Work pays: different benefits of a workfare program in Colombia

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Abstract

We analyze the impact of a Colombian workfare program *Empleo en Acción*. We find that the program increased individual labor income and labor supply. It also had a positive and significant impact on consumption and particularly for food in small municipalities. The program did not crowd out labor supply in the household and, in rural areas; its effects persisted beyond its operation. We examine the mechanisms that may explain these impacts.

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1. Introduction

Offering unemployment insurance in the context of economies with high levels of informality is challenging because employment status is hard to verify. Workfare programs may provide a solution to this problem by requesting work in return for low pay. In this way, individuals working in better-paid work would be discouraged from claiming, thus improving the targeting of the program. (Ravallion, 1991; Besley and Coate, 1992; Zimmermann, 2014).

As pointed out by Ravallion (1991), workfare programs potentially have two different benefits: a transfer benefit, and a stabilization benefit. The transfer benefit is measured by the net amount of resources that an individual receives from the program. The stabilization or risk reducing benefit emerges because participation in the program can contribute towards consumption smoothing when individuals get unemployed or are hit by another type of adverse shock such as adverse weather conditions or crop loss (Zimmerman, 2015).[1]

The existing empirical literature has documented large short-term transfer benefits in workfare programs of India and Argentina, measured by the gains in income while individuals participate in the program (Datt and Ravallion (1994), Jalan and Ravallion (2003), Ravallion et al. (2005)). A growing empirical literature on India's massive public work scheme (MNREGA) identifies positive impacts on wages and households labor income (see e.g. Imbert and Papp (2015) or Azam (2012)).

Recent empirical studies have also found positive impacts of the scheme on households' consumption in rural areas of some states, with higher impact on food consumption (Deininger and Liu, 2013 and Ravi and Engler, 2015, for India's MNREGA; Al-Yriani *et al.* (2015) for Yemen SFD's Labour Intense Public Work (LIWP)). However, these results are for very poor rural economies. It might be the case that better access to formal or informal insurance mechanisms in middle-income economies mitigates the positive impact identified in rural India.

The first contribution of this paper is to document both the transfer and stabilization benefits within the same workfare program, *Job in Action [Empleo en Acción] (EA)* implemented by the Colombian government between 2002 and 2004 in urban and rural municipalities.

In addition to transfer and stabilization benefits, however, workfare programs might have negative unintended effects by crowding out the labor supply of other household members. Thus the second contribution of this paper is to investigate whether *EA* crowded out labor supply of

[1] Unemployment insurance could provide the stabilization benefits that we refer to. However, workers of the informal sector cannot get access to unemployment insurance, partly because they do not contribute, and partly because the public sector cannot identify whether or not they are working.

other adult household members. In the absence of the program, households might offset one member's unemployment shock by increasing their own labor supply, given available opportunities. Moreover, if the workfare pay-rates are not set low enough they may crowd out informal work by the participant. In both cases we will not observe a net increase in household labor supply and instead the workfare program will lead to a misallocation of labor.

In the context of a low-income economy, Datt and Ravallion (1994) find that for one village of the state of Maharashtra in India, men increase work on the farm when women participate in the workfare program that they consider. This is consistent with household members taking up the activities displaced by the workfare program rather than the program crowding out labor effort, and can be related to high rates of involuntary unemployment. However, recent empirical studies have found mixed evidence in this respect for MNREGA. Deininger and Liu (2013), Zimmerman (2012, 2015) do not find significant crowding out effects, while Imbert and Papp (2015)'s results suggest that India's massive workfare did crowd out private labor effort.[2] For Malawi's large public works program (Social Action Fund - MASAF) Beegle *et al.* (2017) find no crowding out. Finally, workfare may also crowd out private transfers and we test whether households stop receiving external transfers because of their participation in *EA*.

One possible motivation for a workfare program, in addition to providing insurance and the opportunity to smooth consumption, is to avoid human capital depreciation that comes from periods of inactivity, to encourage the accumulation of new productive skills that might lead to a shift in the sector of occupation and/or insert beneficiaries in a network of connections that can be useful for their job search. If these effects are at play, workfare programs might have sustainable benefits that last beyond the duration of the program itself. However, little is known empirically about these potential lasting benefits. Ravallion, et al. (2005) considered this important issue by testing whether there are income gains for non-participants who had previously participated in *Trabajar*, a workfare program in Argentina. The authors cannot reject that there are no income gains after participation though they recognize that their test has low power because of their small sample size.[3]

[2] Notice that the potential negative direct effect on labor force participation may results in an increase in wage rates on the casual labor market, hence in positive second order effects.

[3] Testing for this effect is not the main purpose of their paper, but a requisite to interpret the income losses from leaving the project versus staying in the project as the net income gain from participation. They also discuss the importance of the aggregate state of the labor market at end of participation date as a key factor explaining heterogeneous recovery speed from program retrenchment.

The third contribution of this paper is to test whether workfare programs have sustainable effects, beyond their direct impact during the period of participation in the program. The participation in work might prevent the depreciation of human capital and even improve skills, thus enhancing persistently the beneficiaries' labor market opportunities and labor income even after the program finishes. More generally, according to Besley and Coate (1992) workfare programs do not only self-target the poor (i.e. the screening argument), but they can also lead the poor to make better ex ante choices increasing their future earnings abilities and lower their dependence on workfare (i.e. the deterrent argument). We provide a test of sustainability of the benefits shortly after the program ended. With recent findings for a Public Works Program in Côte d'Ivoire by Bertrand et al. (2017), our paper is one of the first to assess this important hypothesis and to report positive results in this dimension.

Ravallion, et al. (2005)'s results considered only urban households. In our case, participants in rural areas participated in tasks that they were not used to, such as construction, offering them possibly new skills and connections to new professional networks. Our larger sample allows us to test whether there is indeed heterogeneity along the rural/urban line and on pre-intervention occupation.

The following section describes the details of *EA* and the data collected. In section 3 we discuss the randomized controlled trial, which is the basis for our analysis. The results are presented and discussed in Section 4, while Section 5 concludes.

2. The program, the data and the participant allocation to the program.

Starting in the mid-1990's, Colombia experienced a lost decade in terms of economic growth, as the real GDP per capita in 2004 was roughly the same as in 1995. In response to the severe recession of the late 1990s and early 2000s, the Colombian government implemented a variety of different welfare programs, including EA, a workfare program whose main objective was to serve as a safety net (DNP (2007)). The program consisted of subsidizing the hiring of non-skilled labor by qualifying public work projects.[4] The nature of the projects ranged from building or repairing roads and other types of infrastructure (health, education, entertainment, sport or cultural venues, and sewage systems). They had to be proposed by local governments, NGOs or other community organizations, which had to cover the non-labor costs of the projects.[5] The maximum duration of each project was five months.

Individuals eligible to participate had to be older than 18, could not be studying during the morning or afternoon, could not be currently employed in a formal job and had to belong to the first or second level of the Colombian Social Classification System (SISBEN)[6]. Eligible individuals could work part-time up to a maximum of five months in an EA project. On average, individuals worked only for 2.4 months in an EA project, probably because pay conditions were worse than in the market[7].

According to government statistics, 3724 projects were approved for funding, 63% of them in municipalities with less than 100,000 inhabitants. Projects were approved between the end of 2000 and March 2003, and started at different times in different municipalities. The last projects funded by the EA program finished in May 2004 (DNP (2007):12). At the start of the program, the government wanted to implement it mainly in large urban areas. However, there was a relatively low demand on the part of the local authorities in these areas (that had to finance the non-labor cost of the projects) and, as a consequence, the government decided, reluctantly, to start the roll-out in small and rural municipalities.

[4] The program paid 2004 US$69 (COL$180,000 Colombian pesos in 2001) a month for each individual working part time (24 hours) per week.
[5] There were some exceptions for projects proposed by local governments.
[6] The Colombian Social Classification System, called SISBEN, is used as an eligibility tool for most social programs in Colombia. There are six possible categories. The first and second one correspond to the poorest in the population.
[7] Workfare programs generally pay worse than in the market to assure that individuals will take normal jobs when available. Individuals could only work part time so that they could look for normal jobs.

This paper uses a sample of 116 randomly selected projects to study the impact of *EA*. Three waves of a longitudinal household survey were collected for each project. The evaluation sample covers both small and large municipalities. The first wave of the evaluation longitudinal panel survey was collected between December 2002 and December 2003. This survey was intended to be a baseline; however, some projects were initiated earlier than originally planned, although no payments were disbursed before the data was collected. We will explain below how our empirical strategy accommodates this issue.

The second wave of data was collected between March 2003 and January 2004, when the projects were still ongoing, with the objective of measuring the impact of the program while the participants have access to it. The third wave was collected between June and September 2004, 4-13 months after the completion of the projects. This third wave is the one that allows us to study the impact of the workfare program once it has finished.

The first wave, included 10947 households. Attrition was moderate and the survey covers 6298 households at the first follow up and *5469* households in the second follow up.

The Randomization and allocation to the program

Before a project started, individuals who were interested in participating had to register their interest. Exploiting the fact that the programs were oversubscribed, the local authorities were asked to choose participants for each project randomly, keeping project specific lists of those randomized in and those not.

The time it took between allocation to the program and its actual start led to some noncompliance, with some treated individuals dropping out and being replaced by individuals originally in the randomized-out list. However, we know who was originally randomized in or not and hence we can carry out an intention to treat analysis.

Finally, when we analyze individual level outcome variables, we exclude from the sample 401 individuals who were living in households who had members in both the list of randomized-in and randomized-out individuals, as one would expect strong intra-household interactions in the behavior of these individuals.[8]

Table 1 shows the relation between those in the randomized in/out list and who participated in EA. In what follows, we refer to municipalities with more than 100,000 inhabitants in major metropolitan areas and big cities as "large" and to municipalities with less than 100,000

[8] We have run our entire analysis without dropping these individuals and obtained very similar effects, both qualitatively and in magnitude, which is a first sign of the absence of crowding out effects.

inhabitants outside major metropolitan areas as "small".[9] As it can be seen, 8% of those randomized out actually participated in the program and 19% of those randomized-in did not.

Table 1. Compliance: First Follow Up.

	Randomized-in (IP=1)	Randomized-out (IP=0)
All municipalities		
Participating in EA (P=1)	2591	162
	(81%)	(8%)
Not Participating in EA (P=0)	594	1902
	(19%)	(92%)
Large municipalities		
Participating in EA (P=1)	1449	89
	(83%)	(8%)
Not Participating in EA (P=0)	287	1035
	(17%)	(92%)
Small municipalities		
Participating in EA (P=1)	1142	73
	(79%)	(8%)
Not Participating in EA (P=0)	307	867
	(21%)	(92%)

3. Identification Strategy

We aim to identify intention-to-treat (ITT) effects, that is, the effect of being randomized-in, which we denote by IP=1. Our identification strategy must consider the possibility that the process of allocating individuals to the randomized-in and out list was possibly compromised.

Tables A1 and A2 in the appendix compare the characteristics of those randomized in vs. out. Table A1 compares basic individual characteristics such as gender, age, education, health indicators, migrant status, training indicators and labor history. Table A2 compares household variables and reveals some small differences, pointing to an excess allocation to treatment of individuals with poorer forms of housing. Though differences are generally not large, some differences are statistically significant.[10] Hence, we cannot rule out the possibility that some unobserved characteristics might be correlated with both the outcome variables and the allocation

[9] This corresponds to the administrative categories of "high priority" (large) and "low priority" (small) municipalities defined for the implementation of *EA*. As mentioned before, the local authority of the "high priority" areas were not too keen in the program to start with, so the actual implementation started in the "low priority" municipalities.
[10] We have also regressed the treatment dummy on similar individual and household characteristics and we reject the joint null hypothesis at a p-value of 0.002, reinforcing our concerns.

to the randomized-in list. We will use difference-in-differences to control for potential imbalance in permanent unobserved characteristics. We thus estimate the following regression model:

$$\Delta y_{ikt} = \alpha IP_{ik} + \beta X_i + \theta_k + \varepsilon_{ikt}, \qquad (1a)$$

$$E[\varepsilon_{ikt} | IP_{ik}, X_i, \theta_k] = 0 \qquad (1b)$$

where $\Delta y_{it} = y_{it} - y_{i0}$ is the difference in the outcome variable y (labor income, hours worked and transfers) for individual *i*, registered in the list of project k between, period t and the reference pre-program period 0.[11] X_i is a vector of individual *i*'s time invariant household and individual characteristics collected at baseline including education, gender, age, socio-economic classification of the neighborhood, household's demographics and assets and whether the household faced some shock since 2000;[12] θ_k is a project fixed effect, which is included because the randomization was within project and allows for differential growth of the outcomes across projects; finally ε_{ikt} is an error term. Equation (1b) states our identification assumption, namely that the unobserved determinants of growth of the outcome variable are mean independent of allocation to treatment conditional on observed characteristics and project identity (which reflects location). Under this assumption, the estimator of α, which we will refer to as diff-in-diff, will provide a consistent estimate of the ITT.

A standard concern with a diff-in-diff estimator in this context is the existence of an Ashenfelter's pre-program dip in earnings among individuals who are treated, as opposed to the comparison group (see Ashenfelter, 1978 and Heckman and Smith, 1999). This is so if the treatment is allocated on the basis of pre-program earnings as in Ashenfelter's original study. However, in our case the selection into treatment was among a population of applicants. The randomization would then eliminate this concern. However, to guard against any potential for the initial conditions to be different due to the possible compromise of the randomization protocol, we use retrospective measures of income and labor supply (y_{ik0}) that refer to 2001,[13] when

[11] The reference year will be 2001 for income and hours worked, and the baseline survey date for consumption and transfers, c.f. infra.

[12] If the regressions are at the household level, then we control for the same household's characteristics plus household head's education, gender, and age.

[13] We could alternatively use values reported for 2000. We have run robustness checks (not reported here) and we did not find significant discrepancies. Values for 2000 and 2001 hours worked are quite similar in mean and variance, income reported for 2000 show however higher standard deviation than 2001 values (as can be seen in Figure 1).

constructing differences of income or labor supply. These were collected retrospectively in the first (baseline) interview. Since the application process took place in 2002, our measure of income and labor supply refers to a period well before the application decision.

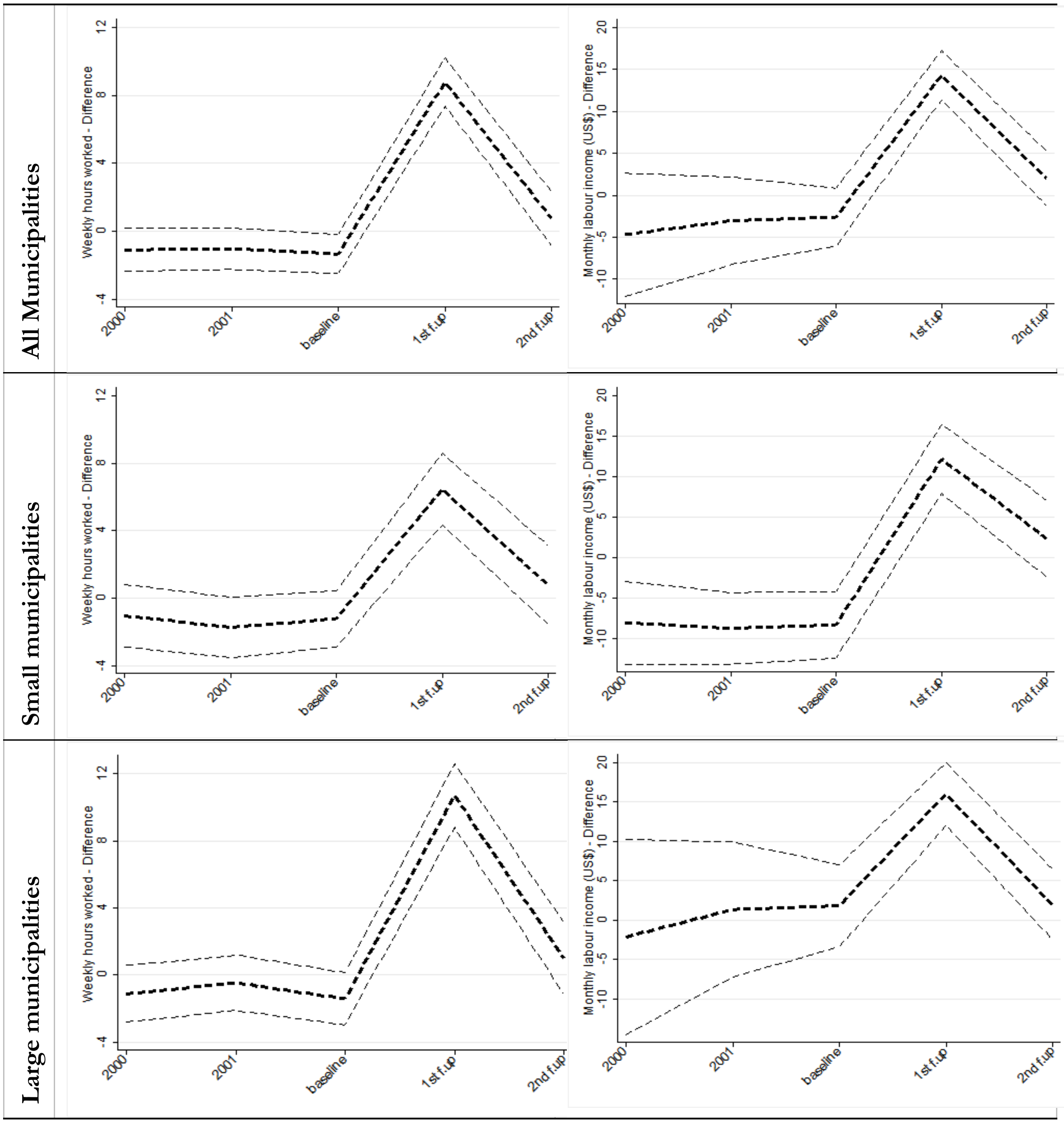
Beside these classical issues related to potential temporal pre-treatment dip, there are two other reasons for using 2001 measures of income and labor supply as the baseline measure y_{ik0}. First, it ensures that y_{ik0} is not affected by expectations of future participation. Second it tackles the problem that some individuals were already working in the EA project when the first wave of data was collected (Dec. 2002-Dec. 2003).

Table 2. Descriptive statistics on large and small municipalities.

Mean (S.d.)	*Large municipalities*	*Small municipalities*
Population in 2004 (1000)	628	33
	(1499)	(35)
Number of projects	35	7
	(46)	(6)
Number projects for 100,000 habitants	16	34
	(22)	(38)
Expenses by project (2004 US$)	19334	23813
	(6559)	(6981)
Expenses by habitant (2004 US$)	4	9
	(6)	(11)
Gini index (2005)	38	44
	(23)	(8)
Poverty rate (2005)	11	52
	(10)	(22)
Rural index (2004)	38	67
	(17)	(15)
Applicants characteristics		
Age	35.4	35.12
	(12.8)	(12.4)
Female	0.45	0.26
N	3239	2532

Note: Gini index, rural index (rural population/population) and poverty rate (poverty head count index based on Multidimensional Poverty Index) are from the Municipal Panel Data CEDE, an initiative of the Center of Economic Development Studies (CEDE for its acronym in Spanish) website. Occupation: Recall during the second follow-up on the main occupation three months before baseline. S.D. sample standard deviation.

Figure 1. Mean individual weekly hours per week and individual monthly labor income (US$). Difference between randomized-in and -out samples for each survey wave and past values.



Note: Thin lines are 95% C.I. bounds; weekly hours worked on LHS, monthly income on RHS

As reported in Table 2, twice as many projects per inhabitants were initiated in small municipalities, relative to the large ones. Expenditure per project was 23% higher in small municipalities, with US$9 per capita versus US$4 per capita in large ones. Small municipalities are mostly rural areas, where poverty is more prevalent and inequality more pronounced. Moreover, applicants to *EA* differed between small and large municipalities with significantly more females and lower educated individuals in the smaller ones. Finally, applicants to *EA* in small municipalities were more likely to be farm workers and less likely to be unemployed (Table 3). Given these differences in both population composition and treatment intensity, we present separate estimates for large and small municipalities.

Figure 1 illustrates the absence of divergent pre-treatment trends although there are some level differences in weekly hours worked and labor income over the period 2000, 2001 and the baseline date. However, these differences between randomized-in and out individuals remain constant over the pre-program period (parallel trends). We also note the absence of any differential pre-program dip in earnings or hours between treatment and control.

Table 3 reports the results of testing for common pre-treatment trends over the pre-program period by regressing of the growth of monthly labor income and weekly hours worked between 2000 and 2001 on the indicator of allocation to treatment ($IP = 1/0$). The fact that none of these coefficients is significant is further support for our identification strategy.

Table 3. Common trend assumption in the pre-program period - between 2000 and 2001.

	Without additional controls			With additional controls		
Dependent variable	All	Small towns	Large Towns	All	Small towns	Large Towns
Weekly hours worked	-0.398	-0.765	-0.0856	-0.300	-0.791	0.206
	(0.480)	(0.653)	(0.693)	(0.498)	(0.672)	(0.735)
N	5615	2453	3162	5439	2397	3042
Monthly labor income	-0.0340	-0.787	0.600	0.601	-0.773	2.229
(US$)	(3.275)	(1.898)	(5.826)	(3.179)	(1.808)	(5.903)
N	5586	2428	3158	5409	2371	3038

Note: Each cell reports the estimate on *IP* of a regression of the change in the dependent variable between 2001 and 2000. The regressions of the estimates reported in the last three columns also include the following control variables are education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities, shocks). Robust standard errors in brackets.

Identifying impacts on consumption

An advantage of our data is that it contains information on consumption, so that, in principle, we can estimate the impact of the program on a variable that is directly related to utility and that is less likely to be affected by short run fluctuations in income. Unlike income and labor supply, we lack retrospective information on consumption for 2000 and 2001, and hence we can only use consumption collected at baseline for the difference-in-differences regression. However, 72 of 116 projects had already started at the time of the baseline data collection and individuals knew their allocation to treatment, allowing them to increase consumption and compromising the estimation of the program effect.

Having said that, the effects of the program on consumption are useful and we thus take several approaches. First, we estimate the standard diff-in-diff specification using baseline consumption, with the caveat that our estimates might underestimate the true effect. Second, we also estimate (1) on the sub-sample of projects that had not started at the baseline survey.

Inference

Throughout the analysis, we compute robust standard errors. P-values are adjusted for multiple hypotheses testing following the Romano-Wolf (2008) stepdown procedure. We consider one first set of four hypotheses corresponding to the four outcomes (income and hours worked during and after participation) of interest for the population as a whole, and a second set of eight hypotheses corresponding to the four outcomes of interest, splitting the sample in small and large municipalities.

4. Results

We first describe the effect on income and hours worked at the individual and household level while the intervention is on-going. We then estimate these effects 6 months after the intervention ended. In a separate section, we check if these impacts are reflected in an increase in consumption per capita. Finally, we shed light on potential channels explaining the observed long lasting impacts.

4.1 By how much did EA increased participating individuals' labor effort and income?

We assess whether *EA* led to an increase in income and hours of work for participants while the projects were on-going.[14]

The first two columns Table 4 refer to the ITT effect of the program at the individual in the top panel and at the household level in the lower one, while the projects were still on-going (1st follow up). The covariates we control for (gender, age, education, migration status, as well as

[14] Here we do not take into account participation costs of the individual or any other benefits of EA, such as increases in productivity and welfare due to public works output. In the case of MNREGA, Imbert and Papp (2015) and Azam (2012) do find such second orders positive impacts of the program, in particular on private labor market wage rates.

household level variables) have almost no effect on the results, which is consistent with our finding on pre-treatment common trends.[15]

The increase in hours work and labor income is positive, quite large, and statistically significant: around 10 more hours per week for randomized-in individuals (compared to 25 weekly hours of work on average for randomized-out individuals) and around 19 more US$ earned per month (compared to monthly labor income of US$50 in the group of randomized-out individuals). In Table 5, we estimate the effect by the size of the municipality and find very similar estimates in small and large municipalities (columns 1 and 3).

One salient criticism of workfare programs is that they may crowd out other work effort, possibly because these jobs may have been designed "too generously" and incentivize households to reduce their labor effort on the private labor market. Indeed Imbert and Papp (2015) do find MRNEGA public work crowds out private work, in contrast to the results by Deininger and Liu (2013) and Zimmerman (2012) who find no evidence of crowding out, or by Rosas and Sabarwal (2016) who report evidence of crowding *in* effect.

From the lower panel of Table 4, we see that the program did not crowd out activities of other household members. Indeed they increase their hours of work by just over three hours and monthly labor income also goes up by $8.49 and both these effects are significant at 5% and 7% respectively. Hence, the program has a positive spillover into other household members leading to substantial increases in household income while the program is in operation. Finally, we estimated the effect of the program on transfers during its operation (first follow up). The impact is effectively zero (impact on net transfers is -0.58, st. error 1.09).

The estimate of the effect of offering treatment on actual program participation ($E[P_i|IP_i = 1, X] - E[P_i|IP_i = 0, X]$)[16] is 0.74 (.78 and .70 for respectively large and small municipalities). Dividing the ITT by this number implies an effect on earnings of EA of US$26 (s.e. 3.2) per month, which represents 38% of the Empleo statutory monthly wage rate (69 US$). This is lower than the impact found in Jalan and Ravallion (2003) and Ravallion et al. (2005) for

[15] These are socio-economic classification of the neighborhood ("estrato"), household size, number of kids and adults, durable goods, dwelling characteristics, household head gender and age, household benefits in program "Familias en Acción", homeownership status, and whether the households suffered shocks over the past 2 years (violence, fire, loss, job loss, illness, death).

[16] Monotonicity holds in the sense that $E[P_i|IP_i = 0] \leq E[P_i|IP_i = 1]\ \forall i$, and independence if $\left(\Delta Y_i^{P=0}, \Delta Y_i^{P=1}, P_i|IP_i = 0, P_i|IP_i = 1\right)$ is independent of IP_i. On the later identification assumption, one may argue that the program may lower competition among involuntary unemployed casual workers, hence positively impacting non-treated individuals, which would lead to an upward biased estimate of the LATE. This is however probably not the case since *EA* was framed in a way that participants could still look for a job while participating, hence keep competing with non-participants.

Trabajar (around 50% of the *Trabajar* statutory wage) and also lower than Galasso and Ravallion (2004) results on *Jefes* (about two third of the program statutory wage). These differences might be partly explained by the fact that 25% of the Empleo participants were already off the program at the first follow up, which may lead to lower impact if some became unemployed after their participation in the program ended.[17] A similar exercise for the impact on hours worked per week gives an estimated LATE of 13 hours per week (s.e. 1.2), which is higher than the preferred estimate in Galasso and Ravallion (2004) for *Jefes*, (9h for a work requirement of 20h for *Jefes* compared to 17h for a work requirement of 24h for *EA*).

Table 4. Diff-in-diff estimates of the ITT effect on individuals and households' outcomes in first (short) and second (long term) follow up.

	Dependent variable	First-Follow-up		Second Follow-up	
Individuals' outcomes	**Weekly hours worked**	9.68***	9.89***	1.61	1.60
		(0.93)	(0.92)	(1.02)	(1.00)
	p-value	<0.01	<0.01	0.24	0.53
	N	4918		*4213*	
	Mean (IP=0)	*24.68*		*24.48*	
	Monthly labor income (US$)	19.47***	19.10***	4.81	4.48
		(2.53)	(2.37)	(2.79)	(2.66)
	p-values	<0.01	<0.01	0.24	0.49
	N	4865		*4201*	
	Mean (IP=0)	*49.68*		*49.95*	
Other household members' outcomes	**Weekly hours worked**	3.26*	3.02	1.27	5.16
		(1.54)	(1.52)	(1.86)	(4.93)
	p-values	0.05	0.11	0.45	0.30
	N	3574	3574	3058	3046
	Mean (IP=0)	*133.24*		*133.90*	
	Monthly labor income (US$)	8.49*	8.90	1.76	4.85
		(4.88)	(4.99)	(1.88)	(4.80)
	p-values	0.07	0.11	0.45	0.30
	N	3456		3046	
	Mean (IP=0)	*63.31*		*63.08*	
Additional controls		No	Yes	No	Yes

Note: Each cell reports the estimate on *IP* of a regression of the change in the dependent variable between the first follow-up and 2001. The bottom panel refers to household members' other than the study individuals. The regressions of the estimates reported in the last three columns also include the following control variables: education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities, shocks). *** p<0.01, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis. Romano-Wolf adjusted p-values: the 4 hypotheses in each column are tested jointly.

We next consider whether the effects of the program lasted beyond its operation. This could happen if participants acquired new skills through working in *EA* projects, or if their work networks improved. Such a possibility could substantially alter the cost/benefit ratio since public

[17] Because each participant could only participate in EA for a limited time, some individuals finished their participation even though the projects were still on-going.

works can be an expensive way of transferring to the poor, relative to say unconditional transfers (see Murgay *et al.* (2016) and Alik-Lagrange and Ravallion (2015), Bertrand *et al.* (2017)). Long-term effects on the beneficiaries, as well as positive effects of the projects themselves (to the extent that they would not have happened otherwise) may be key to the impacts of the program.

Table 4 and 5 report the estimated impacts on hours of work and income, using data from the second follow up, which was collected 4 to 13 months after the end of the projects. Although the impact is not statistically significant when we pool the data from small and large municipalities (Table 4, third and fourth columns), the estimates in Table 5 imply that the program increased the income and hours of participants from small municipalities (second column) but not that of participants from large municipalities (fourth column).[18] Hence, the program increased the long-term labor market impacts in small municipalities but mainly served as a way of targeting welfare benefits in large municipalities. Below, we provide suggestive evidence to explain this result.

To summarize, the program has strong hours and income effects while it is operating. These benefits outlast the program in small municipalities. We now move on to examine the effects on consumption, which is a better indicator of standard of living.

4.2 Consumption benefits of the intervention

The increase in income and hours of work that we have documented so far may be reflected in increases in consumption for two main reasons. First, if households have had a negative shock and they do not have own assets or other mechanisms of insurance or consumption smoothing at their disposal, they will spend the *EA* income. Second, to the extent

[18] A possibility to consider is that projects started later in the small municipalities. We show in Table A4 that this was not the case and that small municipalities' participants actually stopped participating earlier in the past.

Table 5. Effect on individuals and household outcomes in first and second follow up by municipality size.

	Dependent variables	Small Municipalities First Follow-up	Small Municipalities Second Follow-up	Large Municipalities First Follow-up	Large Municipalities Second Follow-up
Individuals' outcomes	**Weekly hours worked**	9.55***	3.58*	10.20***	-0.10
		(1.30)	(1.40)	(1.30)	(1.41)
	p-value	<0.01	0.06	<0.01	0.99
	N	2238	1860	2680	2352
	Control Mean	27.55	27.07	22.33	22.32
	Monthly labor income (US$)	19.21***	11.49***	19.00***	-1.64
		(2.73)	(3.07)	(3.76)	(4.21)
	p-values	<0.01	<0.01	<0.01	0.99
	N	2216	1846	2649	2354
	Control Mean	52.98	52.95	46.99	47.50
Other household members' outcomes	**Weekly hours worked**	2.17	2.27	3.67	7.35
		(2.22)	(4.98)	(2.08)	(7.81)
	p-values	0.46	0.99	0.46	0.94
	N	1483	1230	2091	1816
	Control Mean	120.94	120.09	141.96	143.74
	Monthly labor income (US$)	2.90	3.98	13.46	6.16
		(6.64)	(5.16)	(7.20)	(7.76)
	p-values	0.46	0.99	0.65	0.98
	N	1449	1230	2007	1816
	Control Mean	65.95	63.45	63.14	62.82
	Additional controls	Yes	Yes	Yes	Yes

Note: Each cell reports the estimate on IP of a regression of the change in the dependent variable between the first follow-up and 2001. The bottom panel refers to household members' other than the study individuals. The regressions of the estimates reported in the last three columns also include the following control variables: education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities, shocks). *** p<0.01, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis. Romano-Wolf adjusted p-values: the 8 hypotheses of each panel respectively are tested jointly.

that workfare leads to further permanent labor market opportunities (say because of newly acquired networks) the increase in income may represent a permanent change, which can increase consumption. On the other hand, if workfare provides an easy earnings opportunity for otherwise inactive members of the household, it will act as a transitory increase in income and assets, rather than consumption.

In Table 6, we show the results. These are estimated by taking the difference between the first-follow up and the baseline survey (with the caveat that some of the projects had already started by the time the baseline was collected, which might lead us to underestimate the effect). Overall there is no effect on consumption. However, when we break it down by municipality we find a 5% increase in overall consumption and 10% for food, while the program is in operation but not in the second follow up. We find similar results (if anything, larger) when we restrict the sample to projects that had not started at baseline (see Table A3). Remembering that income increased in both large and small municipalities when the program was in operation, the interpretation is that households in large municipalities were not liquidity constrained and saved the extra program income. Households in the small ones seem to be constrained and use the increased income for consumption.

Interestingly, the positive impacts on consumption are in the range of those found for the impact of MNREGA on rural households' consumption. For the state of Andhra Pradesh, Deininger and Liu (2013) find an increase in consumption of 7%, going up to 13% and 11% when focusing on protein and energy intakes. Following a similar identification strategy, Ravi and Engler (2015) find a similar pattern (+9.6% on food expenditure, but no significant impact on total consumption).

When comparing these impacts on consumption with those identified on income, they are significantly smaller. In the second follow-up survey, ex-participants were asked how they used the extra income earned on EA. 85% of the ex-participants interviewed used *EA* income to buy food, clothes, and other consumption goods or invest it in education. Interestingly 44% of ex-participants report to have used *EA* income to repay debt. This is consistent with theoretical findings of Chau and Basu (2003) who describe the potential positive impact of public work program on debt-bondage in poor rural economies and is of course consistent with the idea that transitory income is saved rather than (fully) consumed. It is also consistent with empirical evidence of reduced levels of indebtedness found by Al-Yriani *et al.* (2015) for

Yemen's LIWP. Of course, some of it is consumed, reflecting the heterogeneous circumstances of the households.

Table 6. Diff-in-Diff estimates of the ITT effect on household's consumption.

Municipality size	All		Small		Large	
Dependent variable	First Follow-up	Second Follow-up	First Follow-up	Second Follow-up	First Follow-up	Second Follow-up
log consumption	0.01	0.01	0.05*	0.02	-0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
pvalues	0.56	0.81	0.07	0.87	0.25	0.87
N	*3853*	*3063*	*1687*	*1328*	*2166*	*1735*
log food consumption	0.02	-0.01	0.10***	0.03	-0.05	-0.05
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
pvalues	0.56	0.81	<0.01	0.77	0.19	0.53
N	*4580*	*3965*	*2085*	*1744*	*2495*	*2221*
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Each cell reports the estimate on *IP* of a regression of the change in the dependent variable between the first follow-up and 2001. The regressions of the estimates reported in the last three columns also include the following control variables: education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities, shocks). *** p<0.01, ** p<0.05, * p<0.1. Robust Standard errors in parenthesis. Romano-Wolf adjusted p-values, of the the 2 hypotheses in column "All" for 1st and 2nd follow up respectively are tested jointly, the 4 hypotheses of the columns "Small towns" and "Large towns" for 1st and 2nd follow up respectively are tested jointly.

4.3 Potential channels to explain the long-lasting impacts

Above we reported that, even after *EA* ended, the income and hours of work of participants had improved in small but not in large municipalities. Four to thirteen months after participation, ex-participants were asked why *EA* had made it easier for them to find a job (Table A.5). Participants (and especially men) living in small municipalities are more likely than participants living in large municipalities to give responses associated with skills enhancements, such as gaining work experience (47% vs. 40%) or learning a new job (15% to 7%)). This is probably because a high share of the labor force in small municipalities was working on farming before *EA*, while the work offered on the *EA* projects was mostly related to the construction industry. Hence, for many beneficiaries living in small municipalities participating in *EA* meant learning new skills related to the construction industry. In large municipalities, where there was no long-term effect, possibly because construction activities were less novel to them and hence they did not acquire new skills as a result of their participation in *EA*.[19,20] This is reflected in

[19] Indeed, when asked to assess why EA had made it easier to find a job, ex-participants living in large municipalities gave answers such as self-confidence gains and "getting in contact with someone to help them to find a job" rather than skill gains

Table 7 which reports the impact of the program on transitions to occupations in the second follow up. Although the estimates are not very precisely estimated, there is a significant move from unemployment and out of the labor force into construction in small municipalities.[21] For large municipalities there is not much to report.

Table 7. Transition matrix from pre-baseline occupation to second follow up.

SECOND FOLLOW UP OCCUPATION	PRE BASELINE LABOUR OCCUPATION			
	Farming	Construction	Self-Employment /Any other	Unemployed or out of labor force
Small municipalities				
N (obs. pre-baseline for this occupation)	*99*	*55*	*918*	*776*
Farming	-.128	-.048	.013	.013
	(.094)	(.095)	(.011)	(.015)
Construction	.065*	-.192	-.002	.043**
	(.032)	(.148)	(.009)	(.012)
Self-Employment /Any other	-.001	.171	-.007	-.013
	(.090)	(.127)	(.023)	(.036)
Unemployed or out of labor force	.065*	.069	-.004	-.040
	(.032)	(.122)	(.019)	(.036)
Large municipalities				
N (obs. pre-baseline for this occupation)	*28*	*97*	*878*	*1431*
Farming	.050	.000	.008	.001
	(.191)	(.000)	(.005)	(.005)
Construction	.100	-.209	.016	.003
	(.070)	(.107)	(.008)	(.012)
Self-Employment /Any other	.025	.104	-.011	.043
	(.211)	(.098)	(.028)	(.027)
Unemployed or out of labor force	-.175	.105	-.013	-.047*
	(.200)	(.090)	(.027)	(.027)

Note: Coefficients from independent linear probability models regressions models $1(O_t|O_{t-1}) = \alpha + \beta.(IP|O_{t-1}) + \varepsilon, E[\varepsilon|IP, O_{t-1}] = 0$. For example, in small municipalities individuals previously in farming have a 12.8 percentage points less chance to end up in farming if they are randomized in. *** p<0.01, ** p<0.05, * p<0.1. Robust s.e. in brackets. P-values not adjusted for multiple hypotheses testing.

[20] We observe similar shares of participants reporting that it has been easier to find a job thanks to *EA*, which contrasts with reported objective success on the labor market. This over-optimistic view on the state of the labor market for ex-participants has been documented in the case of MNREGA in Dutta et al. (2013).

[21] Because of the reduced sample size for each occupation, we do not adjust for multiple hypotheses testing in Table 7. Hence, the results should be taken as suggestive.

5. Conclusions

Workfare programs provide a low paid employment guarantee to individuals in selected public works. They are designed to self-select the poor and provide insurance against job losses by informal sector workers at the possible cost of crowding out private labor effort. We analyze the impact of a Colombian workfare program called Job in Action [Empleo en Acción] to shed light on the following issues.

The key results are that the program itself significantly and substantially raised the hours of work and the earnings of the participants. It also led to income increases and work effort for other household members. In other words the program does not replace other activities that would have happened in its absence, exhibits positive intra-household spillovers, and genuinely increases household income as intended. The effects are similar in large and small municipalities, but the effects last beyond the duration of the program only in small municipalities. We find suggestive evidence that these benefits outlast the program are due to new acquired skills, especially those related to the construction industry, which was the main activity of *EA* projects.

We also find that consumption increases in small municipalities only and by less than the increase in income: households seem to save at least part of the income accrued from the program. In large municipalities, there was no increase in consumption, which is consistent with households not being substantially liquidity constrained.

Overall the program successfully increased the income of the beneficiaries. Whether it justifies its cost is a very hard question. The program improved the participants' long-term prospects in small municipalities but not in large ones. A complete evaluation would have to take into account the value of the public programs themselves and whether they would have happened anyway in the absence of the program. Finally, the open question is whether these workfare programs offer genuine insurance value. The fact that the program increased consumption and did not displace market labor supply points to real value as an insurance mechanism.

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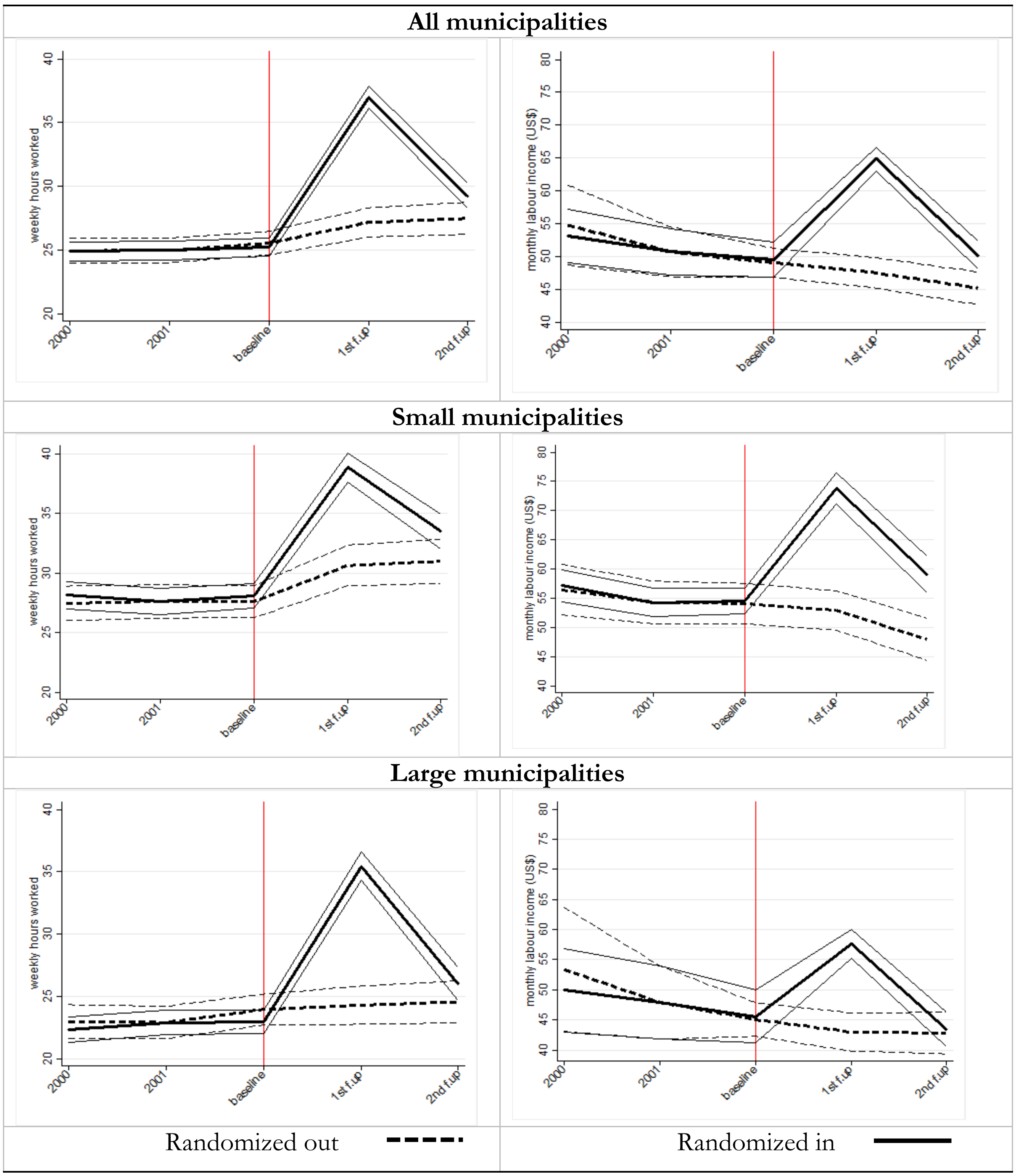
7. Appendix

Additional figures

Figure A.2. Mean individual weekly hours per week and individual monthly labor income (US$) in randomized-in and -out samples for each survey wave and past values in Difference in Difference (reference date 2001)



Note: Thin lines are 95% C.I. bounds; weekly hours worked on LHS, monthly income on RHS.

Additional tables

Table A. 1. Difference in the characteristics of individual initially allocated to participate in *EA* and those not.

	Municipalities size	All	Large	Small
Sex (1=Female)		-0.0445**	-0.0850**	0.00268
		[0.0124]	[0.0180]	[0.0167]
Age		-0.366	-0.298	-0.446
		[0.361]	[0.509]	[0.510]
Illness	Any health problem in the last 2 weeks	-0.0452**	-0.0425**	-0.0484**
		[0.0100]	[0.0139]	[0.0144]
	Had to stay in bed in the last 2 weeks	-0.0288**	-0.0269**	-0.0310**
		[0.00747]	[0.0103]	[0.0109]
	Had to stay in hospital in the last 12 months	-0.00846	0.00263	-0.0214+
		[0.00743]	[0.00998]	[0.0111]
Migrant		0.0125	0.0072	0.0188
		[0.0127]	[0.0178]	[0.0181]
Education	No studies	0.00377	0.0132	-0.00722
		[0.00860]	[0.0106]	[0.0140]
	Primary incomplete	0.0191	0.0247	0.0124
		[0.0130]	[0.0172]	[0.0196]
	Primary complete	-0.019	-0.0405*	0.00612
		[0.0118]	[0.0166]	[0.0168]
	Secondary incomplete	0.00291	-0.000339	0.0067
		[0.0122]	[0.0175]	[0.0168]
	Secondary complete	-0.000844	0.00897	-0.0123
		[0.00988]	[0.0133]	[0.0147]
	More than secondary complete	-0.00598+	-0.0061	-0.00583
		[0.00353]	[0.00465]	[0.00538]
	Has done a training course	-0.0246*	-0.0139	-0.0370*
		[0.0105]	[0.0149]	[0.0148]
Work	Has done paid work in the last 20 years	0.00551	0.0233**	-0.0153*
		[0.00523]	[0.00706]	[0.00775]
	Has done paid work during at least a month in 2001	-0.00336	0.0192	-0.0300+
		[0.0124]	[0.0179]	[0.0170]
	Has done paid work during at least a month in 2000	-0.00773	0.0108	-0.0296+
		[0.0129]	[0.0184]	[0.0179]
	Number of months worked during 2001	-0.363*	0.0808	-0.889**
		[0.145]	[0.201]	[0.209]
	Number of months worked during 2000	-0.320*	0.063	-0.772**
		[0.149]	[0.205]	[0.215]
	Number of hours a week worked during 2001	-1.268+	-0.257	-2.463**
		[0.654]	[0.907]	[0.942]
	Number of hours a week worked during 2000	-0.855	-0.146	-1.689+
		[0.676]	[0.936]	[0.977]
	Monthly individual labor revenue in 2001 (in Dec 2003 pesos)	-2.14	4.383	-9.891**
		[2.684]	[4.507]	[2.414]
	Monthly individual labor revenue in 2000 (in Dec 2003 pesos)	-2.186	3.857	-9.351**
		[3.382]	[5.745]	[2.878]
	Observations	5724	3218	2505

Note: ** p<0.01, * p<0.05, + p<0.1. Robust Standard errors in brackets.

Table A.2. Balance of household characteristics between those that initially intended to participate and not (beneficiaries) – Difference

		Municipalities size	All		Large		Small	
			Difference	s.e.	Difference	s.e.	Difference	s.e.
Household composition	Number of people…	In the household	-0.086	[0.0741]	-0.058	[0.103]	-0.120	[0.106]
		Younger than 7 years old	0.002	[0.0309]	-0.001	[0.0424]	0.005	[0.0451]
		Between 7 and 18 years old	-0.038	[0.0391]	-0.047	[0.0533]	-0.028	[0.0576]
		Older than 18	-0.050	[0.0438]	-0.010	[0.0623]	-0.097	[0.0611]
Housing conditions	Housing is a house		-0.0248**	[0.00894]	-0.0508**	[0.0135]	0.005	[0.0112]
	1= if housing has	Tile flooring	-0.0195+	[0.0103]	-0.004	[0.0147]	-0.0379**	[0.0142]
		Wood flooring	0.003	[0.00438]	-0.006	[0.00607]	0.0129*	[0.00631]
		Conglomerate floor tiles	0.014	[0.0133]	0.026	[0.0184]	-0.001	[0.0192]
		Earthen flooring	0.003	[0.00977]	-0.017	[0.0130]	0.0258+	[0.0148]
		A ceiling	-0.002	[0.0105]	0.0139	[0.0156]	-0.0204	[0.0134]
		Sewage system	-0.006	[0.00902]	0.0187	[0.0123]	-0.0343**	[0.0132]
		A toilet connected to housing	0.007	[0.00960]	0.00663	[0.0120]	0.00726	[0.0153]
		No toilet	-0.005	[0.00786]	-0.00125	[0.00900]	-0.00843	[0.0134]
		A toilet exclusive of household	0.005	[0.0120]	-0.0101	[0.0164]	0.0234	[0.0177]
	1= if walls are made of	Brick	-0.0189+	[0.0112]	0.0105	[0.0147]	-0.0531**	[0.0172]
		Adobe	0.0335**	[0.00910]	0.0206*	[0.00923]	0.0487**	[0.0165]
		Wood	-0.0147+	[0.00750]	-0.0311*	[0.0126]	0.00450	[0.00690]
	1=if housing receives	Water service by pipe	-0.0175*	[0.00803]	0.00394	[0.0107]	-0.0425**	[0.0120]
		Sewage service	-0.010	[0.00751]	0.0193**	[0.00748]	-0.0445**	[0.0136]
	Number of	Rooms	-0.0844*	[0.0354]	-0.0439	[0.0506]	-0.132**	[0.0491]
		Bedrooms	-0.0499+	[0.0267]	-0.0335	[0.0373]	-0.0690+	[0.0380]
	1= if kitchen is	Also used as bedroom	0.010	[0.00696]	0.0184+	[0.0109]	-0.000732	[0.00804]
		Shared with other households	-0.012	[0.00880]	-0.00501	[0.0134]	-0.0207+	[0.0109]
	1= if household uses different source of energy to electricity/gas		-0.0245*	[0.0121]	-0.00648	[0.0149]	-0.0455*	[0.0196]
	1= if household has landline		-0.017	[0.0122]	0.00446	[0.0174]	-0.0427*	[0.0169]
	House ownership status (1= if housing is	Owned	-0.0487**	[0.0136]	-0.0555**	[0.0191]	-0.0408*	[0.0194]
		Rented	0.0232*	[0.0117]	0.0334*	[0.0168]	0.0113	[0.0160]
		Neither rented nor owned	0.0255*	[0.0101]	0.0221	[0.0137]	0.0295*	[0.0148]
	Observations		5769		3238		2531	

Note: ** p<0.01, * p<0.05, + p<0.1. Robust Standard errors in brackets.

Table A.2. Balance of household characteristics between those that initially intended to participate and not (beneficiaries) – Difference (Cont.)

		Municipalities size	All		Large		Small	
			Difference	s.e.	Difference	s.e.	Difference	s.e.
	1= if household owns other properties		0.0219+	[0.0123]	0.0423**	[0.0162]	-0.002	[0.0188]
Assets and Properties	1= if household has …	Books	0.0145+	[0.00743]	0.0151*	[0.00755]	0.014	[0.0135]
		Fridge	-0.0493**	[0.0138]	-0.011	[0.0188]	-0.0947**	[0.0203]
		Sewing machine	0.005	[0.00912]	0.003	[0.0122]	0.007	[0.0137]
		Black & white tv	0.019	[0.0118]	0.014	[0.0166]	0.026	[0.0168]
		Music machine	-0.0234*	[0.0116]	-0.023	[0.0164]	-0.024	[0.0164]
		Bike	0.0432**	[0.0131]	0.0689**	[0.0171]	0.013	[0.0202]
		Motor vehicle	0.002	[0.00614]	-0.001	[0.00748]	0.004	[0.0100]
		Fan	0.004	[0.00982]	0.012	[0.0140]	-0.004	[0.0136]
		Juice machine	-0.004	[0.0141]	0.016	[0.0191]	-0.028	[0.0210]
		Color tv	-0.022	[0.0141]	-0.002	[0.0193]	-0.0462*	[0.0207]
		Books	0.0219+	[0.0123]	0.0423**	[0.0162]	-0.002	[0.0188]
Participation in other social programs	1 if any member of the household participates in …	*Empleo en Acción - EA*	0.539**	[0.00961]	0.664**	[0.0124]	0.392**	[0.0140]
		Familias en Acción	-0.006	[0.00665]	-0.001	[0.00156]	-0.012	[0.0143]
		Jóvenes en Acción	-0.00584*	[0.00254]	-0.00927*	[0.00459]	-0.002	[0.00130]
		Hogares comunitarios	0.013	[0.00802]	0.0206*	[0.0102]	0.004	[0.0127]
		Other	-0.006	[0.00436]	-0.006	[0.00682]	-0.006	[0.00508]
Health, Education and shocks indicators	1 if household suffered a shock in 2000, 2001 or 2002 due to …	Violence or displacement	0.005	[0.00791]	0.008	[0.0118]	0.003	[0.0102]
		Fire, flooding or natural disaster	0.000	[0.00536]	0.012	[0.00767]	-0.0132+	[0.00739]
		Either business or crop loss	0.0339**	[0.00831]	0.014	[0.00955]	0.0566**	[0.0141]
		A member's loss of job	0.0303*	[0.0122]	0.021	[0.0178]	0.0408*	[0.0163]
		A member severe illness	0.0269*	[0.0106]	0.0424**	[0.0142]	0.009	[0.0159]
		A member death	0.0153*	[0.00688]	0.0192*	[0.00975]	0.011	[0.00963]
	Observations		5769		3238		2531	

Note: ** p<0.01, * p<0.05, + p<0.1. Robust Standard errors in brackets

Table A.3. Diff-in-Diff estimates of the ITT effect on household's consumption – Robustness check for projects not started at baseline survey.

Municipalities size		Without additional controls All	Small	Large	With additional controls All	Small	Large
1st follow up							
	Coeff.	0.03	0.04	0.01	0.04	0.06*	0.00
log consumption	*s.e*	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)
	N	*1476*	*903*	*573*	*1476*	*903*	*573*
	Coeff.	0.06*	0.11**	-0.04	0.06*	0.13**	-0.06
log food consumption	*s.e*	(0.04)	(0.04)	(0.07)	(0.04)	(0.04)	(0.08)
	N	*1734*	*1092*	*642*	*1734*	*1092*	*642*
2nd follow up							
	Coeff.	0.06	0.04	0.09	0.05	0.05	0.07
log consumption	*s.e*	(0.03)	(0.04)	(0.05)	(0.03)	(0.05)	(0.05)
	N	*1259*	*700*	*559*	*1259*	*700*	*559*
	Coeff.	0.02	0.04	-0.00	0.02	0.05	-0.06
log food consumption	*s.e*	(0.04)	(0.05)	(0.07)	(0.04)	(0.05)	(0.08)
	N	*1562*	*894*	*668*	*1562*	*894*	*668*

Note: *** $p<0.001$, ** $p<0.05$, * $p<0.1$. Robust Standard errors in parenthesis.

Table A.4. Time elapsed since end of participation in *EA* at second follow up date

Days since end of participation in EA (2nd f.u.)	Mean	Median	S.d.
Large municipalities	319	281	152
Small municipalities	384	396	131
Total	343	357	148

Table A. 5. Self-reported impact of EA on participants' job search constraints.

Municipalities size	Small		Large	
	male	female	male	female
Thanks to EA, has it been easier to find a job?	21%	14%	21%	12%
If yes: Why? main reason				
gained work experience	47%	22%	40%	26%
learned a new job	15%	17%	7%	10%
got in contact with someone who helps	31%	46%	38%	44%
gained in self-confidence	5%	15%	13%	18%
other	2%	0%	3%	1%
If not: Why not? main reason				
have to little work experience	11%	12%	7%	15%
did not learn enough	11%	8%	9%	3%
have no contact with people who may help	24%	21%	40%	33%
I am not able	3%	4%	5%	4%
other (mostly employment shortage, then age and illness)	52%	56%	39%	45%
Did you find a job?	87%	67%	74%	54%
How long did it take? mean ; median (months)	1.7 ; 1	3.3 ; 1	2.1 ; 1	2.9 ; 1

Note: Subsample = Ex-participants in second follow-up survey.

Table A. 6. Share of unemployed among labor active in small and large municipalities in second follow up (Community sample)

	N	Mean	Sd
Large municipalities	6807	14%	0.004
Small municipalities	6309	6%	0.003
Whole	13116	10%	0.003
t-test: P(Ho: diff = 0)	0.000		

Table A.7. Differences in labor occupation transitions probabilities between randomized in and out for the 7 most frequently reported occupations (3 months before baseline to second F.U.) by municipality size

		Pre Baseline occupation						
		Farming	Manufacture	Construction	Services	Self-emp	unemployed	out
		Small Municipalities						
Second follow up occupation	Farming	-0.128	0.083	-0.048	-0.000	0.009	0.069	-0.003
	Manufacture	0.000	-0.100	0.000	0.084	-0.001	-0.020	-0.012*
	Construction	0.065	0.083	-0.192	-0.112	0.000	0.098**	0.026**
	Services	-0.11	-0.20	0.026	0.041	0.002	-0.110	-0.006
	Self-emp	-0.082	0.083	0.066	-0.077	0.010	0.010	-0.009
	unemployed	0.032	-0.117	0.091	-0.105	0.011	-0.167**	0.014
	out	0.032	0.167	-0.022	0.038	-0.009	0.108**	-0.048
		Large Municipalities						
	Farming	0.050	0.000	0.000	0.000	0.012	-0.009	0.005
	Manufacture	0.055	-0.171	0.000	0.000	-0.004	-0.013	-0.001
	Construction	0.100	0.000	-0.209*	0.016	0.018	0.015	-0.006
	Services	-0.10	0.000	0.000	0.074	0.002	-0.007	-0.027**
	Self-emp	0.250	-0.010	0.106	0.174	-0.031	0.025	0.060**
	unemployed	-0.150	0.124	0.075	-0.053	-0.003	0.022	0.004
	out	-0.025	0.057	0.030	-0.157	-0.032	-0.048	-0.058*

Note: We report the difference in the transition shares reported in Table A.8. For example, in small municipalities individuals previously in farming have a -128%points less chance to end up in farming if they are randomized in. *** p<0.01, ** p<0.05, * p<0.1. Coefficients and pvalues from independent regressions models $1(O_t|O_{t-1}) = \alpha + \beta IP + \varepsilon, E[\varepsilon|IP] = 0$.