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The Effects of Micro-entrepreneurship Programs on Labor Market Performance: Experimental Evidence from Chile[†]

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We investigate the impact of a program providing asset transfers and business training to low income individuals in Chile, and asked whether a larger asset transfer would magnify the program's impact. We randomly assigned participation in a large scale, publicly run micro-entrepreneurship program and evaluated its effects over 45 months. The program improved business practices, employment, and labor income. In the short run, self-employment increased by 14.8/25.2 percentage points for a small/large asset transfer. In the long run, individuals assigned to a smaller transfer were 9 percentage points more likely to become wage workers, whereas those assigned to larger transfers tended to remain self-employed. (*JEL* J16, J23, L25, L26, L53, O14, R23)

Income generation strategies for the poor are one of the most crucial issues in development economics. Micro-entrepreneurship has long been considered a plausible strategy to boost the income of vulnerable households as self-employment is a key source of income in developing countries (Cho and Honorati 2014, Blattman and Ralston 2015). Programs combining training and asset transfers have increasingly received attention in the literature as a means to alleviate potential lack of skills and liquidity constraints that could hinder self-employment as an occupational choice or limit micro-business growth. Banerjee et al. (2015); Blattman, Fiala, and Martínez (2014); and Blattman et al. (2016) evaluate one such program, finding positive effects on earnings three or four years after implementation. Despite these promising results, the literature has focused mostly on low income countries, where low productivity agriculture as an occupation is the norm. It is less clear that these types of programs would have similar results in countries with more developed labor markets. As Bauchet, Morduch, and Ravi (2015) argue, a tight, unskilled labor market

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could be important in determining whether a program promoting self-employment would be successful.

We evaluate the "Micro-entrepreneurship Support Program" (MESP),[1] a large-scale government program administrated by the Chilean Ministry of Social Development for 1,950 participants, mostly women, in the Santiago Metropolitan Area. Chile's per capita GDP is US$21,980 purchasing power parity (PPP) (OECD 2016); 80 percent of women's employment in 2009 was wage employment. Over the evaluation period, the unemployment rate in Greater Santiago went from 7.4 percent in 2010 to 5.2 percent in 2013, with wage income increasing by 18 percent.[2] This dynamic labor market provides a unique backdrop for the evaluation of a micro-entrepreneurship program. While active labor market programs in Latin America have been evaluated to a certain extent—finding some positive results particularly among young people (Kluve 2016)—little is known about the long-run impact of micro-entrepreneurship programs in Latin America, in contrast to extensive evidence on such programs from OECD countries (Card, Kluve, and Weber 2015).

MESP has two components: an in-kind transfer of start-up capital, equivalent to about US$600 (approximately six times the beneficiary's average monthly labor income, which is 1.8 times the legal minimum wage), and 60 hours of training over one month on effective business practices. In addition, the program includes follow-up mentoring visits within the next three months. The asset transfer is made in kind, so that the entrepreneur can choose the required materials (or inputs) to purchase, according to the business plan developed during the training. The program targets beneficiaries of the Chilean anti-poverty program, "Chile Solidario," and 93 percent of its participants are women.[3]

In order to evaluate MESP, we randomly allocated individuals into three groups: a pure control group, a treatment group receiving the regular program asset transfer and training, and a second treatment group receiving the regular program plus an additional US$240 asset transfer on top of the original intervention. This second treatment group (referred to as MESP+) was created to study further the effects of in-kind asset transfers.

Our paper contributes to previous literature in three areas: first, we evaluate a government program that covers approximately 24,000 beneficiaries per year. Therefore, considerations of program scalability common in previous literature are not of concern. Second, we include two different levels of asset transfer, allowing examination of the optimal asset level required for the growth of small-scale entrepreneurs.[4] Third, we use administrative data from the Unemployment Insurance

[1] In Spanish, the program was known as "Programa de Apoyo al Microemprendimiento" (PAME). In 2011, it was renamed "Yo Emprendo Semilla" (YES).

[2] National Institute of Statistics (2016a, b) (INE in Spanish).

[3] Average monthly per capita income in our sample is US$98 (US$155 in PPP terms), i.e., US$5.1 per day in PPP. These income levels are high compared to the sample of six low income countries in Banerjee et al. (2015), where 48 percent of the sample exhibit consumption below $1.25 per day PPP, and compared to the sample in Blattman et al. (2016), where the average income is below US$1 per day in Uganda.

[4] To the best of our knowledge, this is the first paper that evaluates a micro-entrepreneurship program that combines training and transfers using two different asset transfer levels. Previously, de Mel, McKenzie, and Woodruff (2008) studied the effects of different levels of asset transfers, but without a business training component.

(UI) system, allowing the analysis of program impacts in the formal labor market over almost four years.

An occupational choice model framework is useful for the interpretation of our results: individuals choose between self-employment and wage employment, depending on returns. Under liquidity constraints, common among low income individuals, an asset transfer similar to that of MESP facilitates entrepreneurial activities.[5] At the same time, if the labor market tightens, as it did in Chile towards the end of the evaluation period, wage employment will be more attractive, potentially drawing people from self-employment to wage employment.

Our results indicate that MESP and MESP+ increased employment and earnings in both the short run and the long run. Nine months after the program concluded, self-employment increased by 14.8 and 25.2 percentage points for MESP and MESP+, respectively. In the long run, almost three years after implementation, there is still an effect on employment, but its composition depends on the asset transfer level: MESP+ increased self-employment by 7 percentage points, while MESP increased wage employment by 9 percentage points. In the short run, both groups increased labor income, but in the long run the effect was significant only for MESP, through an increase in income from wage employment. The administrative data from the UI show similar results, indicating that there were positive income effects of MESP on income from wage employment, but no income effect for MESP+. Additionally, the UI data show positive but not significant effects on formal employment in the long run, for both MESP and MESP+.

Results suggest that in the short run, a larger transfer is useful to increase small business productivity and thereby substitute for wage employment. In the long run, individuals assigned to MESP+ tend to stay in self-employment, which is consistent with their businesses being more productive and/or with the presence of fixed costs of changing occupations and wage employment not being attractive enough.

Some caveats are important to mention. First, we cannot separate out the effects of training from those of the asset transfer; we can only compare different levels of asset transfers conditional on training having been received. Second, there is differential attrition and a mild imbalance in our sample. We study the potential effects of attrition using bounds as in Karlan and Valdivia (2011), finding that our results hold for the less severe cases of attrition bias.

The paper is divided into five sections. After this introduction, we discuss the intervention in Section I; the data and variables used in the paper are described in Section II; we present the results in Section III, and we discuss them in Section IV. We summarize our main conclusions in Section V.

[5] In Chile, only 4 percent of micro-entrepreneurs started their businesses using a formal loan and only 8 percent hold a bank loan (Encuesta de Microemprendimiento 2010), Ministry of Economy). In our survey, only 7 percent had asked for a loan during the last year, and among those that had not asked for a loan, 40 percent did not do so because they thought they would not obtain it.

I. Intervention and Experimental Design

Individuals were randomly assigned to three groups: a pure control group of applicants who were not selected for program participation (observations = 566), a treatment group that received the regular MESP (observations = 689), and a second treatment group that received the regular MESP transfer plus an additional asset transfer under MESP+ (observations = 693). A comparison between the control group and the MESP group provides an estimate of the program's impact; a comparison between the two treatment groups provides an estimate of the effect of the additional transfer, conditional on having received the regular MESP training and the original asset transfer.[6] We detail the group assignment and experimental design below.

A. *The Micro-entrepreneurship Support Program* (*MESP*)

MESP is offered twice per year by the Ministry of Social Development through its implementing agency.[7] The program's purpose is to provide individuals with the skills and capital required to generate income through self-employment by developing their own businesses. MESP's target population comprises extremely poor households. Our sample consists of individuals over 18 years old who are beneficiaries of the anti-poverty program "Chile Solidario" and either unemployed or underemployed.[8] All individuals in our sample applied for the second round of MESP inscriptions in 2010 in the Metropolitan Region of Santiago. The intervention was conducted from October 2010 to February 2011.

Individuals were required to apply to the program at Fondo de Solidaridad e Inversión Social (FOSIS) municipality offices by electronic application, responding to questions to determine eligibility (age, employment situation, and an economic vulnerability score).

The MESP program combines training and an asset transfer. The training runs for four months. The first three weeks consist of 60 hours of intensive formal training in micro-entrepreneurial skills, conducted in 12 group sessions of about 30 people per group. All MESP graduates must have an attendance rate of at least 90 percent in order to qualify for the asset transfer. Over the next three months, beneficiaries are visited three times for program administrators to follow up on the business' performance and to provide managerial advice.[9] The training was conducted October through November 2011, and the asset transfer was delivered in November 2011.

[6] It was politically impossible to separate the training and capital program components in order to individually assess the effectiveness of each component.

[7] The program is implemented by the "Solidarity and Social Investment Fund" (in Spanish: Fondo de Solidaridad e Inversión Social, FOSIS).

[8] The Chilean anti-poverty program Chile Solidario lasts for two years, after which the individuals exit the program (and the monetary benefit ceases). "Underemployment" is loosely defined by the government agency implementing Chile Solidario, and considers as underemployment occupations that provide very low income and require few working hours. Applicants to MESP demonstrate their economic vulnerability by filling out a Social Security Card (SSC, in Spanish: Ficha de Protección Social—FPS), answering various questions about the household, and obtaining a score below the twentieth percentile. The SSC score ranges from 2,072 to 16,316 points; a lower number indicates greater economic vulnerability.

[9] The training and visits are carried out by implementing institutions that are selected through a bidding process. These private organizations are usually nongovernmental organizations (NGOs) or tertiary educational institutions.

The 60-hour MESP training is divided into five parts according to the program protocol: up to eight hours to set up the business concept, at least 20 hours on management skills improvement (such as setting and evaluating progress on business goals, defining products, obtaining customer feedback, and learning about current legislation), at least 20 hours on writing a business plan, at least 8 hours to plan the activities over the next 3 months to be reviewed during the follow-up visits, and up to 4 hours for the beneficiaries to obtain price quotes for the assets they will purchase with the transfer. The business plan can be developed to start a new business or to expand an existing business.

Following the formal training, the beneficiary receives financial support in the form of an in-kind transfer of about US$600 (Ch$300,000) to spend on machinery, raw materials, or other inputs.[10] The spending plan must be authorized by FOSIS according to the business plan developed by the beneficiary during the training period. The trainer accompanies the entrepreneur to purchase these inputs. The amount of funding is standard and does not differ by type of business, economic sector, or geographic location. There is no confirmation as to the existence/use of the asset transfer by the government after the program finishes.

B. *MESP with Additional Funding (MESP+)*

Six months later, an additional transfer of US$240 (Ch$120,000) was made to beneficiaries on top of the US$600 they received under the regular MESP. Spending rules for this transfer were the same as the initial MESP transfer. Individuals did not know about the additional funding when purchasing the first asset as part of their participation in the regular MESP program, and therefore did not consider it when planning for their first round of funding. Figure 1 shows the intervention calendar.

C. *Experimental Design*

Once applications were completed in each municipality (by June 2010), FOSIS checked individual and business eligibility, and sent the screened list to the research team. We randomized participants at the individual level, stratifying within the 27 municipalities and quartiles of the Social Security score. Individuals were grouped into 18 training classes.[11]

The implementing agency then visited each individual selected to participate in the program, conducted a short survey for FOSIS collecting socioeconomic information, and invited them to the training session. We collaborated closely with FOSIS to avoid any contamination of the control group. Only one individual of the control

There are protocols for service provision, including for class content and size, and requirements for a transportation subsidy and childcare. We observed a random sample of training sessions and confirmed that the protocols were correctly implemented.

[10] A maximum of 20 percent can be received in cash or as working capital. More details can be found in online Appendix 1.

[11] Individuals assigned to the control group received a letter from FOSIS indicating that they were not selected due to excess demand, but that they could apply in the following year.

Year	Month	Months since baseline survey	Event	Notes
2010	July		MESP application	Applications accepted starting July 26th.
	August		Random assignment	Randomization was carried out between August 19th and 27th as municipality lists were closing.
	September	**0**	**Baseline survey**	**By September 10th, 86% of the sample was already surveyed.**
	October	1	MESP training begins	
	November	2	MESP initial capital delivery	
	December	3	MESP follow-up visits begin	
2011	January	4		
	February	5	MESP exit	
			↓	
	July	10	MESP + additional funding delivery	Additional funding was delivered between July and August.
	August	11	MESP + additional funding delivery	
	September	12		
	October	**13**	**Follow-up 1 survey**	**Follow-up 1 was mainly during October (from September 18th through November 30th).**
		31	↓	
		32		
		33		
		34		
		35		
2013		36		
	October	**37**	**Follow-up 2 survey**	**Follow-up 2 took place mainly during October and November (from September 18th through January 31st).**
2014		42	↓	
		43		
		44		
	June	45	Administrative data ends	

FIGURE 1. MESP TIMELINE

group eventually received treatment. Individuals could decide to stop participating at any time, but 78 percent of individuals who were offered to participate in MESP graduated from the program. All individuals assigned to treatment, independent of their treatment take-up, are considered in the analysis as part of the treatment group.

Table 1—Survey Response Rates

Survey	All	*Panel A.* Treatment arm			*Panel B. p*-value		
	(1)	Control (2)	MESP (3)	MESP+ (4)	MESP − C (5)	MESP+ −C (6)	MESP−MESP+ (7)
Total assigned	1,948	566	689	693			
2010 baseline	94.4%	94.0%	94.2%	94.9%	0.738	0.933	0.659
2011 endline	87.9%	86.6%	86.1%	90.8%	0.547	0.051	0.007
2013 endline	77.1%	76.3%	74.5%	80.5%	0.633	0.046	0.009

Notes: Column 1 reports statistics for the full sample and columns 2–4 by treatment arm. In panel A, the first row (total assigned) is the assigned sample; the following rows are the percentages of individuals tracked for each survey. The percentages are computed over the total assigned. Panel B shows the *p*-value of the null hypothesis of balanced attrition: column 5 attrition between MESP and control group, column 6 between MESP+ and control group, and column 7 between MESP and MESP+.

II. Data and Descriptive Statistics of the Study Population

We have two sources of data: household surveys and administrative records. In this section, we discuss attrition due to different survey response rates, the balance of our treatment arms, and the population under study.

A. *Data*

We evaluate MESP with three waves of household surveys: a baseline survey, conducted August–October 2010, a first follow-up, conducted October–November 2011, and a second follow-up, conducted September–December 2013.[12] We complement our analysis with administrative data from the Unemployment Insurance system (UI) as an independent data source to assess formal wage employment and to extend the period of analysis. The UI data include information about the job tenure covered by the UI system (formal jobs in the private sector) and the wage received monthly in each job. We merged these monthly data for the period January 2009 to June 2014, allowing us to study the impact on formal employment at 41 and 45 months post-completion of MESP.

B. *Attrition among Treatment Arms*

We present the response rate for all rounds in panel A of Table 1 and the *p*-value of the *t*-test of the equality of attrition rates across treatment arms in panel B. Individuals randomly assigned to the three groups (566 in the control group, 689 to MESP, and 693 to MESP+) were sought for interviews in all survey rounds. Overall, we surveyed 94.4 percent, 87.9 percent, and 77.1 percent of the original sample at baseline, one-year follow-up, and three-year follow-up, respectively. These figures are comparable to those in previous studies (see online Appendix 2).

[12] To avoid bias and ensure the reliability of the instrument, we contracted an impartial third-party survey company to conduct the surveys. The survey was conducted after the training had already begun in only 7 percent of the cases.

Table 2—Study of Attrition by Treatment Arm

Dependent variable: Non-completed survey	Baseline (1)	Follow-up 1 (2011) (2)	Follow-up 2 (2013) (3)
Panel A			
MESP	0.007	0.005	0.011
	(0.009)	(0.018)	(0.027)
MESP+	0.002	−0.035	−0.050
	(0.009)	(0.017)	(0.026)
Panel B			
Baseline characteristics			
Number of months formally employed in 2009	0.007	−0.003	−0.006
	(0.003)	(0.005)	(0.007)
Average formal earnings in 2009	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
Panel C			
Baseline characteristics interacted with MESP			
Number of months formally employed in 2009	−0.002	−0.005	0.005
	(0.006)	(0.009)	(0.011)
Average formal earnings in 2009	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Baseline characteristics interacted with MESP+			
Number of months formally employed in 2009	−0.009	0.001	−0.001
	(0.006)	(0.012)	(0.013)
Average formal earnings in 2009	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
p-value from test that baseline characteristics interacted with MESP are jointly 0	0.169	0.324	0.771
p-value from test that baseline characteristics interacted with MESP + are jointly 0	0.268	0.672	0.678
Observations	1,948	1,948	1,948
Attrition mean	0.056	0.121	0.228

Notes: The dependent variable takes a value of 1 if the individual was not found. Columns 1, 2, and 3 present results for the baseline, follow-up 1 and 2, respectively. The sample are all individuals originally sought. Panel A presents the differential attrition rate. Panel B reports the predictors of attrition. Panel C presents the results when baseline characteristics are interacted by MESP and MESP+.

The follow-up attrition rate of MESP+ is lower than the attrition of MESP and the control group, by 4–6 percentage points. This difference is statistically different from zero for both groups at both end line surveys (Table 1, panel A). We use the administrative data from the UI data to test for attrition on some observable characteristics (such as number of months formally employed before the program, earnings, age, and gender). A full set of interactions of these baseline characteristics with treatment assignment is not statistically significant (Table 2). This is consistent with attrition not leading to sample bias related to observables across treatment arms. Nevertheless, to assess potential selective attrition based on unobservables, we perform a sensitivity analysis below.

C. *Balance Prior to Treatment among Treatment Arms*

We report summary statistics and tests of balance in Table 3 using baseline survey data (panel A) and pretreatment UI data (panel B). There is balance across a wide

Table 3—Variable Means and Tests of Differences between Treatment Groups (*baseline*)

Variables	Observations	Control	MESP	MESP+	Regression *p*-values MESP = C	MESP+ = C	MESP = MESP +
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Survey data							
Gender (Male = 1)	1,839	0.071	0.065	0.076	0.709	0.779	0.514
Age	1,839	35,671	36,177	35,877	0.392	0.847	0.508
Primary education	1,836	0.305	0.315	0.325	0.767	0.432	0.623
Secondary education incomplete	1,836	0.229	0.235	0.267	0.787	0.125	0.206
Secondary education complete	1,836	0.425	0.383	0.358	0.133	0.015	0.354
Tertiary education	1,836	0.041	0.068	0.050	0.035	0.490	0.156
SSC score	1,839	3,420	3,365	3,457	0.695	0.720	0.974
Employed	1,829	0.649	0.659	0.658	0.767	0.896	0.872
Wage employed	1,829	0.165	0.189	0.175	0.496	0.940	0.547
Self-employed	1,829	0.507	0.496	0.508	0.823	0.937	0.763
Labor income (US$)	1,829	105.3	106.2	116.5	0.992	0.237	0.241
Wage work income	1,831	37.08	42.23	38.24	0.593	0.867	0.483
Self-employment income	1,837	67.60	63.55	78.19	0.693	0.107	0.045
Asset index	1,839	−0.005	−0.005	0.009	0.950	0.882	0.931
Credit constraint	1,839	0.395	0.414	0.378	0.458	0.530	0.172
Skills index	1,836	0.015	0.002	−0,014	0.816	0.521	0.682
Panel B. Unemployment insurance data							
Number of months formally employed in 2009	1,948	1,157	1,289	1,491	0.466	0.053	0.212
Average formal earnings in 2009	1,948	42.49	46.50	49.29	0.745	0.468	0.644
Number of months formally employed in 2010	1,948	0.671	0.702	0.729	0.881	0.582	0.675
Average formal earnings in 2010	1,948	49.31	50.80	50.03	0.967	0.948	0.909
Panel C. p-values of F-test							
MESP vs. C	0.815						
MESP + vs. C	0.128						
MESP vs. MESP +	0.291						

Notes: Column 1 shows the number of observations. Columns 2, 3, and 4 show the mean value for the control group, MESP and MESP+, respectively. Columns 5–7 report the *p*-values of the regressions of treatment assignment controlling for strata. Regressions are weighted to account for different probabilities of selection into each stratum (following Duflo, Glennerster, and Kremer 2008). Column 5 reports the *p*-value of the null hypothesis that MESP=Control Group, column 6 shows the *p*-value of the null hypothesis that MESP+ = Control Group, and column 7 reports the *p*-value of the null hypothesis that MESP = MESP+. Panel A data are from a baseline survey; panel B data are from Unemployment Insurance administrative data. Panel C reports the *p*-values of the joint significance test of the covariates (all variables from panel B and self-employment, wage employment, self-employment income, wage income, and assets from survey data) on treatment assignment. Sample size varies due to missing values. Income variables are measured in November 2009 US dollars.

range of survey measures, although three variables report imbalance: the control group reports 6 percentage points more secondary education than MESP+, MESP exhibits 2.5 percentage points more tertiary education than the control group, and MESP+ has US$14 more self-employment income than the MESP group. As we are testing balance in several variables, these differences can occur by chance.

The availability of administrative data from the UI allows us to address the balance of formal employment 18 months prior to application to the program, (which began in July 2010). Throughout the period 2009 to 2010, formal employment was scarce among applicants: on average, they were formally employed for 1.16 and 0.67 months in 2009 and 2010, respectively, while formal monthly income was just above US$42 and US$49, respectively (Table 3, panel B).

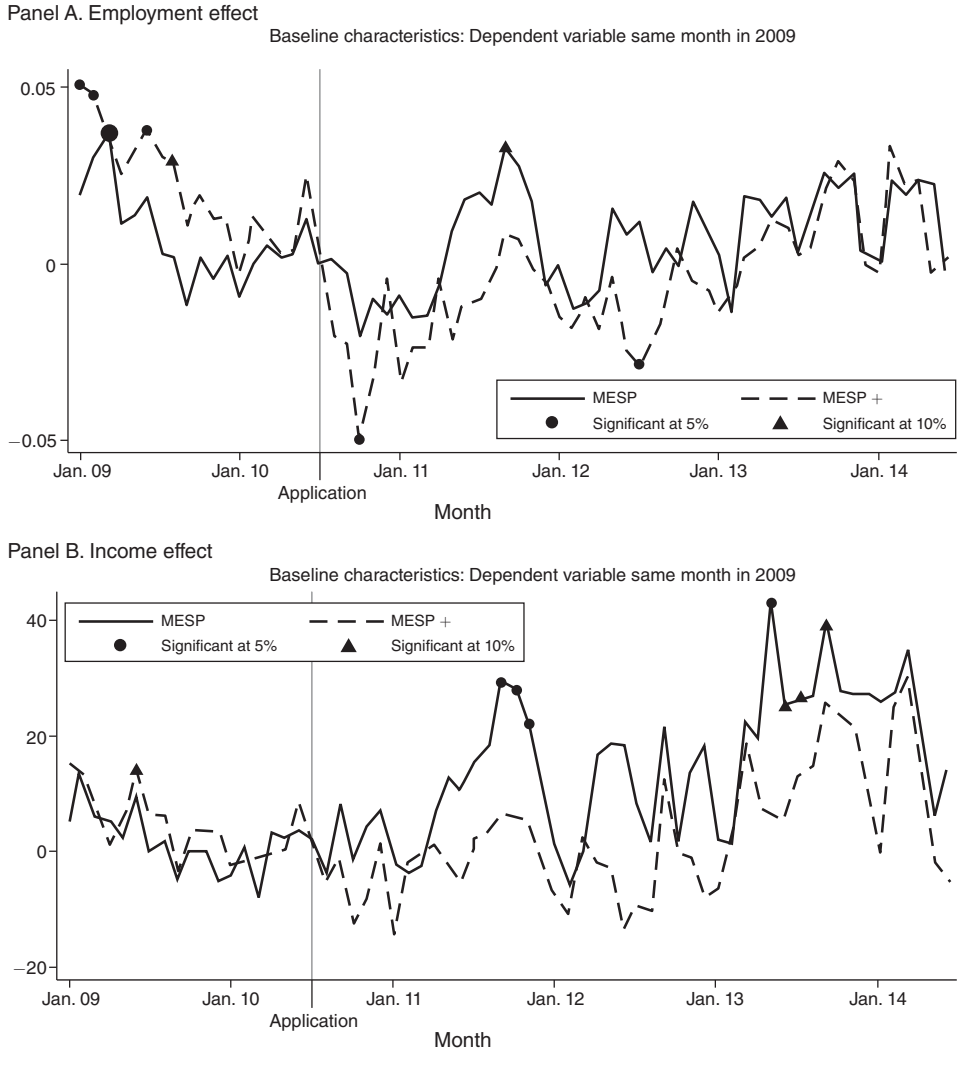

FIGURE 2. INTENT-TO-TREAT EFFECT ON WAGE EMPLOYMENT AND INCOME (*data from Unemplyment Insurance*)

In 2009, we find a mild imbalance between MESP+ and the control group: MESP+ has 0.3 additional months of formal employment ($p < 0.10$, column 6). However, there are no statistically significant differences in the 2010 pretreatment employment patterns in the UI dataset.

A month-by-month balance check using UI data is shown in Figure 2. The figure is divided into two periods. The period before July 2010 presents a balance test for MESP versus the control group and MESP+ versus the control group. After July 2010, the figure shows program effects. There is imbalance between the MESP+ group and the control group in the first months of 2009, with more formal employment in the MESP+ group. These differences vanish in 2010.

To study further the existence of imbalance, we use merged administrative (average of formal employment and income) and survey data (employment and income

information by type of occupation and assets) to predict classification into each treatment arm. The *p*-values of the tests that different pretreatment variables jointly predict being in any treatment arm (MESP versus control, MESP+ versus control, and MESP versus MESP+) are presented in Table 3, panel C. All *p*-values are over 0.05, consistent with the existence of balance across treatment arms (Table 3, panel C).

Overall, the evidence is inconclusive on the existence of imbalance. To be on the safe side and to control for any pretreatment differences across treatment arms, we present our main results controlling for the baseline characteristics available for the full sample and for which *F*-tests are presented in panel C of Table 3. Results are robust to the exclusion of all these variables (online Appendix 3).

D. *The Study Population*

We present the descriptive statistics of the program population in Table 3. The vast majority of the beneficiaries are women (93 percent) and the mean age is 36 years old. Approximately 31 percent of individuals have only completed primary education. The average SSC score is 3,420 points, well below the eligibility threshold of 8,500 points, indicating a high degree of economic vulnerability.

Regarding employment variables, 65 percent report being employed at baseline and about 50 percent report being self-employed. Average monthly labor income for the whole sample is approximately US$105 (considering both employed and unemployed), which is significantly lower than the legal minimum wage of US$344.

We define employment and employment type based on reported income: individuals who report income from self-employment activities are classified as self-employed; those who report income from wage work are classified as wage earners. Since individuals can report income from both types of activities, they can be classified into both types of employment simultaneously. Consequently, if an individual reports any type of labor income, they are classified as employed. We present a detailed description of the construction of all variables in online Appendix 4.

These summary statistics also shed light on the special characteristics of the applicants with respect to the eligible population: applicants are overwhelmingly women[13] and have higher education levels and employment rates than what is observed for females in the lowest income decile in the Chilean population.[14] Therefore, the external validity of the results of the evaluation should be carefully considered and the potential extensions of the program to poor individuals should take into account these characteristics.

Data on type of business suggest that these are mostly one-person businesses (80 percent of businesses have a single business owner; the average number of employees is 0.26) and are very diverse and informal. According to exit data

[13] Although MESP is not intended to be only for women, it offered child care to satisfy beneficiaries' needs. It is important to consider that the 60 hours of training sessions in the mornings over two continuous weeks imply a high opportunity cost when compared to other labor activities.

[14] Compared to individuals in the lowest decile of the population, the MESP sample has a larger proportion who have some high school education and a lower proportion with only primary or some tertiary education. The employment rate for women in the lowest decile in the Chilean population is 30 percent (own calculations based on CASEN survey 2009).

collected by the implementing agency, the most common economic sectors are textile and clothing (38 percent), food (e.g., bakeries and green groceries, 21 percent), and small retail (10 percent). Only 5 percent are formally registered with the Tax Office.

Even though all individuals applied for this micro-entrepreneurship program, a portion of the sample indicates some preference for wage employment. For instance, in the baseline survey, 38 percent of individuals that were not wage employed at the time of the interview declared that they would prefer to work for wages over self-employment.

III. Impact Evaluation Results

Our empirical strategy relies on the random allocation of each eligible individual to a treatment group. The basic regression is based on the following equation:

$$(1) \qquad y_{i,t} = b_0 + b_1\, MESP_i + b_2\, MESP^{+}{}_i + \alpha_1\, y_{i,0} + \alpha_2 x_{i,0}^{UI} + S_{i,j} + \varepsilon_{i,t},$$

where $y_{i,t}$ is the outcome variable (employment, income, or hours of work) of individual i at time t, $MESP_i$ and $MESP+_i$ are dummy indicators of the treatment assignment, and y_{i0} corresponds to the baseline value of the dependent variable; $x_{i,0}^{UI}$ corresponds to baseline variables from the merged UI-survey data, and $S_{i,j}$ are strata fixed effects (strata correspond to SSC score group and the municipality where the individual lives). Baseline variables $(y_{i,0})$ of the dependent variable are available for employment, income, and assets. Errors are clustered at the municipality level to capture common shocks. Regressions are weighted following Duflo, Glennerster, and Kremer (2008) to consider different selection probabilities into the treatment groups in each stratum.[15] Thus, b_1 and b_2 are interpreted as the intention-to-treat (ITT) effects of MESP and MESP+, respectively.

We now study the impact on the main outcomes of interest: employment, income, assets, business practices, and other business outcomes.

A. *Impacts on Employment and Occupational Choice Using Survey Data*

We present the impact of assignment to treatment group on labor market outcomes in Table 4 according to equation (1). The outcomes include self-employment and wage employment (panel A), income and hours worked (panel B), and alternative measures of self-employment business success (panel C). For each outcome, we present results at 12 months (columns 1 to 5) and at 36 months (columns 6 to 10) using data from the two follow-up surveys. For each variable, we show the control group means (columns 1 and 6), the coefficients (and standard errors) on each indicator of treatment group, MESP or MESP+ (columns 2–3 and 7–8), the p-value of the difference of the coefficients of MESP versus MESP+ (columns 4 and 9), and the sample size (columns 5 and 10). All regressions in panels A and B

[15] Details on the weighting methodology can be found in online Appendix 5.

Table 4—Intent-to-Treat Effects on Main Labor Market Outcomes

	2011					2013				
	Control (1)	MESP (2)	MESP+ (3)	*p*-value (4)	Sample size (5)	Control (6)	MESP (7)	MESP+ (8)	*p*-value (9)	Sample size (10)
Panel A. Employment										
Self-employment	0.444	0.148	0.252	0.003	1.625	0.412	0.026	0.070	0.083	1,427
		(0.038)	(0.039)				(0.034)	(0.029)		
Wage employment	0.264	−0.016	−0.050	0.123	1.625	0.336	0.090	0.005	0.003	1,427
		(0.029)	(0.026)				(0.029)	(0.037)		
Total employment	0.659	0.116	0.181	0.021	1.625	0.699	0.073	0.056	0.422	1.427
		(0.031)	(0.028)				(0.031)	(0.032)		
Panel B. Income and hours worked										
Self-employment income	78.33	32.57	55.23	0.118	1,633	90.10	6.59	11.86	0.537	1,433
		(14.778)	(13.424)				(12.975)	(14.036)		
Wage income	62.63	13.43	10.38	0.757	1,649	114.0	33.35	6.03	0.058	1,430
		(11.475)	(12.235)				(13.460)	(13.271)		
Total labor income	142.1	47.23	67.06	0.198	1,625	204.6	39.81	19.33	0.085	1,427
		(20.259)	(17.267)				(15.568)	(18.449)		
Weekly hours worked	20.17	4.37	7.43	0.042	1,677	24.65	3.20	2.61	0.491	1,500
		(1.417)	(1.583)				(1.544)	(1.804)		
Panel C. Business variables										
Number of employees	0.400	0.448	0.477	0.683	1,712	0.613	0.022	0.127	0.085	1,503
		(0.064)	(0.055)				(0.062)	(0.064)		
Sales in past month	83.02	60.90	119.2	0.032	1,712	131.7	−0.007	42.97	0.059	1,503
		(20.52)	(26.12)				(20.16)	(27.74)		
Profits in past month (only 2011)	54.01	40.81	71.80	0.066	1,646					
		(14.95)	(16.92)							
Profits in good month (only 2013)						139.6	−11.99	16.97	0.125	1,503
							(28.37)	(34.98)		
Profits in bad month (only 2013)						37.57	−1.144	12.89	0.150	1,503
							(8.336)	(12.38)		
Profits in average month (only 2013)						76.66	−7.603	7.576	0.194	1,503
							(17.71)	(21.46)		

Notes: Columns 1 and 6 report the mean for the control group at each end point. Columns 2–3 and 7–8 report the intent-to-treat (ITT) estimates and standard errors (in parentheses) of program assignment at each end point. Columns 4 and 9 report the *p*-value of the null hypothesis that MESP = MESP+. All income variables are measured in real US dollars (using exchange rates as of November 2009). The number of employees includes the individual interviewed. Regressions include dummies for strata (defined by a socioeconomic index computed by the government using the Social Security Card score and municipality of residence). All regressions in panels A and B, with the exception of weekly hours worked, include the baseline value of the dependent variables, and employment and earnings covariates from Unemployment Insurance data. Regressions in panel C do not include the baseline value of the dependent variable. Regressions are weighted to account for different probabilities of selection in each stratum (following Duflo, Glennerster, and Kremer 2008). Standard errors are calculated allowing for clustering at the municipality level. Sample size varies due to missing values.

include the baseline value of the dependent variable (and employment and earnings from UI data), with the exception of hours worked, due to lack of data availability. Regressions in panel C do not include the baseline value of the dependent variable for the same reason.

In the short run, both MESP and MESP+ substantially increase self-employment, by 14.8 and 25.2 percentage points, respectively (columns 2 and 3 in Table 4, panel A). The larger transfer of MESP+ has a significantly larger effect than MESP (p-value = 0.003, Table 4, panel A, column 4). In the same period, MESP and MESP+ decrease the probability of being wage employed, though the effect

is marginal (not significant only for MESP+). Overall, both MESP and MESP+ increase total employment, and the increase is larger for MESP+, with corresponding figures of 11.6 percentage points and 18.1 percentage points, respectively.

For the long-run results, we find that both treatment arms have a positive impact on self-employment, but the size of the effect is smaller than in the short run. Furthermore, only the effect of MESP+ is statistically significant (7 percentage points, column 8).[16] Therefore, the additional transfer was successful in keeping self-employment businesses operating. However, MESP+ does not affect wage employment, whereas somewhat unexpectedly, MESP significantly increases wage employment in the long run by 9 percentage points. Finally, both treatment arms increase total overall employment: MESP by 7.3 percentage points and MESP+ by 5.6 percentage points. However, MESP increases employment through wage employment, whereas MESP+ does so through self-employment.

We also investigate whether positive employment effects translate into changes in labor earnings. In the short run, MESP and MESP+ increased monthly self-employment income in 2011 by 41 percent and 70 percent, respectively, compared to the control group (US$32 and US$55 on top of the US$78 obtained by the control group, panel B of Table 4). Therefore, the increase in self-employment induced by the program also increased income from self-employment. However, though the coefficients on MESP and MESP+ are positive, there are no significant effects on wage employment income in the short run. Adding up the effects on both labor markets, total labor income increases under both treatment groups, 33.2 percent for MESP and 47.6 percent for MESP+.[17]

In examining long-run impacts on income, we observe that the strong short-run effects vanish in 2013 for MESP+: although the coefficients for all income measures remain positive, they are much smaller in magnitude and not statistically significant. However, MESP has a positive and significant impact on wage income (29 percent, an increase of US$33 over the US$114 of the control group) and on total labor income (20 percent, an increase of US$40 over the US$204 of the control group). Note that the increase from 2011 to 2013 in wage employment observed in the MESP group occurs at the same time that the control group has increasing wage employment participation and income, consistent with the aggregate wage employment boost that occurred in Chile during that period.

There is also a substantial increase in hours worked, with a larger significant effect for MESP+ in the short run as compared to MESP (an increase of seven hours increased per week MESP+ and four for MESP). As with employment, effects are smaller in the long run. The estimated average number of hours worked for the control, MESP, and MESP+ groups are: 31 (20.17 hrs/0.659 employment rate), 32, and 33, respectively.[18] As the average is similar among the treatment groups, the ITT effect on hours worked appears to come from a greater number of individuals

[16] We cannot reject that the effect of MESP and MESP+ is the same for this outcome (p-value $= 0.083$, column 9 of Table 4).

[17] Although the difference between the effects of MESP and MESP+ on total labor income is important, it is not statistically significant.

[18] The figures for MESP and MESP+ are computed from the control group means and ITT effects presented in Table 4 as $(20.17 + 4.37)/(0.659 + 0.116)$ and $(20.17 + 7.43)/(0.659 + 0.181)$, respectively.

employed (extensive margin), rather than the number of hours actually worked by those who were employed (intensive margin).

The persistent effect of MESP+ on the self-employment probability presented in Table 4 is consistent with the hypothesis that the additional transfer made business more viable. To shed light on this hypothesis, we estimate how the program affected business survival rates by creating a dummy variable for individuals that were self-employed in both 2011 and in 2013. Results are presented in Table A6.8, column 1, online Appendix 6.4. As expected, both MESP and MESP+ increased the prevalence of continuous self-employment, and while the MESP+ had a larger impact than MESP, the coefficients are not statistically different. Therefore, it is not clear that MESP+ increases the survival rate of self-employment compared to MESP.

Finally, we find an increase in employment of other household members in the short run, and a moderate impact on per capita household income (Table A7, online Appendix 7). Therefore, the program does not crowd out employment of other household members.

B. *Impacts on Formal Wage Employment Using Administrative Data from Unemployment Insurance*

The use of high frequency administrative data from the UI allows us to overcome possible survey response bias by testing impacts on formal wage employment and analyzing a longer time frame. Note that by construction, the UI data on employment, which considers only formal jobs, is a subset of our definition of wage employment for the survey data analysis (see online Appendix 4.2 for details).

We estimate ITT coefficients using administrative data from UI for each month (Figure 2). Panel A of Figure 2 displays coefficients for employment, and panel B presents those for earnings. As mentioned in Section IIIC, from July 2010 to June 2014, we employ specification (1), using as baseline characteristics the employment information for the same month as that of the dependent variable, for the year 2009. For instance, when estimating the effect of the treatments in March 2011 on employment, we include as covariates employment in March 2009. We proceed similarly for the income estimates.

Figure 2 shows that starting in 2013, both treatment arms appear to have a positive effect on wage employment, though effects are mostly insignificant. The effect on earnings is more noticeable than the employment effect, particularly for MESP, as it rapidly moves from negative effects in 2010 to positive effects, and significant effects in 2011 and 2013 (for some months). Also, one can see that point estimates of MESP on wage employment and income are above those of MESP+ for almost every month, consistent with findings from survey data. The point estimates of MESP impacts are less than 5 percentage points (see online Appendix 8). These estimates are smaller than the results that we found for wage employment using survey data, which is consistent with unemployment insurance data representing a subset of total employment—specifically, considering only formal wage employment, which could be less accessible for the program beneficiaries.

Overall, the findings with the UI data that MESP has positive effects on wage employment income during 2013 provide a unique robustness check, as the literature

on micro-entrepreneurship or active labor programs seldom has access to high quality administrative records.

C. *Impacts on Business Outcomes*

Measuring business outcomes has proven to be rather difficult (see, for example, de Mel, McKenzie, and Woodruff 2009b). We use a set of alternative business outcomes from survey data to assess the effectiveness of the treatment on the outcomes of number of employees, sales, and profits.

The average number of employees in the control group is 0.4 (not including the employer); that number is doubled for both MESP and MESP+ in 2011, and increases further in 2013, but only for MESP+ (Table 4, panel C). Sales and profits also increased substantially in 2011 for both treatment groups and almost doubled for MESP+, with both sales and profits statistically significantly larger for MESP+ than for MESP. Similar to the long run self-employment income results, the effects on sales and profits are positive but not significant for MESP+ in 2013.

Overall, business outcome results are consistent with the increase in self-employment prevalence and earnings reported in Section IVA, as well as with positive effects decreasing over time.

D. *Impacts on Business Practices and Assets*

As MESP includes both business training and asset transfer, we investigate its effects on the business practices and asset accumulation reported in follow-up surveys as a check on whether the treatments are working through the expected mechanisms. We follow de Mel, McKenzie, and Woodruff (2014) to build business practices and asset indices. We use a set of questions on practices in marketing, inventories, records, and financial planning, which are summed to create a business practice index, and at the same time, we construct a standardized asset index using principal components analysis.[19]

The average number of business practices used by the control group in the short run is 3 out of 25 in 2011 (Table 5). ITT results indicate that there are sizeable positive effects on self-reported business practices under MESP and MESP+ in both the short run and the long run (column 1, panel A of Table 5). Note that in the short run, business practices more than doubled, even though the impact decreased in the long run. These results suggest that the training may have positively affected the business practices of small entrepreneurs for at least three years, and the effect is larger when larger transfers are given.

The impact on self-reported business practices could be upward biased if individuals were already following the practices, and the training simply improved the quality of reporting. We address this potential weakness by asking enumerators to report the existence of inventory and registry books at the time of the interview. We find positive and significant effects on these outcomes in the short run only.

[19] We thank Christopher Woodruff for facilitating use of the questionnaire for business practices. See online Appendix 4.4 for the specific questions used. For the asset index details, see online Appendix 4.8.

TABLE 5—MECHANISMS

	2011					2013				
	Control (1)	MESP (2)	MESP+ (3)	*p*-value (4)	Sample size (5)	Control (6)	MESP (7)	MESP+ (8)	*p*-value (9)	Sample size (10)
Panel A. Business practices										
Business practices (min 0 – max 25)	3.166	4.478 (0.472)	5.590 (0.688)	0.073	1,710	4.282	0.759 (0.372)	1.453 (0.544)	0.078	1,503
Inventory or registry book available (min 0 – max 1)	0.023	0.039 (0.014)	0.058 (0.013)	0.128	1,700	0.067	0.026 (0.016)	0.018 (0.019)	0.720	1,502
Panel B. Business assets										
Asset index	−0.289	0.459 (0.075)	0.552 (0.083)	0.168	1,712	−0.084	0.068 (0.059)	0.145 (0.073)	0.100	1,503

Notes: Columns 1 and 6 report the means for the control group at each end point. Columns 2–3 and 7–8 report the intent-to-treat (ITT) estimates and standard errors (in parentheses) of program assignment at each end point. Columns 4 and 9 report the *p*-values of the null hypothesis that MESP = MESP+. "Inventory or Registry Book" is a dummy variable that takes the value of 1 when the interviewee answered that she can see these items in her business. Sample size varies due to missing values. Business practices are described in online Appendix 7. The asset index was constructed with principal components analysis. Regressions include dummies for strata (defined by a socioeconomic index computed by the government using the Social Security Card score and municipality of residence) and are weighted to account for different probabilities of selection in each stratum (following Duflo, Glennerster, and Kremer 2008). Baseline value of the dependent variable is included only in panel B. Standard errors are calculated allowing for clustering at the municipality level.

In the analysis of the asset index, we found that both treatment programs increase asset accumulation. In the short run, both MESP and MESP+ increased business assets by approximately 0.8 standard deviations ($p < 0.05$) in 2011 (the normalized asset index for the control group is −0.29, and ITT estimates are 0.46 for MESP and 0.55 for MESP+, Table 5, panel B). However, in the long run, only MESP+ maintains significance ($p < 0.10$). As in the case of business practices, the impact on assets seems larger when larger transfers are given. Overall, these results are consistent with the effects found for employment, profits, and income.

Finally, administrative data show that individuals receiving the asset transfer (either MESP or MESP+) used the money to purchase tools (46 percent), inputs (43 percent), infrastructure (6 percent), or "other" investments (5 percent) (mostly working capital). Individuals who were micro-entrepreneurs when applying to the program spent less on infrastructure and more on inputs, though differences were not statistically significant (results are available upon request).

E. *Sensitivity Analysis of Economic Impacts Considering Attrition*

As mentioned previously, the lower attrition rate of MESP+ with respect to MESP and the control group could potentially generate attrition bias. We address this concern by following a strategy similar to Karlan and Valdivia (2011), that is, by imputing outcome values for missing data. We conduct the analysis emphasizing the tests of the significant effects presented previously with survey data.

We impute missing outcomes of both treatment arms involved in the comparison until reaching the full sample size. Since our interest is to test how robust our results are to selective attrition, the imputation is meant to reduce the size of

Table 6—Sensitivity Analysis of Intent-to-Treat Estimates of MESP+ versus MESP to Different Missing Data Scenarios

	ITT			0.1 SD			0.25 SD			0.5 SD		
	MESP (1)	MESP+ (2)	*p*-value (3)	MESP (4)	MESP+ (5)	*p*-value (6)	MESP (7)	MESP+ (8)	*p*-value (9)	MESP (10)	MESP+ (11)	*p*-value (12)
Self-employment 2011	0.148	0.252	0.003	0.166	0.247	0.007	0.175	0.241	0.026	0.192	0.231	0.168
	(0.038)	(0.039)		(0.032)	(0.034)		(0.031)	(0.034)		(0.031)	(0.034)	
Total employment 2011	0.116	0.181	0.021	0.125	0.175	0.034	0.133	0.170	0.106	0.147	0.162	0.493
	(0.031)	(0.028)		(0.025)	(0.025)		(0.025)	(0.024)		(0.025)	(0.024)	
Wage employment 2013	0.090	0.005	0.003	0.049	0.008	0.067	0.031	0.022	0.678	0.001	0.044	0.085
	(0.029)	(0.037)		(0.026)	(0.029)		(0.026)	(0.029)		(0.027)	(0.029)	

Notes: Columns 1–2 replicate the ITT results from Table 3; column 3 replicates the *p*-values of the null hypothesis that MESP = MESP+ from Table 3. In columns 4–5, 7–8 and 10–11, we reestimate the ITT of columns 1 and 2, making hypothetical assumptions about missing data, and in columns 6, 9, and 12, we report the *p*-values of the null hypothesis that the coefficient on MESP = MESP+ in these scenarios. For self-employment and total employment in 2011, we impute relatively high (low) values of the dependent variables for MESP (MESP+), relative to the group mean plus (minus) 0.1, 0.25, and 0.5 standard deviation of the group's distribution. For wage work in 2013, we impute relatively high (low) values of the dependent variables for MESP+ (MESP), relative to the group mean plus (minus) 0.1, 0.25, and 0.5 standard deviation of the group's distribution. Standard errors in parentheses are calculated allowing for clustering at the municipality level. Sample size varies due to missing values. Regressions include dummies for strata (defined by a socioeconomic index computed by the government using the Social Security Card score and municipality of residence), the baseline value of the dependent variable, and employment and earnings covariates from Unemployment Insurance data. Regressions are weighted to account for different probabilities of selection in each stratum (following Duflo, Glennerster, and Kremer 2008).

the impacts we are testing. For example, since we found that MESP+ increases self-employment with respect to MESP in 2011, we impute the missing observations of self-employment in MESP+ (MESP) with its own group distribution mean minus (plus) 0.1, 0.25, and 0.5 standard deviations.[20] Then, we reestimate ITT effects under these data scenarios.

The sensitivity analysis results for MESP+ over MESP presented in Table 6 indicate that the 2011 larger effect of MESP+ over MESP for self-employment is robust up to a change of 0.25 standard deviations (p-value $=$ 0.026). For total employment in 2011, we find that MESP+ has a larger impact than MESP when imputing missing outcomes with values up to 0.1 standard deviations (p-value $=$ 0.034). The result that MESP+ has a larger impact than MESP on wage employment in 2013 is robust to 0.1 standard deviations (p-value $=$ 0.067).

The sensitivity analysis results for MESP+ over the control group presented in Table 7 indicate that in 2011, the effects on self-employment and total employment are robust up to an imputation of the mean minus 0.5 standard deviations. For wage employment in 2011 and self-employment in 2013, the results are robust up to 0.1 standard deviations.

In sum, the sensitivity analysis takes into account attrition that potentially could introduce some form of selection, and offers results consistent with data heterogeneity. Our main results persist under reasonable imputation scenarios.

[20] Karlan and Valdivia (2011) used 0.1 and 0.05 standard deviations. Using larger standard deviations tests robustness more stringently.

Table 7—Sensitivity Analysis of Intent-to-Treat Estimates of MESP+ versus Control Group to Different Missing Data Scenarios

	ITT (1)	0.1 SD (2)	0.25 SD (3)	0.5 SD (4)
Self-employment				
2011	0.252	0.241	0.227	0.203
	(0.039)	(0.035)	(0.035)	(0.037)
2013	0.070	0.050	0.019	−0.033
	(0.029)	(0.023)	(0.023)	(0.024)
Wage employment				
2011	−0.050	−0.042	−0.029	−0.008
	(0.026)	(0.023)	(0.024)	(0.024)
Total employment				
2011	0.181	0.170	0.157	0.135
	(0.028)	(0.025)	(0.026)	(0.027)
2013	0.056	0.034	0.006	−0.041
	(0.032)	(0.027)	(0.028)	(0.029)

Notes: Column 1 replicates the ITT results from Table 3 for MESP+. In columns 2–4, we reestimate the ITT of column 1, making hypothetical assumptions about missing data. For self-employment and total employment in 2011 and 2013, we impute relatively high (low) values of the dependent variables for the control group (MESP+), relative to the group mean plus (minus) 0.1, 0.25, and 0.5 standard deviation of the group's distribution. For wage employment in 2011, we impute high (low) values to MESP+ (control group), relative to the group mean plus (minus) 0.1, 0.25, and 0.5 standard deviation of the group's distribution. Standard errors in parentheses are calculated allowing for clustering at the municipality level. Sample size varies due to missing values. Regressions include dummies for strata (defined by a socioeconomic index computed by the government using the Social Security Card score and municipality of residence), the baseline value of the dependent variable, and employment and earnings covariates from Unemployment Insurance data. Regressions are weighted to account for different probabilities of selection in each stratum (following Duflo, Glennerster, and Kremer 2008). Sample size varies due to missing values.

IV. Discussion

We now focus on discussing our main findings by first analyzing whether the results are consistent with an occupational choice model, and then we attempt to shed some light on the puzzling result of the long-run increase of wage employment.

Occupational Choice Model Predictions.—Within a simple traditional occupational choice model under liquidity constraints, a larger capital transfer should induce more self-employment and self-employment income, which is what we find in the short run: MESP+ has larger effects than MESP.[21] However, in the long run, this prediction does not seem to hold. In fact, although the point estimate of the impact on self-employment of MESP+ is about twice the estimated effect for MESP, the difference is not statistically significant. Moreover, in the long run, only MESP generates income gains, which it accomplishes through wage employment. Even then, the long-run results are not entirely consistent with an occupational choice model, which we investigate in more detail below.

[21] These predictions can be found in models such as Evans and Jovanovic (1989); Lloyd-Ellis and Berhnardt (2000); and Blattman, Fiala, and Martínez (2014), among others.

Table 8—Heterogeneity with Baseline Asset Index, Credit Access and Skills

	Self-employment			Self-employment income		
	(1)	(2)	(3)	(4)	(5)	(6)
MESP	0.066	0.049	0.054	−3.361	−17.94	−16.13
	(0.073)	(0.073)	(0.074)	(44.05)	(43.30)	(44.08)
MESP+	0.119	0.094	0.082	−1.827	−11.52	−10.42
	(0.091)	(0.094)	(0.092)	(38.23)	(43.28)	(43.06)
Interaction of treatment with:						
Asset index and MESP	0.023		0.027	−6.834		−7.147
	(0.024)		(0.023)	(9.734)		(9.555)
Asset index and MESP+	−0.053		−0.040	−14.89		−13.93
	(0.026)		(0.024)	(10.82)		(10.59)
Credit restriction and MESP		0.025	0.030		27.23	26.14
		(0.077)	(0.077)		(23.05)	(23.88)
Credit restriction and MESP+		0.056	0.061		18.91	17.43
		(0.052)	(0.052)		(19.60)	(19.47)
Observations	3,052	3,052	3,047	3,066	3,066	3,061

Notes: Columns 1 to 6 report the intent-to-treat (ITT) estimates and standard errors (in parentheses) of program assignment at each end point on self-employment and self-employment income using pooled data of the two end line surveys. In columns 3 and 6, we use a skills index as a covariate. The asset index and skills index were constructed using principal component analysis. Credit restriction is a dummy variable that takes the value of 1 if the person does not have access to credit (has been rejected or did not request credit because he/she thought that the bank would not grant the credit). Regressions include dummies for strata (defined by a socioeconomic index computed by the government using the Social Security Card score and municipality of residence), the baseline value of the outcome variable, a dummy variable for the 2013 period, and an interaction between the year dummy and the treatment indicators. Regressions are weighted to account for different selection probabilities in each stratum (following Duflo, Glennerster, and Kremer 2008). Standard errors are calculated allowing for clustering at a municipality level. Sample size varies due to missing values.

To examine results within the presence of liquidity constraints, we test whether liquidity-constrained individuals are more likely to become entrepreneurs when receiving the transfer and whether they have higher self-employment income under MESP and MESP+. As in Blattman, Fiala, and Martínez (2014), we analyze the existence of treatment heterogeneity by interacting assignment to treatment with a proxy of wealth (asset index) and a variable measuring credit constraints (Table 8). In general, the coefficients show the expected signs. Those individuals with fewer assets have greater self-employment and self-employment income, and the coefficient is larger and significant for MESP+. Also, those individuals with active credit constraints also have greater self-employment, though the coefficients of the interactions are not statistically significant.

Finally, a simple occupational choice model would predict that when the opportunity cost of self-employment increases (i.e., the control group wage increases), self-employment should decrease. This effect should be at least as large for MESP as for MESP+, if the additional transfer of MESP+ had already increased self-employment in 2011. In fact, we find that wage employment increases under MESP in 2013, but not under MESP+. This is consistent with the opportunity cost prediction of the model, but since we cannot exogenously vary the income from wage employment, this interpretation can only be treated as speculative.

Table 9—Transition Matrix (2011–2013)

	2013		
	Self-employed (%)	Wage employed (%)	Unemployed (%)
2011			
Panel A. Control group			
Self-employed	57.4	24.3	18.4
Wage employed	17.9	51.2	31
Unemployed	27.6	22.4	50
Panel B. MESP			
Self-employed	59.3	21.8	19
Wage employed	11.8	59.2	28.9
Unemployed	14.9	40.6	44.6
Panel C. MESP+			
Self-employed	57.5	18.7	23.7
Wage employed	13.8	61.5	24.6
Unemployed	24.7	30.9	44.4

Notes: Panels A, B, and C present labor transitions from 2011 to 2013 for the control, MESP, and MESP+ groups. Percentages were computed considering only those people with a single labor status in 2011 and 2013. We also omit individuals with missing labor status missing in either 2011 or 2013.

Long-Run Increase in Wage Employment Hypothesis.—In the long run, in 2013, we find that wage employment increased under MESP but not under MESP+ (compared to the control group). This is an unexpected result, as MESP was not designed to increase wage employment. We study two hypotheses that might be consistent with this finding. First, individuals could have used self-employment to increase their future employment prospects as wage earners, which we call the "stepping stone" hypothesis. Second, the training provided by the program could affect "soft-skills," or general working skills that are also valuable in the wage sector.[22]

To Study the "Stepping Stone" Hypothesis, *We Compute Occupational Transitions between 2011 and 2013.*—If this hypothesis is correct, we should observe transitions from self-employment to wage employment more often in MESP and MESP+ than in the control group. However, we note that the self-employment to wage employment transition is higher in the control group (24.3 percent) than in the MESP group (21.8 percent) or the MESP+ group (18.7 percent), demonstrating no evidence for this hypothesis (Table 9).[23]

In parallel, the transition matrices do show a significant higher probability of moving from unemployment in 2011 to wage employment in 2013 for the MESP group compared to the MESP+ group, 40.6 versus 30.9 percent, respectively (Table 9), driving the overall long-run wage employment effect. This evidence shows that the

[22] It is also possible that as the program generates self-employment, the additional income stream from participation could allow individuals to pay for some fixed costs of entry into wage employment (such as clothing, transport costs, etc.). Unfortunately, we do not have data to test this hypothesis.

[23] Also under this hypothesis, individuals might learn new skills while self-employed, and thereby become more hirable. We coded occupation (at three digits) with the survey data (e.g., hairdresser), and find evidence in the opposite direction: overall, 43 percent, 41 percent, and 37 percent of individuals in the control, MESP and MESP+ group remain in the same occupation across surveys. Among the self-employed in 2011 and wage-employed in 2013, these figures are 36 percent, 29 percent, and 32 percent, respectively. See online Appendix 6.2.

program may also be useful in the long run to exit unemployment without necessarily stepping into self-employment.

To examine the "soft skills" hypothesis, we tested whether the program had an effect on self-esteem or empowerment, finding no effects on those variables (see online Appendix 6.3).

V. Conclusions

We find that training and a US$300–420 asset transfer have positive effects on employment and earnings, with long-run impacts smaller than short-run impacts. Positive impacts are observed in both self-employment and wage employment. MESP+ has stronger effects on self-employment, whereas MESP has stronger effects on wage employment. Overall, the positive effects are in line with recent literature on the "big push" (Banerjee et al. 2015; Blattman, Fiala and Martínez 2014; and Blattman et al. 2016), where interventions targeted to very poor individuals in seven low income countries included at least asset transfers and training, and demonstrated positive effects on income generation even at three years post-implementation. In addition, while the literature suggests that asset transfers are usually less successful for females (de Mel, McKenzie, and Woodruff 2009a; and Fafchamps et al. 2014), we find positive impacts in a sample consisting almost entirely of women, suggesting these programs may be useful for them as well.

We add to previous literature by studying the effects of a micro-entrepreneurship program in a middle-income country with decreasing unemployment. A related case in the literature is the anti-poverty program evaluation in South India by Bauchet, Morduch, and Ravi (2015), where they find that a tight labor market competes with self-employment activities, making the program seemingly ineffective. Our results show the importance of studying self-employment programs in a diverse set of labor market margins, considering all employment opportunities available.

We also show that the level of asset transfer is a key aspect of the program: too small a transfer can only have short-term effects on self-employment, while larger and/or regular transfers can result in more self-employment for longer periods. The effect of the larger asset transfer on income is less clear, as total labor income is higher for MESP than for MESP+ in the long run. In addition, our impact evaluation considers a large-scale government program, so the broader effectiveness and scalability of this type of interventions seems plausible.

Although beneficial for individuals, it is important to compare these results to program costs in order to guide policies. The cost-benefit analysis of the program can be computed by comparing the total labor income increase to the program's direct costs. According to the implementing agency's figures, the direct cost per participant is US$1,200 for MESP and US$1,440 for MESP+ (including both training and the asset transfers). Considering a complete fade out of labor income gain from 2013 to 2014, MESP has a net present value of US$292, while MESP+ has a net present value of US$82, using a 5 percent annual discount rate (online Appendix 9). The benefit-to-cost ratios are 1.24 and 1.06 respectively, which are on the low side of the ratios found by Banerjee et al. (2015), which range from −1.98 to 4.3.

Further research could attempt to separate training effects from asset transfer effects, as the literature is scarce in this area. More information about these 27 program components on their own would offer a better understanding of the mechanisms through which individuals benefit from such programs when facing the choice of self-employment versus wage employment. Our results also complement the literature on active labor market program evaluations, where meta-analyses have suggested that some positive impacts emerge only in the long run, particularly those focused on human capital formation (Kluve 2016 for Latin America, and Card, Kluve, and Weber 2015, worldwide). Understanding the training interventions that vulnerable individuals require to perform better in the labor market as a whole remain a challenge.

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