

## Work pays: different benefits of a workfare program in Colombia

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### Abstract

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 in the particular context of a middle-income economy: (1) whether the program crowds out labor effort by members of the household different from the participant, (2) 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 and (3) whether there are some gains from participating in the program six months after the program has finished.

Our results show no evidence of the program crowding out private labor effort by other household members. In addition, we find that the program had large positive transfer benefits, as the program increased individual's labor income and labor supply in large urban as well as small rural municipalities. There is a positive significant impact in small municipalities on consumption which is doubled when focusing only on food consumption. Finally, we do find that the program had a positive effect on individuals' outcomes as well as on households' monthly labor income per capita in small rural municipalities six months after the program ended. We shed light on the potential channels explaining this novel result in the literature on public work schemes.

**JEL codes:** D04 (Microeconomic Policy: Formulation, Implementation, and Evaluation); H53 (Government Expenditures and Welfare Programs); I38 (Government Policy • Provision and Effects of Welfare Programs); J48 (Particular labor markets, Public Policy); J38 (Wages compensation and labor costs, Public Policy); J22 (Time Allocation and Labor Supply)

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## 1. Introduction

Workfare programs provide a low wage to individuals that work in selected public works. The low wage and the work requisite mean that poor individuals are more likely to participate in the program, which is desirable for the targeting of the program (Ravallion (1991), Besley and Coate (1992)). This self-selecting feature of workfare programs makes them a popular intervention and a substitute to unemployment insurance in developing countries, where governments usually lack the capacity and information systems to identify poor and/or unemployed individuals, possibly because of the presence of a large informal sector (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 simply 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) for a model in a poor rural economy).<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 and it might be the case that a better access to formal 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)*

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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.

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 effects on efficiency. In particular, public works hiring might crowd out households private labor effort. In the absence of the program, households might offset an individual's unemployment shock by increasing the labor effort exerted by other individuals of the households.

In the context of a low-income economy, Datt and Ravallion (1994) find that for one village of the state of Maharashtra in India, a poor rural labor-surplus economy, the work of men on the farm increases when women participate in the workfare program that they analyze. 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.

The second contribution of this paper is to investigate whether *EA* crowded out labor supply of other adult household members in rural and urban areas of a middle-income economy. This aspect is important because crowding out may affect the extra earnings from the program and because it creates heterogeneous forgone earnings on which relies the self-targeting performance of public work schemes. Recent empirical studies find mixed evidence in this respect for MNREGA. Deininger and Liu (2013) and 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> We are however not aware of any other empirical research that looks at how workfare programs affect intra-household allocation of labor in middle-income economies, such as Colombia. Crowding out effects might be different in low and in middle-income economies (Ravallion (1999)), as well as in rural and urban areas. Involuntary underemployment might be less pronounced than in rural India, leading to potentially higher crowding out effects where non-participating households members tend not to take over forgone participant's labor opportunities. Workfare may also crowd out private transfers and we test whether households stop receiving external transfers because of their participation in *EA*.

The third contribution of this paper is to test whether workfare programs, in the long run, improve the labor market opportunities of participants. According to Besley and Coate (1992) workfare programs do not only self-target the poor (i.e. the *screening argument*), but

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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.

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*). Moreover, the participation in work might prevent the depreciation of human capital or even increasing it. Participants in a workfare program might improve some of their skills and expand their contacts, or change their labor search habits, which might enhance persistently their labor market opportunities and labor income, as well as other household's members ones, even after the program finishes.

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> 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.

Ravallion, et al. (2005)'s results considered only urban households. Having data on rural and urban municipalities, we can check if rural participants did not benefit more in the long term. Rural households were asked to perform new tasks related to building, quite different from their usual farming activities. The knowledge and skills acquired in these new tasks may have increased their productivity and help them switch to better paid occupations or to find a job if they were unemployed. The larger sample that we use in this paper allows to test for these long lasting effects with higher power, and to shed light on their heterogeneity with respect to participants' pre-intervention occupation and their economic environment.

The following section describes the particularities of *EA* and the data collected. In section 3 we present our identification strategy. The results are analyzed in Section 4, while Section 5 concludes.

## **2. The program and the data**

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

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<sup>3</sup> Testing for this 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.

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 must 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.

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 survey. The second wave of data was collected between March 2003 and January 2004, while 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; the projects had finished between 4 and 13 months before it was collected. This third wave is the one that allows us to study the impact

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<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.

<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.

of the workfare program once it has finished. Something to note about the first wave of data collection (baseline) is that, for some projects, individuals were already working in the *EA* project when the data was collected, although no individuals had been paid yet. This was due to some projects starting earlier than originally planned with the objective of providing relief as soon as possible. We will explain below how our empirical strategy accommodates this issue.

Before a project started, individuals who were interested in participating needed to register their interest in a given project. The local authorities were asked to create two lists, one of randomized-in individuals, and another one of randomized-out. However, from conversations with program officials and field workers, we know that such randomizations did not always take place and some individuals were included in these lists through some ad hoc process different from a randomized mechanism. Although we have both lists of individuals, we do not know whether a given person was included in one of the two lists through a randomized mechanism or through a different process. Our identification strategy will also take this into account.

**Table 1. Joint distribution of participants (*P*) and randomized in (*IP*).**

FIRST FOLLOW UP		
When <i>P</i> =currently participating in EA during the first follow-up		
First follow up	<i>IP</i> = 1	<i>IP</i> = 0
<i>P</i> = 1	1944 (64%)	120 (5%)
<i>P</i> = 0	1115 (36%)	2064 (95%)
When <i>P</i> = participated or currently participating in EA		
First follow up	<i>IP</i> = 1	<i>IP</i> = 0
<i>P</i> = 1	2591 (81%)	162 (8%)
<i>P</i> = 0	594 (19%)	1902 (92%)
SECOND FOLLOW UP		
When <i>P</i> =participated or currently participating in EA		
Second follow up	<i>IP</i> = 1	<i>IP</i> = 0
<i>P</i> = 1	2441 (86%)	293 (15%)
<i>P</i> = 0	405 (14%)	1610 (85%)

As it is to be expected, some individuals who were in the list of randomized-in, eventually decided not to participate in EA (i.e. non-compliance), in which case replacements were

found among the list of randomized-out.<sup>8</sup> Our sample is a random sample drawn from the list of individuals who were in the randomized-in and randomized-out lists (so they had all expressed their willingness to work for an EA funded project), independently of their actual participation in the 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>9</sup>

### 3. Identification Strategy

To explain our identification strategy, it is useful to introduce some notation. Let the variable  $IP_i$  be an indicator function that equals one if the individual  $i$  was part of the randomized-in list, and 0 if he was part of the randomized-out one. Let the variable  $P_i$  be an indicator function that equals one if the individual  $i$  actually participated in the EA project, 0 otherwise. The joint distribution of  $IP_i$  and  $P_i$  is given in Table 1. The total number of people that was initially allocated to the program is 3185 (i.e.  $IP_i = 1$  in the first follow up) and the total number of people that participated in an EA project is 2753 (i.e.  $P_i = 1$  in the first follow up).

#### i. Identifying transfer benefits

We aim to identify intention-to-treat (ITT) effects, that is, the effect of being in the list of randomized-in individuals ( $IP=1$ ).<sup>10</sup> Our identification strategy must consider the challenge that the process of allocating individuals to the randomized-in and out list was not entirely random. Tables A1 and A2 in the appendix compare the characteristics of randomized-in and out. Table A1 compares basic individual characteristics such as gender, age, education, health

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<sup>8</sup> For some projects there was a substantial lag between the moment when the list was drawn and the project started.

<sup>9</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.

<sup>10</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).

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. 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-difference to control for the violations in the protocol of randomization. More precisely, we will estimate the following regression model:

$$\Delta y_{it} = \alpha IP_{ik} + \beta X_i + \theta_k + \varepsilon_{ikt}, \quad E[\varepsilon_{ikt} | IP_{ik}, X_i, \theta_k] = 0 \quad (1)$$

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, and  $\varepsilon_{ikt}$  is an error term. Importantly, the project fixed effect is used to reflect that the allocation to  $IP = 1$  or  $IP = 0$  was conditional on being registered in a list of a given project. We are thus identifying the effects by taking within project differences rather than treating all projects as homogeneous. The estimator of  $\alpha$ , to which we will refer as DIF-in-DIF, will provide a consistent estimate of the ITT as long as there are no time varying unobserved variables that determine both the outcome variable and the allocation of individuals into  $IP = 1$ .

The existence of an Ashenfelter's pre-programme dip (Ashenfelter (1978)) among individuals included in the 'randomized-in' lists in defiance of the protocol is a potentially serious limitation for a difference in difference approach as the one we outline above. In the presence of such a dip, individuals included in the 'randomized-in' lists not because of a randomization process but for some other reason, might be those who experience a temporal decline in earnings in the period prior to program entry. If that is the case, the difference in difference estimator might overestimate the impact of the program (Heckman and Smith (1999)).

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<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.



First, we notice that both individuals with  $IP = 1$  and  $IP = 0$  are drawn from the pool of applicants, so that the standard concern with the Ashenfelter’s dip might be less severe in our case. Second, as baseline measures of income and labor supply,  $y_{i0}$ , we will use retrospective measures of income and labor supply that refer to 2001<sup>13</sup>, and which were collected in the baseline interview, rather than those relating to the first wave of data (Dec. 2002-Dec. 2003) was collected, but. Since, the application process took place in 2002, our measure of income and labor supply refers to a period before the potential temporal dip in income that is contemporaneous with 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 allows us to ensure 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 the following, 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 habitants were initiated in small municipalities, relative to the large ones. Expenditure per project were 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.

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. While this identifying assumption cannot be tested we present some corroborative evidence

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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.

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 systematically lower for randomized-out individuals, suggesting that the allocation process favored individuals more in need for the program (though 95% confidence intervals do overlap). However, we observe a clear parallel trend over this pre-program period for both outcome variables, which comforts further our common trend assumption. Notice also that no pre-program dip can be observed on these figures.

More formally, Table 4 reports the results of regressions of the growth of monthly labor income and weekly hours worked between 2001 and 2000 on the  $IP$  dummy variable, showing that the differences in growth between those with  $IP = 1$  and those with  $IP = 0$  are small and not statistically different from zero. This evidence corroborates our identification assumption. We will thus focus mostly on DIF-in-DIF regressions estimates of impacts.

**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	Sd	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

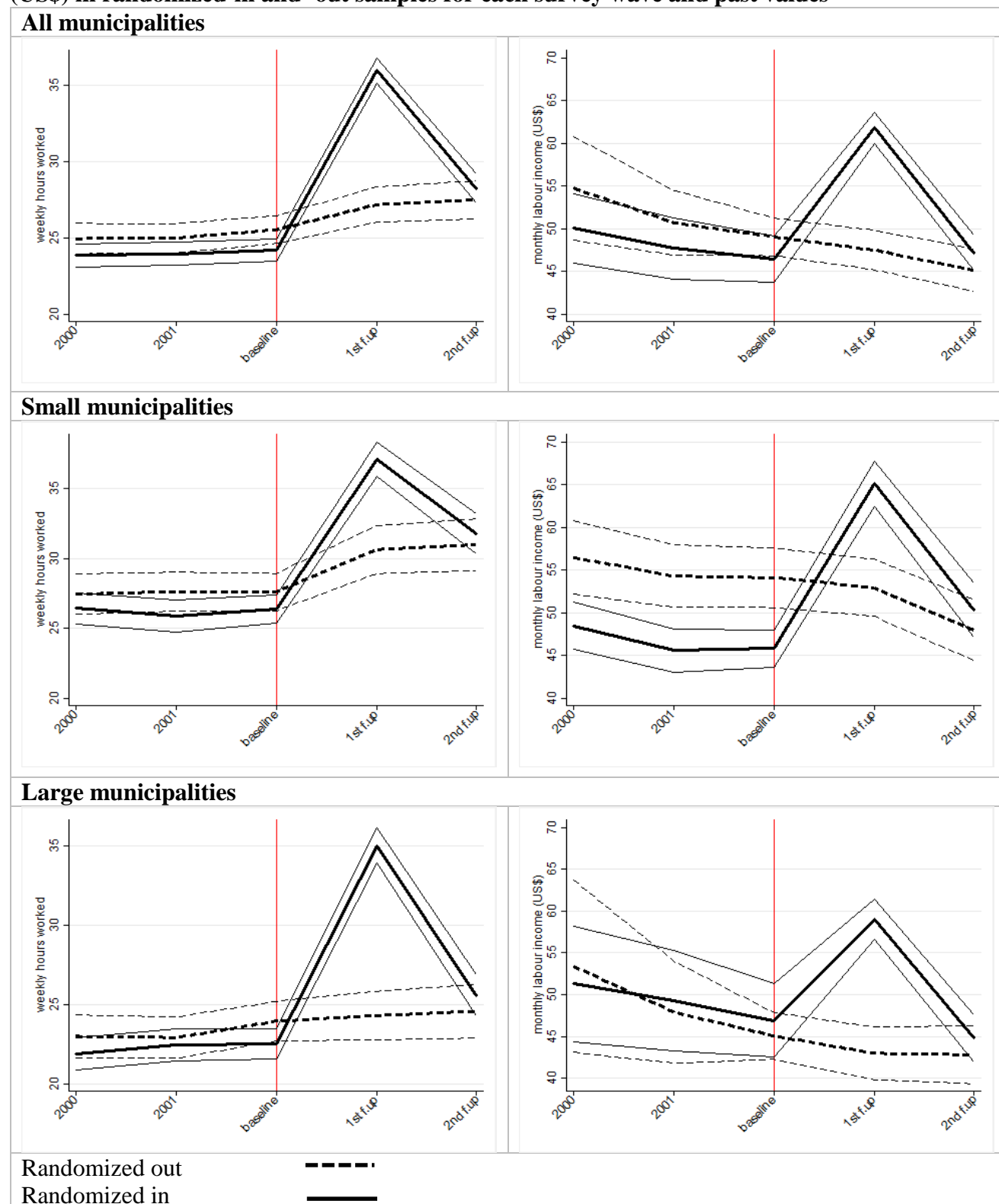
Note: Gini index, rural index (rural population/population) and poverty rate (poverty head count index based on Multidimensional Poverty Index) are from the Municipal Panel Data CEDE, an initiative of the Center of Economic Development Studies (CEDE for its acronymy in Spanish) website.

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

<i>Whole population</i>	<i>N</i>	<i>share (%)</i>	<i>Large municipalities</i>	<i>N</i>	<i>share (%)</i>	<i>Small municipalities</i>	<i>N</i>	<i>share (%)</i>
<i>Out of labor force</i>	1599	37.34	<i>Out of labor force</i>	976	40.1	<i>Indpt self-imp</i>	694	37.55
<i>Indpt self-imp</i>	1255	29.31	<i>Indpt self-imp</i>	561	23.05	<i>Out of labor force</i>	623	33.71
<i>Unemployed</i>	608	14.2	<i>Unemployed</i>	455	18.69	<i>Unemployed</i>	153	8.28
<i>Building</i>	152	3.55	<i>Community</i>	102	4.19	<i>Farming</i>	99	5.36
<i>Community</i>	137	3.2	<i>Building</i>	97	3.99	<i>Building</i>	55	2.98
<i>Farming</i>	127	2.97	<i>Domestic</i>	83	3.41	<i>Commerce</i>	41	2.22
<i>Domestic</i>	113	2.64	<i>Commerce</i>	49	2.01	<i>Business owner</i>	37	2
<i>Commerce</i>	90	2.1	<i>Manufacture</i>	29	1.19	<i>Community</i>	35	1.89
<i>Business owner</i>	49	1.14	<i>Farming</i>	28	1.15	<i>Domestic</i>	30	1.62
<i>Manufacture</i>	46	1.07	<i>Help for free</i>	14	0.58	<i>Help for free</i>	30	1.62
<i>Help for free</i>	44	1.03	<i>Business owner</i>	12	0.49	<i>Manufacture</i>	17	0.92
<i>Transport</i>	21	0.49	<i>Transport</i>			<i>Public work</i>		
<i>Communication</i>			<i>Communication</i>	10	0.41	<i>Public work</i>	14	0.76
<i>Public work</i>	18	0.42	<i>Electr., Gaz, Water</i>			<i>Transport</i>		
<i>Electr., Gaz, Water</i>	9	0.21	<i>Public work</i>	8	0.33	<i>Communication</i>	11	0.6
<i>Teaching</i>	7	0.16	<i>Teaching</i>	4	0.16	<i>Not specific</i>	4	0.22
<i>Not specific</i>	6	0.14	<i>Not specific</i>	4	0.16	<i>Teaching</i>	3	0.16
<i>Mines</i>	1	0.02		2	0.08	<i>Mines</i>	1	0.05
						<i>Electr., Gaz, Water</i>		
						<i>Water</i>	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\$) in 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. Coefficient of *IP* on a regression of differences between 2001 and 2000.**

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

Note: Control variables are education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities, shocks). Robust standard errors in brackets.

ii. *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. Therefore, the contamination of the randomization process and the fact that some projects had already started to operate (and individuals to work on them) when the first data collection took place, can constitute a problem, especially if consider the possibility anticipation effects in consumption.

Having said that, the effects of the program on consumption and, in particular, on consumption in the long run are particularly interesting to establish the overall impact of the program. We will take two approaches. First we will estimate the standard Diff-in-Diff specification using baseline consumption. We will also estimate (1) on the sub-sample of projects that had not started at the baseline survey.

An alternative strategy to estimate the short-run effects of consumption is to estimate the following specification:

$$\log(C_{i1st\text{ f.u.}}) - \log(C_{i2nd\text{ f.u.}}) = \alpha IP_{ik} + \beta X_i + \theta_k + \varepsilon_{ik}, \quad E[\varepsilon_{ikt} | IP_{ik}, X_i, \theta_k] = 0 \quad (2)$$

where the dependent variable is log household consumption in the second wave (1<sup>st</sup> follow up) minus household consumption in the third wave (2<sup>nd</sup> follow up).<sup>15,16</sup> The coefficient on  $IP_{ik}$  in equation (2) represents the difference between the impact when the program was operating (short run) and the long run impact. Unobserved constant (and log-additive) pre-existing differences between the treatment and control individuals due to the failure of randomization should cancel out, as in the standard Diff-in-Diff approach. If the program has no long run impact such a coefficient should be equal to the short run impact. If the long run impact is the same as the short run impact, this coefficient should be zero. In summary, the alternative strategy outlined in (2) will deal with constant and log-additive pre-existing differences at the cost of underestimating the short-term impact if the long-run impact is non-zero.

Throughout the analysis we compute robust standard errors clustered at the project level. 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 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 report results on four sets of hypotheses that our data allows to test. The first two sets are related to transfer and stabilization benefits. We then explore the potential crowding out effects of *EA* hiring. Finally, we describe the ITT effect of *EA* after six months and shed light on potential channels explaining these long lasting impacts.

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

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<sup>15</sup> Our measure of consumption does not include rent because it was not asked in the second wave.

<sup>16</sup> We do not need to do this for labor supply or income because our measure of pre-program labor supply and income refer to 2001, when the projects had not started.

costs of the individual or any other benefits of EA, such as increases in productivity due to public works output<sup>17</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 controls (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 Accion*”, 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). The results show that the program had large positive *transfer* benefits, as the program increased individuals’ hours of work and income while the program was on going. 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\$ (compared to US\$53 and US\$47 per month in the control group for resp. small and large).

Dividing the ITT (19.5 US\$ per month) by 74.7%, the estimate of  $E[P_i|IP_i = 1, X] - E[P_i|IP_i = 0, X]$ <sup>18</sup> from a first stage regression model, gives 26US\$ per month (2SLS robust s.e. 3.2), which is the LATE estimate for the program. These estimates are lower than the ATT estimated in Jalan and Ravallion (2003) and Ravallion et al. (2005) for *Trabajar* (resp. US\$ 157 and US\$140). *EA* impact was 38% of monthly earnings (69 US\$), which is also lower than the impact found in these two studies (their LATE estimate accounting for about half of the *Trabajar* wage) and also lower than Galasso and Ravallion (2004) results on *Jefes* (about two third of the program wage for their preferred specification). These differences might be partly explained by the fact that 25% of participants were already off the program at

<sup>17</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>18</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.



the first follow up, which may lead to lower impact if some become 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*).

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

Dependent variable		All	Small towns	Large Towns
<b><i>Individuals' outcomes</i></b>				
Weekly hours worked		9.68***	9.69***	9.39***
		(0.93)	(1.31)	(1.37)
	<i>pvalues</i>	<.001	<.001	<.001
	<i>N</i>	4918	2238	2680
	<i>Mean (IP=0)</i>	24.68	27.55	22.33
Monthly labour income (US\$)		19.47***	19.35***	19.71**
		(2.53)	(2.71)	(4.23)
	<i>pvalues</i>	<.001	<.001	0.010
	<i>N</i>	4865	2216	2649
	<i>Mean (IP=0)</i>	49.68	52.98	46.99
<b><i>Households' outcomes</i></b>				
Weekly hours worked		14.05***	13.96**	13.79**
		(1.93)	(2.85)	(2.66)
	<i>pvalues</i>	<.001	0.010	0.010
	<i>N</i>	3574	1483	2091
	<i>Mean (IP=0)</i>	64.31	65.96	63.14
Monthly labour income (US\$)		31.01***	25.11**	37.86**
		(5.93)	(7.32)	(9.13)
	<i>pvalues</i>	<.001	0.010	0.010
	<i>N</i>	3456	1449	2007
	<i>Mean (IP=0)</i>	133.23	120.94	141.91

Note: Control variables are 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 (Romano-Wolf adjusted pvalues) Robust Standard errors in parenthesis.

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 more credibility to our results.

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 6, 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 (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 and hours worked remained higher in the second follow up, when the public works programs had already finished: hours at the individual level have increased by 3.9 hours and income by US\$12.<sup>19</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.

**Table 6. Diff-in-Diff estimates of the ITT effect on individuals and household outcomes in second follow up.**

Dependent variable		All	Small towns	Large Towns
<i>Individuals' outcomes</i>				
Weekly hours worked		1.61	3.89	-0.52
		(1.02)	(1.47)	(1.45)
	<i>pvalues</i>	0.247	0.328	0.983
	<i>N</i>	4213	1861	2352
	<i>Mean (IP=0)</i>	24.48	27.07	22.32
Monthly labour income (US\$)		4.81	12.15**	-0.79
		(2.79)	(3.11)	(4.71)
	<i>pvalues</i>	0.247	0.025	0.983
	<i>N</i>	4201	1847	2354
	<i>Mean (IP=0)</i>	49.95	52.95	47.50
<i>Households' outcomes</i>				
Weekly hours worked		4.03	7.37	0.94
		(2.25)	(3.41)	(3.04)
	<i>pvalues</i>	0.247	0.566	0.983
	<i>N</i>	3058	1227	1831
	<i>Mean (IP=0)</i>	63.08	63.45	62.82
Monthly labour income (US\$)		10.06	21.18	3.53
		(6.20)	(6.42)	(10.14)
	<i>pvalues</i>	0.247	0.112	0.983
	<i>N</i>	3046	1230	1816
	<i>Mean (IP=0)</i>	133.9	120.09	143.74

Note: Control variables are 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 (Romano-Wolf adjusted pvalues). Robust standard errors in parenthesis.

<sup>19</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.

We next consider the impacts of the program, both in the short 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. This is the insurance or stabilization property of workfare. 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 we should 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 increase assets, rather than consumption.

**Table 7. Diff-in-Diff estimates of the ITT effect on household's consumption.**

		With additional controls		
Dependent variable		All	Small towns	Large Towns
<i>1st follow up minus baseline</i>				
log consumption	<i>Coeff.</i>	0.01	0.05*	-0.02
	<i>s.e</i>	(0.02)	(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
<i>2nd follow up minus baseline</i>				
log consumption	<i>Coeff.</i>	0.01	0.02	0.01
	<i>s.e</i>	(0.02)	(0.03)	(0.03)
	<i>N</i>	3063	1328	1735
log food consumption	<i>Coeff.</i>	-0.01	0.03	-0.05
	<i>s.e</i>	(0.03)	(0.04)	(0.04)
	<i>N</i>	3965	1744	2221
<i>1st minus 2nd follow up</i>				
log consumption	<i>Coeff.</i>	0.03	0.05	-0.01
	<i>s.e</i>	(0.02)	(0.03)	(0.03)
	<i>N</i>	2873	1276	1597
log food consumption	<i>Coeff.</i>	0.05	0.07*	0.02
	<i>s.e</i>	(0.02)	(0.04)	(0.03)
	<i>N</i>	3788	1703	2085

Note: Control variables are education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities, shocks). Robust standard errors in brackets. \*\*\*  $p < 0.001$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

In the upper panel of Table 7, 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 (+11%) implying (contrary to Engel's "law") 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).

However, when we turn to the longer-term effect, when the program has ended 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 that 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 7.

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 8 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 8. 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

- i. *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”. 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). To address this issue we compare the effects of the program at the individual participant level and at the household level: There is evidence of crowding out if the individual level income and work effort outcomes are larger than the household level ones.<sup>20</sup>

The bottom panel of Tables 9 refers to the ITT effect of the program at household level. The effect of *EA* on household labor hours and earnings is never smaller than at the individual level. There is thus no evidence of *EA* hiring crowding out private labor effort by other household members.<sup>21</sup> The effect seems even larger, which suggests that non-participating household members do profit from the participation, 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.). These results are consistent with self-reported answers to a direct question on crowding-out: in the second follow-up ex-participants were asked if some household members reduced their labor effort to which 99.5% of them answered negatively.

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 9 we find no significant impact, neither in the first, or in the second follow-up survey.

<sup>20</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>21</sup> The average number of participants in households with at least one participant is 1.05.

**Table 9. Diff-in-Diff estimates of the ITT effect on household's monetary and in-kind net transfers.**

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

Note: Control variables are education, gender, age, socio-economic classification of the neighborhood, households' characteristics (demographics, assets and facilities, shocks). Robust standard errors in brackets. \*\*\* p<0.001, \*\* p<0.05, \* p<0.1.

ii. *Investigating the lasting impact of EA*

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

As mention 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 be the case that ex-participants in small municipalities evolved in a less tight labor market where their acquisition of new skills and contacts can help reallocating their labor supply toward better paid sectors or/and with higher demand for workers.

Six months after participation, ex-participants were asked to self-assess the impact of EA (Table A.8). Comparing further small and large municipalities in Table A.8, we find that small municipalities' participants explain more often the ease by objective skills enhancement, like learning a new job or gaining work experience. This is consistent with a high share of the labor force in small municipalities hired on farming work before EA, while the work offered on the projects was mostly related to building activities. Hence, for many participating in EA meant learning new skills. In large municipalities, ex-participants mention more subjective reasons, like getting in contact with someone helping them to find a

job or gaining in self-confidence.<sup>22</sup> Probably, building activities were less novel to them and hence they did not acquire new skills as a result of their participation in EA.

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 reported 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>23</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 previously mentioned, our survey is characterized by a mass point in self-employment and unemployed/out of labor force. All other categories represent a rather small share of the population and it is thus difficult to test the significance of these results. However, 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 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.

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<sup>22</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>23</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).

**Table 10. 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: 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



## 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 results in the empirical literature on workfare program.

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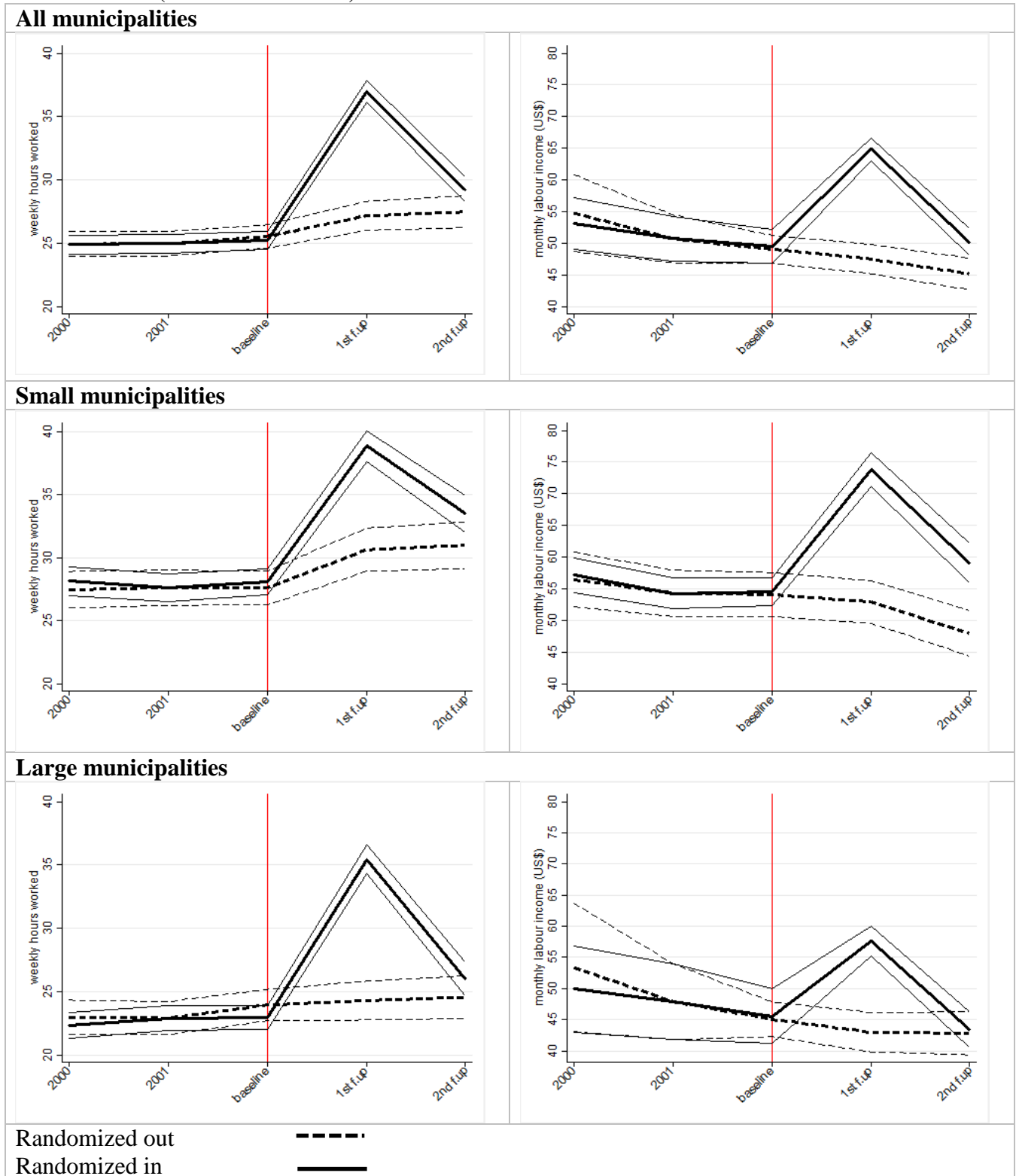
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## 7. Appendix

### i. 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.

ii. *Additional tables*

**Table A. 1. Balance of individual characteristics between those that initially intended to participate and not (beneficiaries) – Difference**

		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 ( = 1 if ...)	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]
Education	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]
	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]
Work	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]
	Number of months worked during 2000	-0.320* [0.149]	0.063 [0.205]	-0.772** [0.215]
		-1.268+ [0.654]	-0.257 [0.907]	-2.463** [0.942]
	Number of hours a week worked during 2001	-0.855 [0.676]	-0.146 [0.936]	-1.689+ [0.977]
	Number of hours a week worked during 2000	-2.14 [2.684]	4.383 [4.507]	-9.891** [2.414]
	Monthly individual labor revenue in 2001 (in dec 2003 pesos),	-2.186 [3.382]	3.857 [5.745]	-9.351** [2.878]
	Monthly individual labor revenue in 2000 (in dec 2003 pesos),			
	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]
		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]	
	1= if housing has	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]
	1= if walls are made of	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]
		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 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]
		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.)**

Cont.)

			All		Large Towns		Small Towns	
			Difference	s.e.	Difference	s.e.	Difference	s.e.
Assets and Properties	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]
			<i>Empleo en Acción - EA</i>	0.539**	[0.00961]	0.664**	[0.0124]	0.392**
Participation in other social programs	1 if any member of the household participates in ..	<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]
			Violence or displacement	0.005	[0.00791]	0.008	[0.0118]	0.003
Health, Education and shocks indicators	1 if household suffered a shock in 2000, 2001 or 2002 due to ...	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.3. Diff-in-Diff estimates of the ITT effect on individuals and household outcomes in first and second follow up without additional controls.**

First follow-up estimates					Second follow-up estimates					
Dependent variable		All	Small towns	Large Towns	Dependent variable		All	Small towns	Large Towns	
<b><i>Individuals' outcomes</i></b>					<b><i>Individuals' outcomes</i></b>					
Weekly hours worked		9.89***	9.55***	10.20***	Weekly hours worked		1.60	3.58*	-0.10	
		(0.92)	(1.30)	(1.30)		N	(1.00)	(1.40)	(1.41)	
		4918	2238	2680				4213	1860	2352
	Mean (IP=0)	24.68	27.55	22.33				Mean (IP=0)	24.48	27.07
Monthly labour income (US\$)		19.10***	19.21***	19.00***	Monthly labour income (US\$)			4.48	11.49***	-1.64
		(2.37)	(2.73)	(3.76)		N	(2.66)	(3.07)	(4.21)	
		4865	2216	2649				4201	1846	2354
	Mean (IP=0)	49.68	52.98	46.99				Mean (IP=0)	49.95	52.95
<b><i>Households' outcomes</i></b>					<b><i>Households' outcomes</i></b>					
Weekly hours worked		13.94***	12.85***	14.75***	Weekly hours worked		3.56	6.11	1.66	
		(1.90)	(2.78)	(2.60)		N	(2.20)	(3.32)	(2.93)	
		3574.00	1483.00	2091.00				3058	1227	1831
	Mean (IP=0)	64.31	65.96	63.14				Mean (IP=0)	63.08	63.45
Monthly labour income (US\$)		31.87***	23.25**	38.41***	Monthly labour income (US\$)			11.23	17.85**	6.22
		(5.92)	(7.42)	(8.77)		N	(6.18)	(6.25)	(9.77)	
		3456.00	1449.00	2007.00				3046	1230	1816
	Mean (IP=0)	133.23	120.94	141.91				Mean (IP=0)	133.9	120.09

Note: Robust standard errors in brackets. \*\*\* p<0.001, \*\* p<0.05, \* p<0.1.



**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
<b><i>1st minus 2nd follow up</i></b>				
log consumption	<i>Coeff.</i>	0.03	0.05	0.01
	<i>s.e</i>	(0.02)	(0.03)	(0.02)
	<i>N</i>	2873	1276	1597
log food consumption	<i>Coeff.</i>	0.04	0.08*	0.01
	<i>s.e</i>	(0.03)	(0.04)	(0.03)
	<i>N</i>	3788	1703	2085

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.**

		Without additional controls			With additional controls		
Dependent variable		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
<b>1st minus 2nd follow up</b>							
log consumption	<i>Coeff.</i>	0.00	0.05	-0.08	0.01	0.06	-0.09
	<i>s.e</i>	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.05)
	<i>N</i>	1121	663	458	1121	663	458
log food consumption	<i>Coeff.</i>	0.04	0.09	-0.05	0.05	0.10	-0.01
	<i>s.e</i>	(0.04)	(0.05)	(0.07)	(0.04)	(0.05)	(0.07)
	<i>N</i>	1442	883	559	1442	883	559

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.**

Small municipalities only							
Dependent variable		Without additional controls			With additional controls		
		All	female	male	All	Female	male
<i>Individuals' outcomes</i>							
Weekly hours worked	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 labor income [US\$]	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: 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: 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: P(Ho: diff = 0)</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**

		PRE BASELINE LABOUR OCCUPATION								
SECOND FOLLOW UP OCCUPATION	<i>Small IP = 0</i>	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
	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
	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>LARGE</i>	Farming	Manufacture	Building	Commerce	Community	Domestic	Indpt self-imp	unemployed	out
	<i>IP = 0</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
SECOND FOLLOW UP OCCUPATION	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>IP = 1</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
SECOND FOLLOW UP OCCUPATION	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.8.