$200	0.039***	-0.024	0.009	0.039***	-0.022	-0.026
	(0.010)	(0.017)	(0.026)	(0.010)	(0.017)	0.067
Income U$200-U$600	-0.028*	0.026	-0.001	-0.026*	0.022	-0.027
	(0.012)	(0.018)	(0.026)	(0.011)	(0.018)	0.089
Income U$600-U$1000	-0.002	0.000	0.005	-0.002	0.000	-0.007
	(0.002)	(0.004)	(0.007)	(0.002)	(0.004)	0.013

Income more than U$1000	0.002+	-0.001	0	0.002+	-0.001	-0.003
	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)	0.002
One job	0.036**	0.002	-0.056+	0.036**	0.002	-0.029
	(0.011)	(0.022)	(0.029)	(0.011)	(0.022)	0.088
Two Jobs	-0.014	0.002	0.046+	-0.013	0	-0.026
	(0.010)	(0.020)	(0.026)	(0.010)	(0.019)	0.033
Three jobs	-0.011*	0.004	-0.008	-0.011*	0.004	-0.01
	(0.005)	(0.011)	(0.010)	(0.005)	(0.011)	0.033