Investigating different benefits of workfare programs

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Abstract

Workfare programs provide a low wage to individuals that work in selected public works. They are designed to provide insurance against job losses by informal sector workers. Ravallion (2003 and 2005) estimates the impact of the Argentinean Workfare program called "Jefes" on the participant's income while she is participating in the program. We analyze the Colombian workfare program called "Empleo en Accion" to shed light on the following issues: (1) whether or not the program crowds out labour effort by members of the household different from the participant, (2) whether or not there are some gains from participating in the program six months after the program has finished, (3) whether or not there are gains in household consumption which is important to asses the role of the program as an insurance mechanism.

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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 in developing countries where government usually lack the capacity and information systems to identify poor and/or the unemployed individuals.

Following Ravallion (1991), workfare programs potentially have two different benefits: a transfer benefit, and a stabilization benefit. 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, crop loss...[1] The existing empirical literature has documented large transfer benefits in workfare programs of India and Argentina, measured by the gains in income from participating in the program (Datt and Ravallion 1994, Jalan and Ravallion 2003, Ravallion *et al* 2005). However, lack of data on household consumption means that the potential stabilization benefits of workfare programs remain unexplored from an empirical perspective. Data on household consumption is required to assess the stabilization benefits of workfare programs because in the absence of the workfare program households might still attain consumption smoothing by reducing assets or receiving transfers. The first contribution of this paper is to document both the transfer and stabilization benefits of a workfare program, *"Empleo en Accion" (EA)* implemented by the Colombian government between ZZZZ and YYYY.

The second contribution of this paper is to study the effect that workfare programs can have in enhancing the labour market opportunities of participants. Participation in a workfare program might help participants to expand their skills and contacts, which might enhance their labour market opportunities even after the program finishes. Ravallion *et al* (2005) already touched on this important issue by testing whether or not there are income gains for no participants who had previously participated in the *Trabajar* workfare programme in Argentina. They cannot reject that there are no income gains

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

after participation though the authors recognize that their test has low power because of their reduced sample size.[2]

Workfare programs might have consequences on the intrahousehold allocation of labour. In the absence of the program, households might offset an individual's unemployment shock by increasing the labour effort exerted by other individuals of the households. Workfare programs might crowd out this labour effort. Datt and Ravallion (1994) find that for one village of the state of Maharashtra in India, the work of men on the farm increases when women participate in the workfare program that they analyse. This is consistent with household members taking up the activities displaced by the workfare program rather than the program crowding out labour effort. We are not aware of any other empirical research that looks at how intrahousehold allocation of labour is affected by workfare programs. However, this is an important issue because it is argued that the cost of using workfare programs is that public-sector work crowds out private sector work, increasing the size of the poverty gap and the costs of poverty alleviation (Besley and Coate 1992). The third contribution of this paper is to investigate whether or not *EA* crowds out labour supply of other adult household members, as well how it affects children school attendance and labour supply. The effect of workfare programs on child schooling is ambiguous from a theoretical perspective because while the program provides income to offset the adverse unemployment shock, the child might need to carry out home production activities for the adults to participate in the program.

The programme and the data

Colombia has recently experienced a lost decade in terms of economic growth as the real GDP per capita in 2004 is roughly the same as in 1995. In response to this severe recession, the Colombian government implemented Empleo en Acción (EA), a workfare program whose main objective was to serve as a safety net. The program consists of subsidizing non-skilled labour of qualifying projects.[3] The nature of the projects ranges from building or repairing roads, sewage system, health infrastructure, education infrastructure, entertainment, sport or cultural venues. They must be proposed by the

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[2] 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 into the project.

[3] The program paid $180,000 (Colombian pesos as of 2001) a month for each individual working part time (24 hours).

local government, NGOs or other community organizations which must cover the non-labour costs of the projects.[4] The maximum duration of each project was 5 months.

Individuals eligible to participate must be older than 17 years old, must not be studying during the morning or afternoon, and must belong to the first or second level of the Colombian Social Classification System (SISBEN).[4] 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. 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.

According to government statistics, 3845 projects were approved for funding but only 3724 projects were carried out, 63% of them in municipalities with less than 100,000 inhabitants. Projects were approved between the end of 2000 and March 2003. The last projects funded by the EA program finished in May 2004.

Three waves of a longitudinal household survey were collected in order to evaluate the impact of EA. The first wave was collected between December 2002 and December 2003. This survey was intended to be a baseline survey. However, though no individuals had been paid by working in the EA project at the time of the survey, in some cases individuals had already started to work in the projects when the survey was collected. 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 we deal with this problem. The second wave of data was collected between March 2003 and February 2004, while the project was carried out. The objective of this second wave is to measure the impact of the program while the participants are working in it. The third wave was collected between June 2004 and September 2004. The projects had finished between 4 and 13 months before this third wave was collected. This third wave is the one that allows us to study the impact of the workfare program once it has finished.

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[4] There were some exceptions for projects proposed by local government

[4] The Colombian Social Classification System, called SISBEN, is widely used as a eligibility tool for social programs in Colombia. There are six possible categories. The first and second one correspond to the poorest in the population.

Within each community, the sample was drawn from the list of individuals that had expressed their will to work for an EA funded project. The local authorities were asked to randomize in order to determine who would participate in the program. From conversations with program officials and field workers, we know that such randomization did not always take place. Moreover, some individuals that were initially allocated to participate in the EA project decided not to participate, in which case replacements were found among the list of those individuals that had expressed their will to participate but were not initially allocated to participate.[5]

Identification Strategy

It is useful to introduce some notation in order to better convey our identification strategy. Let the variable IP_i equals one if the individual i was initially allocated to participate in the EA project and 0 otherwise. Let the variable P_i equals one if the individual i actually participated in the EA project, 0 otherwise. Notice that all the individuals expressed their will to participate in the EA project, irrespective of whether IP_i is one or zero. The joint distribution of IP_i and P_i is given in Table 1.

Table 1. Joint distribution of *P* and *IP*. Number of individuals in each category

	IP=1	*IP=0*
P=1	2734	199
P=0	638	2042

Note: Data refers to the second wave

Our identification strategy should consider two potential problems. First, whether an individual had *IP* equals to 1 or 0 was not entirely randomized and hence the variable could be correlated with unobservable characteristics that also determine our outcome variables. Secondly, the variable (P=1) was not randomized either and who of those with *IP=1* ended up having *P=1* might also depend on unobservable characteristics.

For expositional purposes, consider for the time being that the allocation of individuals to *IP=1* was randomized, but the selection into *P=1* was not randomized. In this case,

[5] In some projects there was a substantial lag between the moment when the list was drawn and the project started.

the impact of the program could be estimated by Instrumental Variables using P_i as treatment variable, and IP_i as an instrument. This would yield a consistent estimate of the impact of the program because IP_i would be orthogonal to unobserved variables that determine the outcome but still there is a correlation between the treatment indicator, P_i, and the instrument, NP_i.

The strategy outlined above might give inconsistent estimates of the impact of the program because the allocation of individuals to *IP=1* was not always randomized, and hence, we cannot rule out that the unobserved characteristics affect both outcome variables and whether or not an individual was allocated *IP* equal to one. In order to remove this source of bias, we will consider differences in the outcome variable in our estimating equation. More formally, we will use the moment condition $E[\Delta\varepsilon_{it} \mid X_i, IP_i] = 0$ to estimate the following equation:

$$\Delta y_{it} = \alpha P_i + \beta X_i + \Delta\varepsilon_{it}, \tag{1}$$

where $\Delta y_{it} = y_{it} - y_{i0}$ is the difference for individual i between the outcome variable in period t and the baseline period, $\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{i0}$ is the difference for individual i between the unobserved determinants of y_{it} and the unobserved determinants of y_{i0}, and X_i is a vector of individual i's time invariant household and individual characteristics. The estimator of α, to which we will refer as IV-DIF-in-DIF, will provide a consistent estimate of the impact of the program as long as there are not time variant unobserved variables that determine both the outcome variable and the allocation of individuals into *IP=1*.

The Ashenfelter's dip is a criticism that often applies to difference in difference estimators as the one that we outline above. According to this, individuals that apply for a welfare program are the ones that experience a temporal dip in their income. Consequently, the difference in difference estimator will overestimate the impact of the program. We will consider this criticism in two different ways. First, both those individuals with *IP=1* and *IP=0* are obtained from the pool of applicants, so both groups would have experienced, in average, the same temporal income when they applied for the program. Second, as baseline measures of income and labour supply, y_{i0}, we will not use income and labour supply as at the time when the first wave of data was collected, but retrospective measures of income and labour supply that, though were collected in the interview of the first wave, refer to the year 2001. The application process took place in the year 2002, so our measure of income and labour supply refers to a period before the temporal dip in income that is contemporaneous with the participation decision. Using the retrospective measure of income and labour supply are useful not only to solve the problem caused by the Ashenfelter's dip, but also because in some cases the first wave of data was collected when individuals were already working in the *EA* project, though they had not started to receive payments for it. A further advantage of using retrospective income and labour supply, instead of the one collected at the first wave of data collection, is that past income and labour supply would have not been affected by expectations on future participation.

Tables A1 and A2 in the appendix compare the characteristics of those with *IP=1* with those of *IP=0*. Table A1 compares basic individual characteristics such as gender, age, education, health indicators, migrant status, and whether or not the individual has recently received training. Table A2 compares variables related to past labour market outcomes. The comparison shows that the differences are very small and non-statistically significant in most cases. Though there is a significant difference in the rate of individuals with a limitation to carry out daily activities in the 15 days previous to the interview, this is non reflected in a longer term measure as the rate of individuals hospitalized in the last 12 months. It is also questionable whether limitation in daily activity in the last 15 days is a predetermined variable or not as knowing that they will be called to participate in a public works programme might affect their behaviour. Our overall conclusion is that the group IP=0 and IP=1 are very similar, at least in observable characteristics. Though we know that the actual allocation did not follow a randomized scheme as it had been planed, it does not seem that the deviations from this randomized benchmark were very important. Moreover, any additive time invariant difference between IP=0 and IP=1 will be purged by using differences-in-differences.

The key assumption of our differences-in-differences identification strategy is that the growth in the outcome variables for those with *IP=1* in the absence of the program had been the same as the growth in the outcome variable experienced by those with *IP=0*. Though this assumption, usually called *common trends assumption,* cannot be tested, it is reassuring that it has hold in the past. In order to check this, we use income and labour supply information for 2001 and 2000 collected retrospectively in the *EA* evaluation survey. Table 2 reports the results of the growth in labour income and hours worked between 2001 and 2000 on the *IP* dummy variable.

Table 2. Common trend assumption. Coefficient of *IP* dummy variable on a regression of differences between labour income (hours worked) between 2001 and 2000.

	Without additional control variables			With additional control variables		
Dependent variable	All	Small towns	Large Towns	All	Small towns	Large Towns
ΔMonthly Labour income(01-00). (US$)	1.19 (2.70)	-0.93 (1.71)	2.85 (4.60)	1.05 (2.76)	-0.95 (1.56)	2.80 (4.85)
Δweekly hours worked(01-00)	0.13 (0.36)	-0.66 (0.46)	0.74 (0.51)	0.11 (0.36)	-0.61 (0.44)	0.61 (0.51)

Standard errors are clustered at the project level. Control variables are: education, gender, age, socio-economic classification of the neighbourhood, and index of municipality public finances

Table 2 shows that the difference in growth in labour income and hours worked between those with IP equals 1 and those with IP equals 0 are small and not statistically different from zero. This goes in favour of our identification assumption. If we estimate the same regressions but using actual participation (P) rather than the *IP* dummy, we reject the hypothesis of common trends, especially in the small towns. We obtain that the growth in hours worked was larger for actual participants than for non-participants. This is important for two reasons. First, it means that our retrospective measures of labour and hours worked are meaningful. Second, it implies that we should exploit the variation in *IP* rather than the variation in *P*.

Results

Table 3 shows the estimates of participation into the program in a individual labour income and labour supply, household labour income and household consumption. The effects refer to two different moments in time: the second column refer to the effects of the program while the projects were still on-going, and the third column refer to the effects of the program once the projects had already finished. The results show that the program had positive *transfer* benefits, as the program increased individual's income and labour supply while the program were on going. The program did not fully crowd out other sources of support and hence household consumption also increases as a result of the program. Consequently, the program also provided *stabilization* benefits. Notice that we estimate the effect of the program on consumption for the subsample of projects that had not started at the moment of the baseline survey.[6]

The effect of the program on household labour income was larger than the effect on the participant's labour income, which indicates that the program had a positive externality in other household members. Consequently, the program was far from crowding out labour supply of other household members. Table 4 shows that the effect of the program is particularly important in female headed households that are usually particularly vulnerable to shocks.

Table 3. Effect of participating on an EA project. IV estimate of α in Equation (1)

Dependent variable:	When projects were still on going	When projects were already finished
Weekly hours of work of participant	8.42** (1.43)	1.60 (1.20)
Monthly labour income in US$ earned by the participant	13.94** (4.81)	3.95 (4.35)
Household monthly labour income (US$).	30.43** (10.94)	22.08** (11.07)
Household consumption (%). (1)	8.91%* (4.74)	14.60** (0.06)

Note: (1) Projects that had not started when wave 1 was collected.
**, * denote statistical significance at the 5% and 10% respectively.

The results in Table 3 show that the program had an important effect on individual's income and labour supply while the projects were still on going, but these effects make much smaller and not statistically different from zero once the projects have already finished. Table 6 shows that the effect of the program once the projects have finished are heterogeneous in the size of the town. The program has improved the labour market prospects of participants in small towns, even once the projects are finished.

[6] We do not need to do this for labour supply or income because our measure of pre program labour supply and income refer to 2001, when the projects had not started.

Table 4. Effect of participating on a EA project (sample of households where the head is female). IV estimate of α in Equation (1)

	When projects were still on going	When projects were already finished
Household monthly labour income (US$), female headed households	39.0* (22.26)	27.22** (13.05)
Household consumption. Female headed households	21.25%** (0.09)	25.80%** (11.42)

Note: **, * denote statistical significance at the 5% and 10% respectively.

Table 5. Effect of participating on a EA project (when projects were still on going). IV estimate of α in Equation (1)

Dependent variable:	Large towns	Small towns
Weekly hours of work of participant	9.31** (1.76)	6.47** (2.25)
Monthly labour income in US$ earned by the participant	9.25 (7.58)	19.44** (4.65)
Household monthly labour income (US$).	38.93** (16.86)	21.35** (8.08)

Note: **, * denote statistical significance at the 5% and 10% respectively.

Table 6. Effect of participating on a EA project (when projects had already finished). IV estimate of α in Equation (1)

Dependent variable:	Large towns	Small towns
Weekly hours of work of participant	-0.13 (1.66)	3.57 (1.69)**
Monthly labour income in US$ earned by the participant	-4.64 (7.24)	13.33** (3.45)
Household monthly labour income (US$).	23.74 (17.58)	18.43** (8.08)

Note: **, * denote statistical significance at the 5% and 10% respectively.

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Appendix

Table A1. Comparison of basic demographics of those with *IP=1* with those of *IP=0*

Variable	Average IP=0	(IP=1)-(IP=0)
1 if Female	0.385 (0.027)	-0.025 (0.025)
Age	35.22 (0.40)	-0.12 (0.42)
1 if limited daily activity in the last 15 days	0.162 (0.010)	-0.044* (0.011)
1 if hospitalized in the last 12 months	0.076 (0.005)	-0.002 (0.006)
1 if immigrant	0.34 (0.021)	0.017 (0.016)
1 if person has no education	0.096 (0.009)	0.012 (0.008)
1 if person only has some primary education	0.281 (0.012)	0.01 (0.013)
1 if person has primary complete but no secondary education	0.223 (0.009)	-0.010 (0.010)
1 if person has studied some secondary education but has not finished	0.239 (0.010)	-0.003 (0.011)
1 if person has finished secondary education	0.137 (0.010)	-0.004 (0.010)
1 if person has more than secondary education	0.017 (0.003)	-0.005 (0.004)
1 if person has recently received training	0.153 (0.008)	-0.004 (0.009)

(*) Statistically significant 95% . Standard errors are clustered at the project level

Table A2 Comparison of past labour market outcomes of those with *IP=1* with those of *IP=0*

Variable	Average IP=0	(IP=1)-(IP=0)
1 if person has ever worked	0.956 (0.008)	0.024* (0.007)
1 if person worked in 2001	0.717 (0.015)	0.008 (0.016)
1 if person worked in 2000	0.688 (0.015)	0.001 (0.014)
Number of months worked in 2001	6.70 (0.173)	-0.288 (0.184)
Number of months worked in 2000	6.561 (0.178)	-0.330 (0.181)
Average of number of hours worked a month in 2001	24.80 (0.739)	-0.969 (0.798)
Average of number of hours worked a month in 2000	22.87 (1.02)	-1.15 (0.903)
Average monthly labour earnings in 2001 (US$)	49.90 (2.07)	-2.05 (2.71)
Average monthly labour earnings in 2000 (US$)	53.11 (2.94)	-3.31 (3.64)

Note: 2600 Colombian pesos= 1 US$. (*) Statistically significant 95% . Standard errors are clustered at the project level