

## 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

In this paper, we study a workfare program and provide three contributions. First, we study the impacts of such a program. Second, we test whether the program crowds out labor supply of beneficiary families. Third, we check whether the program has long-lasting effects, beyond the duration of its operation.

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, 2014b).

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 (see Zimmerman, 2014a).<sup>1</sup>

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 on MNREGA, 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). However, these results are for 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.

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<sup>1</sup> 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.

In addition to transfer and stabilization benefits, however, workfare programs might have negative unintended effects by crowding the labor supply of other household members. Thus the second contribution of this paper is to investigate whether *EA* crowded out labor supply of 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, 2014a) 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.<sup>2</sup> For Malawi's large public works program (Social Action Fund - MASAF) Beegle *et al.* (2015) 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 provide 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.<sup>3</sup>

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<sup>2</sup> 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.

<sup>3</sup> 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

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 enhancing persistently the beneficiaries' labor market opportunities and labor income even after the program finishes. More general, 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. Our paper is one of the first to assess this important hypothesis and the first to report positive results in this dimension.

Ravallion, et al. (2005)'s results considered only urban households. In our case, participants in rural areas were confronted with tasks 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 design of the randomized experiment and our identification strategy. The results are analyzed 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.<sup>4</sup> 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.<sup>5</sup> The maximum duration of each project was 5 months.

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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.

<sup>4</sup> 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.

<sup>5</sup> There were some exceptions for projects proposed by local governments.

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)<sup>6</sup>. Eligible individuals could work part-time up to a maximum of 5 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<sup>7</sup>.

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 area. 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.

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 5350 households. Attrition was moderate and the survey covers 4918 households at the first follow up and 4203 households in the second follow up.

### **The Randomization plan and the eventual allocation to the program**

Before a project started, individuals who were interested in participating had to register their interest in a given project. Exploiting the fact that the programs were oversubscribed the local

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<sup>6</sup> 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.

<sup>7</sup> 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.

authorities were asked to choose participants for each project randomly, keeping project specific lists of those randomized in and those not. While the randomization seemed to go mainly to plan, we have circumstantial evidence that in some cases it was compromised by the local authorities including individuals in the treatment group based on other criteria. We refer to individuals allocated to treatment as those “randomized-in”. However our identification strategy will take into account the possible compromise of the randomization.

The time it took between allocation to the program and its actual start led to noncompliance, with some treated individuals dropping out and being replaced by individuals originally allocated to the controls. However, we know the original allocation to treatment and control and hence we can carry out an intention to treat analysis. People on the treatment and control lists had all applied to participate in a *EA* funded project.

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.<sup>8</sup>

Given this final sample size the compliance structure is shown in Table 1. As it can be seen, 8% of randomized controls actually participated in the program and 19% of those randomized-in did not.

**Table 1. Compliance: First Follow Up**

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

<sup>8</sup> 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.

### 3. Identification Strategy

We aim to identify intention-to-treat (ITT) effects, that is, the effect of being offered treatment, which we denote by  $IP=1$ .<sup>9</sup> 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 allocated to treatment and those not. 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. The comparison confirms the reports from program officials and field workers that the allocation process was not entirely random. Though differences are generally not large, some differences are statistically significant<sup>10</sup>. Hence, we cannot rule out the possibility that some unobserved characteristics might be correlated with both the outcome variables and the allocation to the randomized-in list. We will use difference-in-differences to control for the violations in the protocol of randomization. More precisely, we will estimate the following regression model:

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

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

where  $\Delta y_{it} = y_{it} - y_{i0}$  is the difference for individual  $i$ , registered in the list of project  $k$ , in the outcome variable  $y$  (labor income, hours worked and transfers) in period  $t$  and the reference pre-program period 0.<sup>11</sup>  $X_i$  is a vector of individual  $i$ 's time invariant household and individual characteristics 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;<sup>12</sup>  $\theta_k$  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  $\varepsilon_{ikt}$  is an error term. Equation (1b) states our identification assumption that no time varying unobserved variables that determine both the outcome variable and the allocation of individuals into  $IP = 1$ .

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<sup>9</sup> The average impact on the participants can be then easily obtained from this estimate dividing the ITT by the difference between the observed compliance rate and the share of randomized-out individuals used as replacements for non-compliers ( $E[P|IP = 1] - E[P|IP = 0]$ ). This holds under monotonicity and independence assumption as shown e.g. in Duflo, Glennerster and Kremer (2008).

<sup>10</sup> We have also run a more conservative balance check regressing the treatment dummy on similar individual and household characteristics. Testing the joint null for all parameters in each of these regressions leads us to reject the joint null hypothesis, reinforcing our concerns.

<sup>11</sup> The reference year will be 2001 for income and hours worked, and the baseline survey date for consumption and transfers, c.f. *infra*.

<sup>12</sup> 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.

Under this assumption, the estimator of  $\alpha$ , to which we will refer as DIF-in-DIF, 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-programme 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 both participants and non-participants were deemed eligible to join the program. However, to guard against any potential for the initial conditions to be different due to the possible compromise of the randomization protocol, when constructing differences of income or labor supply, we use retrospective measures of income and labor supply ( $y_{i0}$ ) that refer to 2001,<sup>13</sup> which were collected retrospectively in the baseline interview, rather than those relating to the first wave of data (Dec. 2002-Dec. 2003). 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 to use 2001 measures of income and labor supply as  $y_{i0}$ . First, it ensures that  $y_{i0}$  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).

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 inhabitants outside major metropolitan areas as “small”.<sup>14</sup> 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

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<sup>13</sup> 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).

<sup>14</sup> 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.



composition and treatment intensity, we present separate estimates for large and small municipalities.

The key assumption of our differences-in-differences identification strategy is that the counterfactual growth in the outcome variables for those with  $IP = 1$  *within* each project, would have been the same as the growth we observe for those not allocated to a project. Such an assumption would be automatically satisfied if randomization was not compromised. In estimating equation (1a), we require that this assumption holds for the periods of interest, conditional on observables and project fixed effects. While this identifying assumption cannot be tested, we present some corroborative evidence by using retrospective data on income and labor supply for 2001 and 2000 collected at baseline. Figure 1 illustrates the absence of such trend differences. Over the period 2000, 2001 and baseline date, we observe that monthly labor income and weekly hours worked tend to be indeed a bit lower for randomized-in individuals, suggesting that any distortions to the randomization process favored individuals more in need of the program (though the 95% point-wise confidence intervals do overlap). However, we observe that the differences between randomized-in and out individuals remain constant over the pre-program period (parallel trends), consistent with the requirements of the diff-in-diff assumptions. Notice also the absence of any differential pre-program dip in earnings or hours between treatment and control.

More formally, Table 4 reports the results of testing the common trend 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 assumption, and an indication that if the randomization was compromised, it was not in any substantial way.

**Table 2. Descriptive statistics on large and small municipalities.**

	<i>Large municipalities</i>			<i>Small municipalities</i>			<i>Whole sample</i>		
	Mean	Med.	S.d.	Mean	Med.	S.d.	Mean	Med.	S.d.
<i>Population in 2004 (1000)</i>	628	262	1499	33	20	35	249	43	934
<i>Number of projects</i>	35	23	46	7	5	6	17	7	31
<i>Number projects for 100,000 habitants</i>	16	7	22	34	16	38	28	14	34
<i>Expenses by project (2004 US\$)</i>	19334	19415	6559	23813	24676	6981	22191	22403	7113
<i>Expenses by habitant (2004 US\$)</i>	4	1	6	9	4	11	7	3	10
<i>Gini index (2005)</i>	38	41	13	44	44	8	42	44	10
<i>Poverty rate (2005)</i>	11	9	10	52	52	22	37	36	27
<i>Rural index (2004)</i>	38	35	17	67	68	15	57	60	21
<i>Applicants characteristics</i>									
	Mean	S.d.	N	Mean	Sd	N	Diff	Ttest, P(Ho:Diff=0)	
Age	35.4	12.84	3239	35.12	12.42	2532	0.28	0.405	
Female	0.45	0.5	3239	0.26	0.44	2532	0.19	0.000	
Edu1: Level of Education 1	0.09	0.28	3239	0.13	0.33	2530	-0.04	0.000	
Edu2: Level of Education 2	0.26	0.44	3239	0.31	0.46	2530	-0.05	0.000	
Edu3: Level of Education 3	0.24	0.42	3239	0.2	0.4	2530	0.03	0.002	
Edu4: Level of Education 4	0.27	0.44	3239	0.2	0.4	2530	0.07	0.000	
Edu5: Level of Education 5	0.13	0.34	3239	0.14	0.35	2532	-0.01	0.161	
Edu6: Level of Education 6	0.01	0.12	3239	0.02	0.12	2532	0.00	0.620	

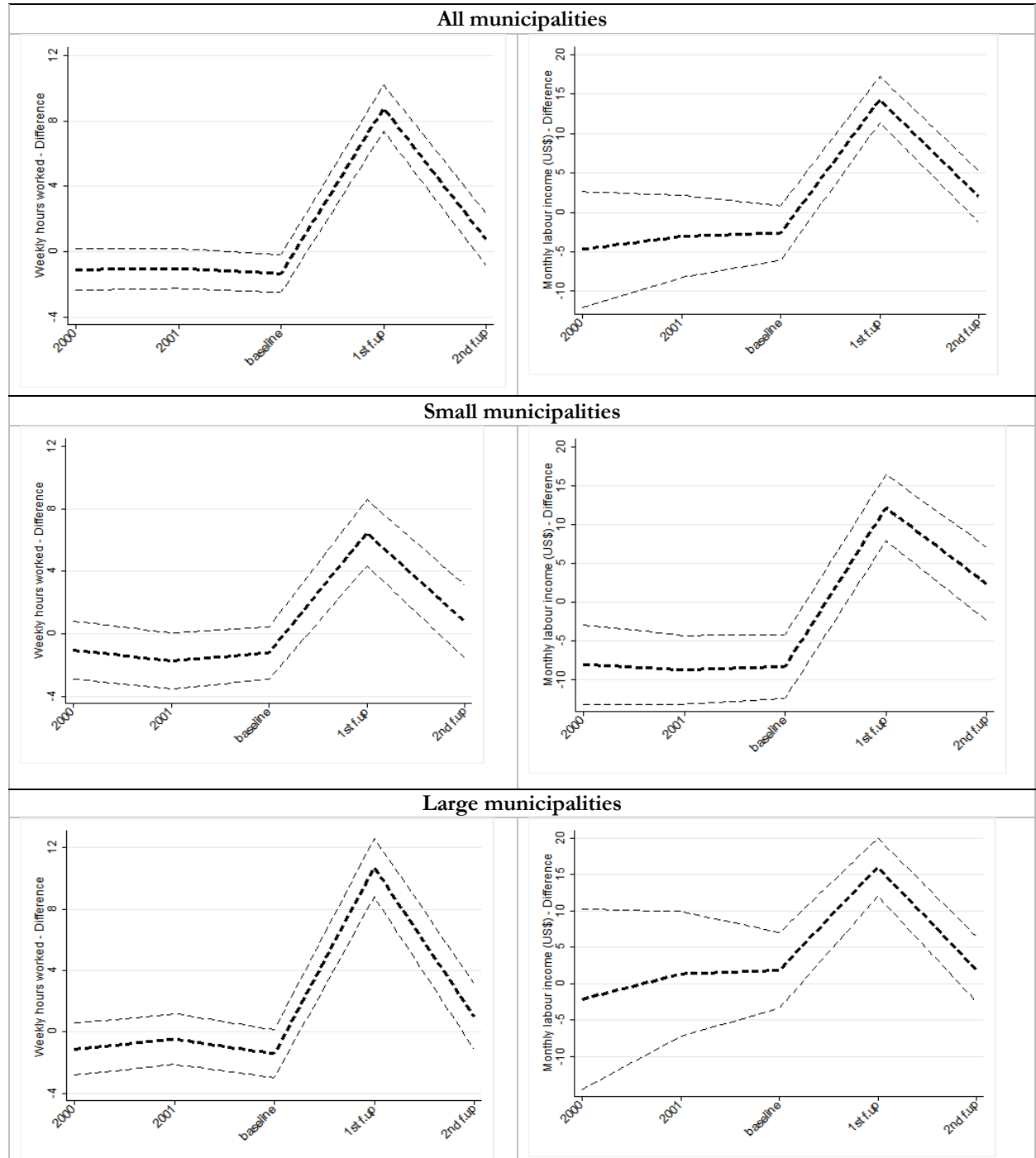
Note: Gini index, rural index (rural population/population) and poverty rate (poverty head count index based on Multidimensional Poverty Index) are from the Municipal Panel Data CEDE, an initiative of the Center of Economic Development Studies (CEDE for its acronymy in Spanish) website.

**Table 3. Descriptive statistics on labor force occupation in large and small municipalities 3 months before baseline.**

<b>Whole population</b>	<b>N</b>	<b>share (%)</b>	<b>Large municipalities</b>	<b>N</b>	<b>share (%)</b>	<b>Small municipalities</b>	<b>N</b>	<b>share (%)</b>
Out of labor force	1599	37.34	Out of labor force	976	40.1	Independent self-imp	694	37.55
Independent self-imp	1255	29.31	Independent self-imp	561	23.05	Out of labor force	623	33.71
Unemployed	608	14.2	Unemployed	455	18.69	Unemployed	153	8.28
Building	152	3.55	Community	102	4.19	Farming	99	5.36
Community	137	3.2	Building	97	3.99	Building	55	2.98
Farming	127	2.97	Domestic	83	3.41	Commerce	41	2.22
Domestic	113	2.64	Commerce	49	2.01	Business owner	37	2
Commerce	90	2.1	Manufacture	29	1.19	Community	35	1.89
Business owner	49	1.14	Farming	28	1.15	Domestic	30	1.62
Manufacture	46	1.07	Help for free	14	0.58	Help for free	30	1.62
Help for free	44	1.03	Business owner	12	0.49	Manufacture	17	0.92
Transport Communication	21	0.49	Transport Communication	10	0.41	Public work	14	0.76
Public work	18	0.42	Electricity, Gas, Water	8	0.33	Transport Communication	11	0.6
Electricity, Gas, Water	9	0.21	Public work	4	0.16	Not specific	4	0.22
Teaching	7	0.16	Teaching	4	0.16	Teaching	3	0.16
Not specific	6	0.14	Not specific	2	0.08	Mines	1	0.05
Mines	1	0.02				Electricity, Gas, Water	1	0.05

Note: Recall during the second follow-up on the main occupation three months before baseline

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

**Table 4. Common trend assumption between 2000 and 2001.**

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

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. However, the estimation of such impacts, relative to the impacts on income, is complicated by the fact that we lack retrospective information on consumption for 2000 and 2001. In our Diff-in-Diff approach, we can only control for consumption at baseline. 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. 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 4 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 8 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 going on. We then test these effects 6 months after the intervention. 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.

### *i. Effects during the intervention*

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

We assess whether *EA* led to an increase in income and hours of work to participants while the projects were on-going. In doing this, 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<sup>15</sup>.

The top panel of Table 5 refers to the ITT effect of the program at individual level while the projects were still on-going (1<sup>st</sup> follow up). The results are obtained controlling for individual characteristics (gender, age, education, migration status) and household-level controls.<sup>16</sup>

The increase in hours work and labor income is significantly positive and very similar in small and large towns: around 10 more hours per week for randomized-in individuals (compared to 27.5 and 22 weekly hours worked on average in the control group for small and large municipalities respectively) and around 19 more US\$ earned per month (compared to US\$53 and US\$47 per month in the control group for small and large municipalities respectively).

The estimate of the effect of offering treatment on program participation ( $E[P_i|IP_i = 1, X] - E[P_i|IP_i = 0, X]$ )<sup>17</sup> 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 (US\$25 (s.e. 4.7) and US\$28 (s.e. 3.8) for large and small municipalities respectively), which represents 38% of the Empleo statutory monthly wage rate (69 US\$), which is lower than the impact found in Jalan and Ravallion (2003) and Ravallion et al. (2005) for *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

<sup>15</sup> 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.

<sup>16</sup> 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). The results without covariates are very similar; they are reported in the appendix in Table A.3.

<sup>17</sup> Monotonicity holds in the sense that  $E[P_i|IP_i = 0] \leq E[P_i|IP_i = 1] \forall i$ , and independence if  $(\Delta Y_i^{P=0}, \Delta Y_i^{P=1}, P_i|IP_i = 0, P_i|IP_i = 1)$  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.

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 the program ended. A similar exercise for the impact on hours worked per week gives an estimated LATE of 13 hours per week (2SLS robust 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*).

As reported in tables A.3 of the appendix, we do not observe major changes in ITT effects when we exclude the covariates from the regressions, which is consistent with our results on the common-trend assumption and provides further corroboration that the diff-in-diff strategy deals adequately with the compromise of the randomization.

**Table 5. Diff-in-Diff estimates of the ITT effect on individuals and households' outcomes in first follow up.**

Dependent variable	<i>With additional controls</i>			<i>Without additional controls</i>		
	All	Small towns	Large Towns	All	Small towns	Large towns
<b><i>Individuals' outcomes</i></b>						
Weekly hours worked	9.68*** (0.93)	9.69*** (1.31)	9.39*** (1.37)	9.89*** (0.92)	9.55*** (1.30)	10.20*** (1.30)
<i>p-value</i>	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
<i>N</i>	4918	2238	2680	4918	2238	2680
<i>Mean (IP=0)</i>	24.68	27.55	22.33	24.68	27.55	22.33
Monthly labor income (US\$)	19.47*** (2.53)	19.35*** (2.71)	19.71*** (4.23)	19.10*** (2.37)	19.21*** (2.73)	19.00*** (3.76)
<i>p-values</i>	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
<i>N</i>	4865	2216	2649	4865	2216	2649
<i>Mean (IP=0)</i>	49.68	52.98	46.99	49.68	52.98	46.99
<b><i>Other household members' outcomes</i></b>						
Weekly hours worked	3.26* (1.54)	2.95 (2.31)	3.31 (2.11)	3.02 (1.52)	2.17 (2.22)	3.67 (2.08)
<i>p-values</i>	0.05	0.49	0.49	0.11	0.46	0.46
<i>N</i>	3574	1483	2091	3574	1483	2091
<i>Mean (IP=0)</i>	133.24	120.94	141.96	133.24	120.94	141.96
Monthly labor income (US\$)	8.49* (4.88)	4.68 (6.64)	13.30 (7.23)	8.90 (4.99)	2.90 (6.64)	13.46 (7.20)
<i>p-values</i>	0.07	0.49	0.43	0.11	0.46	0.65
<i>N</i>	3456	1449	2007	3456	1449	2007
<i>Mean (IP=0)</i>	63.31	65.95	63.14	63.31	65.95	63.14

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 hypothesis of the column “All” are tested jointly, the 8 hypothesis of the columns “Small towns” and “Large towns” are tested jointly.

*Did EA hiring crowd out households’ private labor effort and transfers received by participating households?*

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. This is an important question both in understanding the overall effects of the program and in designing better its targeting. 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. To address this issue we estimate the effects of the program on household level outcomes net of the study individuals ones. Negative estimates would be consistent with crowding-out and positive ones with crowding-in.<sup>18</sup>

The bottom panel estimates of Table 5 are never negative. There is thus no evidence of *EA* crowding out private labor effort by other household members.<sup>19</sup> The effect seems even positive, which suggests that non-participating household members do profit from the participation of another household member, possibly in the form of job opportunities found by the participant while she is on the program (extended networks, job offers related to the project, etc.).

The program may also crowd out private transfers. Households may stop receiving external transfers because of the extra-income earned on *EA*. As shown in Table 6, however, we find no significant impact, neither in the first, nor in the second follow-up survey.

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<sup>18</sup> Crowding out is here understood in its broad definition as a situation where the total household’s labor supply outside *EA* is lowered by the participation on *EA*.

<sup>19</sup> The average number of participants in households with at least one participant is 1.05.



**Table 6. Diff-in-Diff estimates of the ITT effect on household's monetary and in-kind net transfers, in January 2004 US\$.**

	Without additional controls			With additional controls		
	All	Small towns	Large towns	All	Small towns	Large towns
<i>1<sup>st</sup> follow up</i>	-0.98 (1.08)	-0.3 (1.62)	-1.58 (1.44)	-0.58 (1.09)	0.23 (1.67)	-1.02 (1.47)
<i>N</i>	4668	2119	2549	4668	2119	2549
<i>2<sup>nd</sup> follow up</i>	1.35 (1.82)	2.2 (2.37)	0.63 (2.70)	1.4 (1.91)	1.64 (2.50)	1.31 (2.83)
<i>N</i>	3946	1716	2230	3946	1716	2230

Note: Impact on net monthly monetary household income transfers in US\$ January 2004. Each cell reports the estimate on *IP* of a regression of the change in the dependent variable between the first follow-up and 2001, and the second 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). \*\*\*  $p < 0.001$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Robust Standard errors in parenthesis.

**ii. *Effects after the intervention the intervention ended:***

Having established that the program did increase hours of work and employment while it was in operation, which is perhaps not surprising, we next turn to the effect that the program might have had after it finished its operation. In Table 7, we 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 in which the participants were working. This analysis is particularly interesting to test whether the workfare program provided a channel for obtaining longer-term employment or productivity gains (which might happen because of the work experience gained or an increase in the network of contacts acquired during the program). We find that, in small towns, income increased by US\$12 in the second follow up, when the public works programs had already finished (hours also increased but the effect is not statistically significant after the multiple hypothesis test adjustment).<sup>20</sup> No effect is observed in large towns, maybe because there were already ample work opportunities and the connections obtained through *EA* may not have been as important. We investigate these potential channels in the next section.

<sup>20</sup> A first obvious reason explaining why small municipalities would show a significant long-lasting impact would be that in these municipalities *EA* participation happened systematically more recently than in large ones. We show in Table A4 that this was not the case and that small municipalities' participants actually stopped participating earlier in the past.

**Table 7. Diff-in-Diff estimates of the ITT effect on individuals and household outcomes in the second follow up – following program termination**

Dependent variable	With additional controls			Without additional controls		
	All	Small towns	Large Towns	All	Small Towns	Large Towns
<b><i>Individuals' outcomes</i></b>						
Weekly hours worked	1.61 (1.02)	3.89* (1.47)	-0.52 (1.45)	1.60 (1.00)	3.58* (1.40)	-0.10 (1.41)
<i>p-values</i>	0.24	0.05	0.97	0.53	0.06	0.99
<i>N</i>	4213	1861	2352	4213	1860	2352
<i>Mean (IP=0)</i>	24.48	27.07	22.32	24.48	27.07	22.32
Monthly labour income (US\$)	4.81 (2.79)	12.15*** (3.11)	-0.79 (4.71)	4.48 (2.66)	11.49*** (3.07)	-1.64 (4.21)
<i>p-values</i>	0.24	<0.01	0.97	0.49	<0.01	0.99
<i>N</i>	4201	1847	2354	4201	1846	2354
<i>Mean (IP=0)</i>	49.95	52.95	47.50	49.95	52.95	47.50
<b><i>Other household members' outcomes</i></b>						
Weekly hours worked	1.27 (1.86)	0.77 (2.81)	1.65 (2.49)	5.16 (4.93)	2.27 (4.98)	7.35 (7.81)
<i>p-values</i>	0.45	0.97	0.97	0.30	0.99	0.94
<i>N</i>	3058	1227	1831	3046	1230	1816
<i>Mean (IP=0)</i>	133.90	120.09	143.74	133.90	120.09	143.74
Monthly labour income (US\$)	1.76 (1.88)	1.28 (2.82)	1.68 (2.56)	4.85 (4.80)	3.98 (5.16)	6.16 (7.76)
<i>p-values</i>	0.45	0.97	0.97	0.30	0.99	0.98
<i>N</i>	3058	1227	1831	3046	1230	1816
<i>Mean (IP=0)</i>	63.08	63.45	62.82	63.08	63.45	62.82

Note: Each cell reports the estimate on *IP* of a regression of the change in the dependent variable between the second 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 hypothesis of the column "All" are tested jointly, the 8 hypothesis of the columns "Small towns" and "Large towns" are tested jointly.

#### *Investigating the lasting impact of EA*

To our knowledge the strongly significant lasting impact on ex-participants' income and hours worked in small municipalities is new in the empirical literature on public work schemes. It is thus important to understand what may explain this long lasting impact.

As mentioned in Ravallion et al. (2005) the state of local labor markets can be a catalyst of the impact of workfare on ex-participants (the authors find smaller losses from program retrenchment in the provinces with lowest unemployment rates). We do not have access to unemployment rates by municipalities, but in our sample unemployment is more pronounced in

large municipalities (Table 3). It may thus be the case that ex-participants in small municipalities faced a more favorable labor market offering better opportunities.

Four to thirteen months after participation, ex-participants were asked why *EA* had made it easier for them to find a job (Table A.8). 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 hired on farming work before *EA*, while the work offered on the projects was mostly related to building activities. Hence, for many beneficiaries participating in *EA* meant learning new skills. In large municipalities, where there was no long-term effect, building activities were less novel to them (Table A.8) and hence they did not acquire new skills as a result of their participation in *EA*. 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.<sup>21</sup>

Our data allows us to compare the labor force occupations transitions between pre and post *EA* labor force occupations, conditional on pre *EA* occupations for randomized-in and -out in small and large municipalities. As can be seen in Tables A.10 and A.11, it appears first that the share of previously working falling into unemployment tends to be higher in large municipalities.

In order to compare further the transition differentials between randomized in and out, we report in table 8 and table 9 the sample mean estimator of  $\mathbb{P}[O_t|O_{t-1}, IP = 1] - \mathbb{P}[O_t|O_{t-1}, IP = 0]$  where  $O_t$  is the labor occupation in second follow-up and  $O_{t-1}$  the one three months before the baseline survey. Several differences emerge between small and large municipalities. In small municipalities randomized-in individuals have a higher probability to switch from farming to building and community work activities<sup>22</sup> (+14%pt vs. +10%pt in large ones), from unemployment /out of labor force to building (+10%pt and +3%pt vs 2%pt and 0%pt in large ones). They also have a lower probability to stay in farming (-13%pt vs. +1%pt in large ones) and to stay unemployed (-22%pt vs. +3%pt in large ones). As such, these differences can however hardly be interpreted as being significant: most individuals in our survey are either self-employment and unemployed/out of labor force and all other categories represent a rather

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<sup>21</sup> 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).

<sup>22</sup> The positive impact on the transition from farming to building is also observed in large municipalities, but farming workers represent a smaller share of the labor force in large ones (Table 3).

small share of the population. This implies low power when testing the significance of mean differences, in particular considering the number of hypothesis jointly tested.

Table 8 presents the ITT of EA on two transitions i) from unpaid (out of lab. Force, unemployed, help for free) to any paid activities, and ii) from self-employed/business owner to other paid activities. We found significant differences from past unpaid activities to current any paid activities in small municipalities and from past self-employed or owner activities to current other paid activities in large municipalities. After trying many different ways of pooling activities, all other transitions concern too small categories to be reasonably tested.

**Table 8. Differences in labor occupation transitions probabilities between randomized in and out for unpaid and self-employed/business owner occupations (3 months before baseline to second F.U.)**

		Pre baseline labor occupation					
		Unpaid			Self-employed/Business Owner		
		All	Small	Large	All	Small	Large
Second follow up occupation	“any paid activities”	0.025 (0.263) 2251	0.069* (0.061) 806	-0.001 (0.969) 1445			
	“other paid activities”				0.014 (0.436) 1184	-0.012 (0.589) 686	0.064** (0.039) 498

Probability, standard errors in parenthesis and number of observations. \*p<0.10, \*\* p<0.05, \*\*\* p<0.01

The overall picture emerging from these descriptive statistics is the one in which participants from small rural municipalities have learned new skills (thanks to participation e.g. in building activities which were relatively new to them) and get new contacts, which helped them switch from badly to better paid activities and from unemployment to building and farming jobs. In large municipalities, participants' previous occupations were probably more similar to those demanded on EA projects, implying lower human capital accumulation, which could be anyway hardly materialize in extra-income because of tighter labor market constraints.

**Table 9. 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.)**

		PRE BASELINE LABOUR OCCUPATION						
		Farming	Manufacture	Construction	Services	Self-emp	unemployed	out
SECOND FOLLOW UP OCCUPATION	<b><i>SMALL</i></b>							
	Farming	<b>-0.12</b>	0.08	-0.06	-0.02	0.01	0.07	0.00
	Manufacture	0.00	-0.10	0.00	0.01	0.00	-0.02	-0.01
	Construction	<b>0.08</b>	0.08	-0.21	-0.03	-0.01	<b>0.11</b>	<b>0.04</b>
	Services	0.04	<b>-0.2</b>	0.06	0.19	0.00	-0.12	0.03
	Self-emp	-0.06	0.08	0.08	-0.07	0.00	0.02	-0.03
	unemployed	0.03	-0.12	0.08	-0.10	0.01	<b>-0.16</b>	0.01
	out	0.02	0.17	0.04	0.02	-0.01	0.10	-0.04
	<b><i>LARGE</i></b>							
	Farming	<b>0.01</b>	0.00	0.00	0.00	0.01	-0.01	0.00
	Manufacture	0.05	-0.17	0.00	0.02	0.00	-0.01	0.00
	Construction	<b>0.10</b>	0.00	-0.20	0.01	0.03	<b>0.01</b>	<b>0.00</b>
	Services	-0.19	0.00	0.07	-0.04	0.03	0.01	-0.02
	Self-emp	0.25	-0.01	0.10	0.02	-0.04	0.02	0.07
	unemployed	-0.19	0.12	0.09	-0.04	0.00	<b>0.02</b>	0.01
	out	-0.04	0.06	0.09	0.07	-0.02	-0.05	-0.05

Reading 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 -13%points less chance to end up in farming if they are randomized in.

### *iii. Effect on consumption*

We next consider the impacts of the program, both in the short-runs and in the medium run, on household consumption. 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 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 the upper panel of Table 10, we report the impact of the program on log total consumption and food consumption 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). These estimates are ITT effects controlling for additional households characteristics (see Table A.4 of the appendix for results without covariates). We find an increase of 5% in small municipalities on log consumption;

the effect on food consumption is (+10%) implying a higher sensitivity of food in such poor communities. These positive impacts 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).

**Table 10. Diff-in-Diff estimates of the ITT effect on household's consumption.**

Dependent variable	With additional controls			Without additional controls		
	All	Small towns	Large towns	All	Small towns	Large towns
<i>1st follow relative to baseline</i>						
log consumption	0.01 (0.02)	0.05* (0.02)	-0.02 (0.02)	0.01 (0.01)	0.05** (0.02)	-0.03 (0.02)
N	3853	1687	2166	3853	1687	2166
log food consumption	0.02 (0.02)	0.10*** (0.03)	-0.05 (0.03)	0.02 (0.02)	0.10*** (0.03)	-0.05* (0.03)
N	4580	2085	2495	4580	2085	2495
<i>2nd follow up relative to baseline</i>						
log consumption	0.01 (0.02)	0.02 (0.03)	0.01 (0.03)	-0.00 (0.02)	0.01 (0.03)	-0.01 (0.03)
N	3063	1328	1735	3063	1328	1735
log food consumption	-0.01 (0.03)	0.03 (0.04)	-0.05 (0.04)	-0.02 (0.02)	0.04 (0.04)	-0.07* (0.03)
N	3965	1744	2221	3965	1744	2221

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 (upper part) and between the second follow-up and 2001 (bottom-part). 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). \*\*\*  $p < 0.001$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Robust Standard errors in parenthesis.

However, when we turn to the longer-term effect, we find no change in consumption relative to baseline. This is despite the fact we see some increases in income and hours of work in the second follow up. This means either that consumption was already affected at baseline because of anticipation effects, or indeed that households view the program effects as transitory and save the extra income. As a check we report in Table A.5 of the appendix the estimates on the

subsample of households where the projects had not started at baseline. We obtain similar results to the one presented in Table 10.

When comparing these impacts on consumption with those identified on income, they are significantly smaller. One may thus wonder what participating households actually did with the share of extra income that is not used to smooth or increase consumption. In the second follow-up survey ex-participants were asked on how they used the extra income earned on EA used. The results in Table 10 document that 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. Of course some of it is consumed, reflecting the heterogeneous circumstances of the households.

**Table 11. Ex-participants self-reported use of income earned on EA (second F.U.)**

<i>Did you use EA income on...</i>	<i>mean</i>	<i>N</i>
accommodation	4%	2580
repay debt	44%	2585
business creation	3%	2581
medical treatment	6%	2575
public services	41%	2579
other (food, clothes, education)	85%	2574

## 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.

First, we test whether the program crowds out labor effort by members of the household different from the participant in the particular context of a middle-income economy. Our results show no evidence of EA hiring crowding out private labor effort by other household members. In addition, we find no evidence of crowding out both monetary and in-kind transfers to the beneficiary household by the program.

Second, we test whether there are gains in household labor income, but also in consumption, which is important to assess the role of the program as an insurance mechanism. We find that the program had large positive *transfer* benefits, as the program increased individual's

labor income and labor supply (i.e. hours of work) while the program was on going in large as well as small towns. Finally, we find that *EA* may have provided stabilization benefits in small municipalities with a positive significant impact in small municipalities on log consumption which is doubled when focusing only on food consumption, which is consistent with previous studies for rural India (Deininger and Liu (2013) and Ravi and Engler (2015)).

Third, we test whether there are some gains from participating in the program six months after the program has finished. We do find that *EA* had a significant positive effect on individuals' labor income and labor supply as well as on households' monthly labor income per capita in small municipalities. We provide descriptive statistics on labor occupation transitions pre and post intervention. For *EA* workers in small rural municipalities we exhibit evidence of sectors switch from farming to building and community activities, consistent with new skills accumulation, as well as higher probability to escape unemployment and to return to the labor force. These results support the idea that public work schemes may change participants' human capital accumulation or participants' labor market conditions when the work offered is far from their previous labor occupation, which can favor their future labor income after the program ended. This is to our knowledge a new result in the empirical literature on workfare program.

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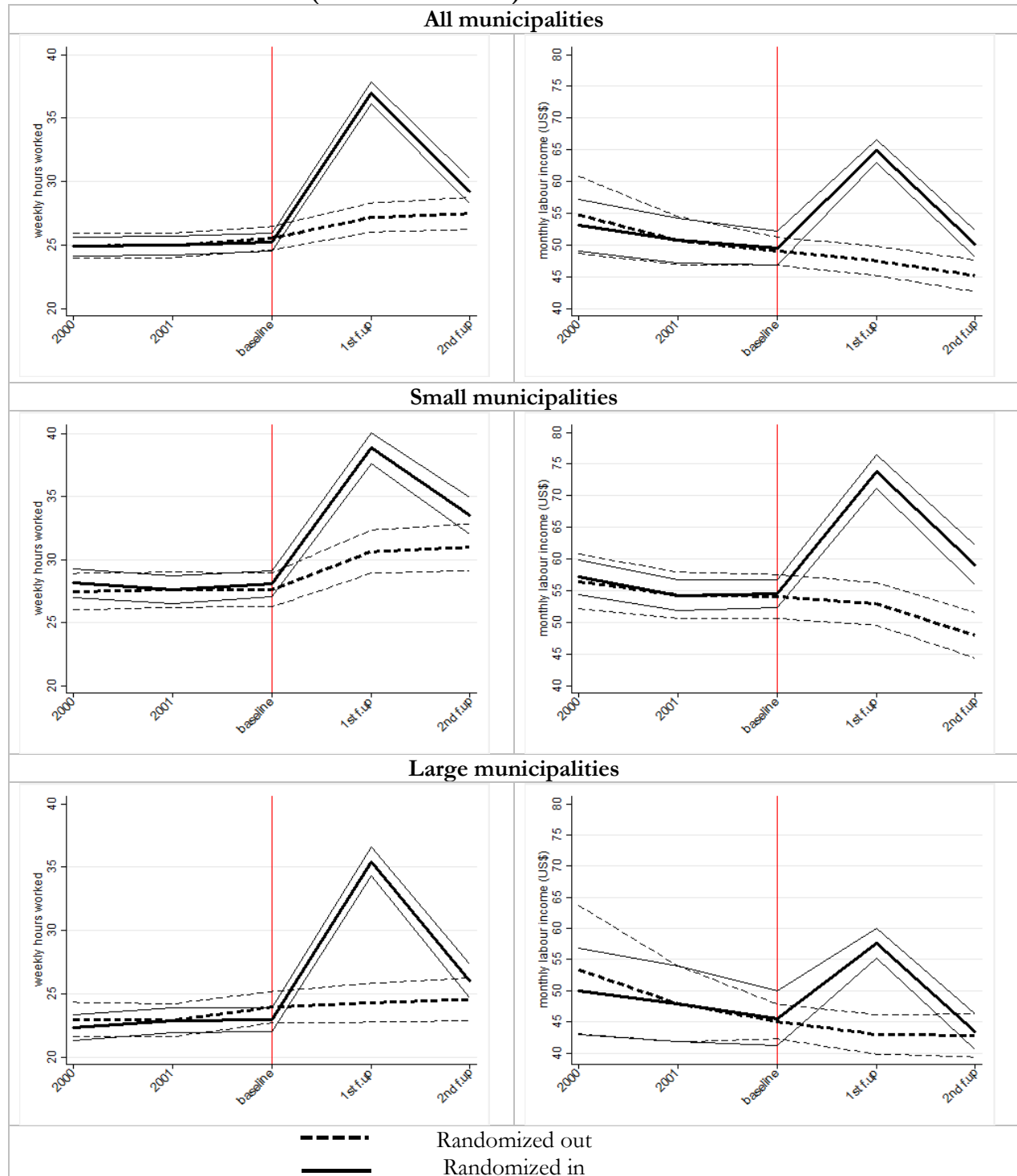
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## 7. Appendix

*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.

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

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

			All		Large Towns		Small Towns	
			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]
		Brick	-0.0189+	[0.0112]	0.0105	[0.0147]	-0.0531**	[0.0172]
	1= if walls are made of	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]
		Rubbish disposal and collection 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 and 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	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]
	housing is	Neither rented nor owned	0.0255*	[0.0101]	0.0221	[0.0137]	0.0295*	[0.0148]
		Books	0.0219+	[0.0123]	0.0423**	[0.0162]	-0.002	[0.0188]
Observations			569		3238		2531	

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

			All		Large Towns		Small Towns	
			Difference	s.e.	Difference	s.e.	Difference	s.e.
<b>Assets and Properties</b>	1= if household owns other properties		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]
	1= if household has	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]
<b>Participation in other social programs</b>	1 if any member of the household participates in ..	<i>Empleo en Acción - EA</i>	0.539**	[0.00961]	0.664**	[0.0124]	0.392**	[0.0140]
		<i>Familias en Acción</i>	-0.006	[0.00665]	-0.001	[0.00156]	-0.012	[0.0143]
		<i>Jóvenes en Acción</i>	-0.00584*	[0.00254]	-0.00927*	[0.00459]	-0.002	[0.00130]
		<i>Hogares comunitarios</i>	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]
<b>Health, Education and shocks indicators</b>	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 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			569		3238		2531	

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

**Table A.4. Diff-in-Diff estimates of the ITT effect on household's consumption without additional controls.**

Dependent variable		Without additional controls		
		All	Small towns	Large Towns
<b><i>1st follow up</i></b>				
log consumption	<i>Coeff.</i>	0.01	0.05**	-0.03
	<i>s.e</i>	(0.01)	(0.02)	(0.02)
	<i>N</i>	3853	1687	2166
log food consumption	<i>Coeff.</i>	0.02	0.10***	-0.05*
	<i>s.e</i>	(0.02)	(0.03)	(0.03)
	<i>N</i>	4580	2085	2495
<b><i>2nd follow up</i></b>				
log consumption	<i>Coeff.</i>	-0.00	0.01	-0.01
	<i>s.e</i>	(0.02)	(0.03)	(0.03)
	<i>N</i>	3063	1328	1735
log food consumption	<i>Coeff.</i>	-0.02	0.04	-0.07*
	<i>s.e</i>	(0.02)	(0.04)	(0.03)
	<i>N</i>	3965	1744	2221

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

**Table A.5. Diff-in-Diff estimates of the ITT effect on household's consumption – Robustness check for projects not started at baseline survey.**

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

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



**Table A.6. Time elapsed since end of participation in EA at second follow up date**

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

**Table A.7 Long lasting ITT effect in small municipalities, gender heterogeneity.**

<b>Small municipalities only</b>			Without additional controls			With additional controls		
Dependent variable			All	female	male	All	Female	male
<b><i>Individuals' outcomes</i></b>								
Weekly worked	hours	Coeff.	3.51*	3.71	3.41*	3.89**	3.56	4.07*
		s.e	(1.40)	(2.83)	(1.66)	(1.47)	(3.11)	(1.77)
		N	1861	500	1361	1861	500	1361
Monthly income [US\$]	labor	Coeff.	11.39***	9.58	11.00**	12.15***	10.29	12.58**
		s.e	(3.07)	(5.26)	(3.79)	(3.11)	(6.09)	(3.89)
		N	1847	496	1351	1847	496	1351

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

**Table A. 8. Self-reported impact of EA on participants' job search constraints.**

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

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

**Table A. 9. Share of unemployed among labor active in small and large municipalities in second follow up (Community sample)**

	N	Mean	Sd
<i>Large municipalities</i>	6807	14%	0.004
<i>Small municipalities</i>	6309	6%	0.003
<i>Whole</i>	13116	10%	0.003
<i>t-test: <math>P(H_0: \text{diff} = 0)</math></i>	0.000		

**Table A. 10. Labor force transitions between pre-baseline and 2<sup>nd</sup> follow-up for the most frequently reported occupations in Small municipalities**

<b>SECOND FOLLOW UP OCCUPATION</b>	PRE BASELINE LABOUR OCCUPATION									
	<i>Small</i>	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
	<i>IP = 0</i>									
	Farming	0.78	0.00	0.13	0.00	0.13	0.00	0.02	0.05	0.04
	Manufacture	0.00	0.60	0.00	0.05	0.00	0.08	0.00	0.02	0.01
	Building	0.00	0.00	0.53	0.16	0.00	0.00	0.01	0.02	0.01
	Commerce	0.03	0.00	0.00	0.37	0.00	0.00	0.01	0.07	0.06
	Community	0.00	0.00	0.07	0.11	0.33	0.00	0.03	0.07	0.04
	Domestic	0.00	0.20	0.00	0.00	0.00	0.50	0.00	0.00	0.03
	Indpt self-imp	0.17	0.00	0.07	0.16	0.33	0.08	0.87	0.27	0.34
<b>SECOND FOLLOW UP OCCUPATION</b>	unemployed	0.00	0.20	0.07	0.11	0.13	0.00	0.02	0.48	0.03
	out	0.03	0.00	0.13	0.05	0.07	0.33	0.04	0.02	0.44
	<i>IP = 1</i>									
	Farming	0.65	0.08	0.08	0.00	0.00	0.06	0.03	0.11	0.04
	Manufacture	0.00	0.50	0.00	0.14	0.00	0.00	0.00	0.00	0.01
	Building	0.07	0.08	0.32	0.05	0.00	0.00	0.01	0.12	0.04
	Commerce	0.02	0.00	0.03	0.41	0.11	0.00	0.01	0.05	0.05
	Community	0.07	0.00	0.08	0.05	0.53	0.11	0.02	0.07	0.07
	Domestic	0.02	0.00	0.03	0.09	0.05	0.44	0.01	0.00	0.05
	Indpt self-imp	0.10	0.08	0.16	0.18	0.21	0.00	0.86	0.27	0.31
	unemployed	0.03	0.08	0.16	0.00	0.05	0.00	0.03	0.26	0.04
	out	0.05	0.17	0.16	0.09	0.05	0.39	0.03	0.12	0.40

Reading Note: Each sub column sums up to 1. E.g. in small municipalities 78% of randomized out individual who were in farming before the baseline are still in farming in the second follow-up.

**Table A. 11. Labor force transitions between pre-baseline and 2<sup>nd</sup> follow-up for the most frequently reported occupations in large municipalities**

SECOND FOLLOW UP OCCUPATION	PRE BASELINE LABOUR OCCUPATION									
	<i><b>LARGE</b></i>	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
	<i><b>IP = 0</b></i>									
	Farming	0.29	0.00	0.00	0.00	0.00	0.00	0.00	0.02	0.01
	Manufacture	0.00	0.57	0.03	0.00	0.00	0.00	0.01	0.07	0.02
	Building	0.00	0.00	0.58	0.03	0.00	0.00	0.01	0.08	0.04
	Commerce	0.00	0.00	0.03	0.28	0.04	0.07	0.01	0.05	0.06
	Community	0.00	0.00	0.03	0.00	0.54	0.00	0.00	0.08	0.04
	Domestic	0.29	0.00	0.03	0.10	0.00	0.59	0.00	0.01	0.07
	Indpt self-imp	0.00	0.14	0.10	0.28	0.19	0.07	0.80	0.16	0.20
unemployed	0.29	0.14	0.13	0.10	0.13	0.07	0.04	0.35	0.05	
out	0.14	0.14	0.06	0.21	0.11	0.19	0.11	0.17	0.52	
<i><b>IP = 1</b></i>										
Farming	0.30	0.00	0.00	0.00	0.00	0.00	0.02	0.01	0.01	
Manufacture	0.05	0.40	0.03	0.00	0.04	0.00	0.01	0.06	0.02	
Building	0.10	0.00	0.36	0.05	0.02	0.00	0.03	0.10	0.04	
Commerce	0.10	0.00	0.03	0.35	0.09	0.04	0.02	0.04	0.04	
Community	0.00	0.00	0.02	0.00	0.42	0.04	0.02	0.08	0.05	
Domestic	0.00	0.00	0.02	0.05	0.00	0.46	0.01	0.02	0.06	
Indpt self-imp	0.25	0.13	0.22	0.45	0.09	0.09	0.78	0.18	0.27	
unemployed	0.10	0.27	0.20	0.05	0.13	0.04	0.04	0.37	0.06	
out	0.10	0.20	0.13	0.05	0.20	0.34	0.09	0.13	0.47	

Reading Note: See table A.10.

**Table A.12. Differences in labor occupation transitions probabilities between randomized in and out for the 9 most frequently reported occupations (3 months before baseline to second F.U.)**

SECOND FOLLOW UP OCCUPATION	PRE BASELINE LABOUR OCCUPATION									
	<i><b>SMALL</b></i>	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
	Farming	<b>-0.13</b>	0.08	-0.05	0.00	-0.13	0.06	0.01	0.07	0.00
	Manufacture	0.00	-0.10	0.00	0.08	0.00	-0.08	0.00	-0.02	-0.01
	Building	<b>0.07</b>	0.08	-0.22	-0.11	0.00	0.00	0.00	<b>0.10</b>	<b>0.03</b>
	Commerce	-0.01	0.00	0.03	0.04	0.11	0.00	0.00	-0.02	0.00
	Community	<b>0.07</b>	0.00	0.01	-0.06	<b>0.19</b>	<b>0.11</b>	-0.01	0.00	0.02
	Domestic	0.02	-0.20	0.03	0.09	0.05	-0.06	0.00	0.00	0.02
	Indpt self-imp	-0.07	0.08	0.09	0.02	-0.12	-0.08	-0.01	-0.01	-0.03
	unemployed	0.03	-0.12	0.09	-0.11	-0.08	0.00	0.01	<b>-0.22</b>	0.01
	out	0.02	0.17	0.02	0.04	-0.01	0.06	-0.01	0.10	-0.04
	<i><b>LARGE</b></i>	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
	Farming	<b>0.01</b>	0.00	0.00	0.00	0.00	0.00	0.01	-0.01	0.01
	Manufacture	0.05	-0.17	0.00	0.00	0.04	0.00	0.00	-0.01	0.00
	Building	<b>0.10</b>	0.00	-0.22	0.02	0.02	0.00	0.02	<b>0.02</b>	<b>0.00</b>
	Commerce	0.10	0.00	0.00	0.07	0.05	-0.04	0.00	-0.01	-0.03
	Community	<b>0.00</b>	0.00	-0.02	0.00	<b>-0.11</b>	<b>0.04</b>	0.01	0.00	0.01
	Domestic	-0.29	0.00	-0.02	-0.05	0.00	-0.13	0.01	0.01	0.00
	Indpt self-imp	0.25	-0.01	0.12	0.17	-0.1	0.02	-0.03	0.02	0.07
	unemployed	-0.19	0.12	0.07	-0.05	0.00	-0.04	0.00	<b>0.03</b>	0.01
	out	-0.04	0.06	0.06	-0.16	0.09	0.15	-0.02	-0.05	-0.05

Reading note: See table A. 10