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Welfare programs and labor supply in developing countries: experimental evidence from Latin America

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Abstract This study looks at the effect of welfare programs on work incentives and the adult labor supply in developing countries. The analysis builds on the experimental evaluations of three programs implemented in rural areas: Mexico's *Programa Nacional de Educación, Salud y Alimentación* (PROGRESA), Nicaragua's *Red de Protección Social*, and Honduras' *Programa de Asignación Familiar*. Comparable results for the three countries indicate that the effects that the programs have had on the labor supply of participating adults have been mostly negative but are nonetheless small and not statistically significant. However, the evidence does point to the presence of other effects on labor markets. In the case of PROGRESA, there is a small positive effect on the number of hours worked by female beneficiaries and a sizeable increase in wages among male beneficiaries and a resulting increase in household

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labor income. Moreover, PROGRESA seems to have reduced female labor-force participation in ineligible households. These results imply that large-scale interventions may have broader equilibrium effects.

Keywords Welfare programs · Income support · Labor supply · Work incentives · Conditional cash transfers · Randomized control trials · Developing countries

JEL Classification J08 · J22 · I38

1 Introduction

This study explores the effect of welfare programs on work incentives and the adult labor supply in developing countries. The analysis builds on the experimental evaluations of three programs implemented in rural areas in Latin America: Mexico's *Programa Nacional de Educacion*, *Salud y Alimentacion* (PROGRESA), Nicaragua's *Red de Protección Social* ("Social Protection Network") (RPS), and Honduras' *Programa de Asignación Familiar* ("Family Allowance Program") (PRAF). The study takes advantages of the random assignment of localities to program deployment and control groups and presents comparable estimates of impacts on the adult labor supply and remuneration levels. These estimates are based on homogeneous datasets and were arrived at through the use of a common estimation methodology.

The impact of welfare and income-support programs on labor supply has been widely studied in developed countries (Moffitt 2002; Meghir and Phillips 2008; Moffitt and Scholz 2009). This literature has pointed out the existence of work disincentives among recipient households, and these and other considerations have prompted recent reforms that have incorporated sophisticated measures to mitigate these negative effects (Moffitt 2003a; Blundell and Hoynes 2004; Dickens et al. 2004; Michalopoulos et al. 2005). The programs under study here are conditional cash transfer (CCT) programs, which combine monetary benefits with incentives for curbing child labor and fostering the accumulation of human capital. Benefit receipt is subject to a series of verifiable conditions, such as school attendance, vaccination, and regular medical checkups, among others. The results of a number of evaluations in Latin America indicate that cash transfers, especially when combined with conditionalities, have proved successful in increasing welfare and human capital accumulation in recipient households and in reducing child labor (see the reviews by Rawlings and Rubio (2003, 2005) and Fiszbein and Schady (2009)).

Unlike their recent counterparts in the USA and Europe, however, these programs do not incorporate measures to guard against potential negative impacts on the adult labor supply. Moreover, there is very little consistent, systematic evidence regarding this aspect, despite the existence of a wealth of empirical analyses concerning their intended outcomes. This study attempts to establish whether these cash transfers have any incentive effects on the labor supply of adults in recipient households, on non-eligible individuals, and on the broader labor-market equilibrium.

The main contribution made by this study is the systematic clear-cut evidence that it provides concerning the labor-supply effects of welfare programs in developing

countries. Despite the crucial role played by such programs in the income-generation process among poor segments of the population, there is limited evidence concerning labor-supply decisions in this context. Existing studies point to the presence of complex interactions among public policy, work incentives, and labor allocation within households (see, for instance, Ardington et al. 2009). Moreover, the systematic evidence presented below is derived from experimental evaluation designs,[1] which have clear advantages over the policy and natural experiments underlying most previous studies of welfare programs and labor outcomes (Angrist and Krueger 1999; Blundell and MaCurdy 1999; Imbens et al. 2001; Eissa et al. 2008). These evaluation strategies have also overcome some of the shortcomings of previous randomized experiments, such as those of the negative income tax of the 1960s and 1970s in the USA (Ashenfelter and Plant 1990; Moffitt 2003b).

Comparable results for the three countries indicate that the effects of these programs on the labor supply of participating adults are, while primarily negative, small and nonsignificant. Even though they provided considerable transfers, the programs did not reduce the labor supply substantially in the short term. However, the evidence also reveals the presence of other effects on labor markets. In the case of PROGRESA, there was a small positive effect on the number of hours worked by female beneficiaries, a sizeable increase in wages, especially among male beneficiaries, and a resulting increase in household labor income after the program had been in operation for 2 years. These impacts can be attributed to changes in the labor supply of adults in eligible households and to the increased amount of time available to women as a result of higher school enrollment rates among children. Moreover, PROGRESA seems to have reduced the female labor-force participation rate in ineligible households in the localities randomly assigned to the program. This mechanism is related to recent findings on the indirect impact of CCTs on ineligible households (Angelucci and De Giorgi 2009) and implies that large-scale interventions can have broader equilibrium effects. This additional layer of complexity should be considered in the design and evaluation of future interventions. These equilibrium effects also have important consequences for the interpretation of results from randomized controlled trials (see Moffitt 2003b; Duflo et al. 2008; Heckman 2008; and the debate between Deaton 2009 and Imbens 2010).

The study is organized as follows. Section 2 discusses the theoretical underpinnings of the potential impact of cash transfers on labor supply and presents a review of the empirical evidence for countries in Latin America. Section 3 briefly reviews the programs and their evaluation strategies and then goes on to describe the estimation and inference procedures. Section 4 presents the empirical results on labor-market outcomes for adults in the three programs. Section 5 provides an overview of the results and some conclusions.

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[1]Behrman and Todd (1999), Skoufias et al. (1999), Skoufias and Parker (2001) and Gertler (2004), among others, describe the original experimental evaluation strategy of Mexico's PROGRESA on which the evaluations of RPS and PRAF were based.

2 Labor markets and conditional cash transfer programs

2.1 Potential impacts of CCTs on labor markets

CCT programs combine short-term poverty alleviation (through cash transfers) with long-term outcomes that are achieved through the use of incentives for human capital accumulation (school attendance, health checkups, improved nutrition, and the reduction of child labor).[2] With the exception of minor training components in some programs, the overall design of the CCTs in Latin America is not directly related to the employment of adults in beneficiary households. There are no restrictions on work, and, unlike previous workfare-like initiatives in developing countries, CCTs do not use low-wage jobs as targeting mechanisms (Besley and Coate 1992; Kanbur et al. 1994). Most importantly, earned labor income does not reduce benefit levels. In this sense, CCTs constitute a simpler policy instrument than welfare programs in developed countries: as a pure subsidy (as far as adults are concerned—although some of the conditionalities might imply some costs in terms of time), CCTs do not induce steep replacement rates as traditional welfare programs do, nor do they entail the complexity of welfare-to-work initiatives such as the Earned Income Tax Credit in the US (Eissa and Liebman 1996) or the UK's Working Families' Tax Credit (Meghir and Phillips 2008).[3]

The lack of work requirements does not mean, however, that the programs are neutral in terms of adult labor supply and work incentives. The income-support component and the conditionalities relating to children's health and education might still have affected these outcomes. The economic theory suggests several ways in which CCTs can affect work decisions within recipient households. In a standard static model of choice between consumption and leisure, the components of CCTs may play a role through at least four channels.

Firstly, the cash transfer component of the program generates an increase in unearned non-labor income. As such, it induces a pure income effect, which loosens the budget constraint of the recipient households. The rise in unearned income could reduce the number of hours of work if leisure is a normal good for beneficiaries, but the presence of fixed hour or money costs, such as commuting or childcare (Cogan 1981; Bhattarai and Whalley 2003; Kluve and Tamm 2012), may induce an increase in labor supply as a result of the lump-sum transfer (Ralitza and Wolff 2011).

Conditionalities constitute the second channel through which CCTs may induce behavioral responses in the adult labor supply. The requirements related to children's human capital accumulation may have an impact on a household's allocation of time: the positive impact of CCTs on children's school attendance could free up time previously spent on childcare (Blau and Tekin 2007; Baker et al. 2008; Mörk et al. 2013),

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[2]A more detailed description of the three programs is provided in Section 3.1 and in the Electronic Supplementary Material.

[3]Moreover, program overlap is less of a problem for program evaluation (Moffitt 2002) in the cases under study: PROGRESA consolidated several different programs in Mexico, while PRAF and RPS represented some of the first attempts made to provide widespread income support in Honduras and Nicaragua.

thereby further reducing the cost of work. There is some evidence of reduced participation of women in domestic work in the case of PROGRESA (Parker and Skoufias 2000; Skoufias and Parker 2006).

The third channel is related to the potential decrease in household income associated with the reduction in child labor (Basu and Hoang Van 1998). This effect diminishes the net impact of cash transfers in households where children are induced to reduce their participation in work activities and could therefore mitigate the transfer's potential disincentive in terms of the adult labor supply.[4]

Finally, the fourth channel operates through different types of spillovers. On the one hand, there may be indirect effects: Angelucci and De Giorgi (2009), for instance, find that PROGRESA has had an impact on the consumption of ineligible households in program communities, and Bobonis and Finan (2009) report substantial spillovers in terms of secondary school enrollment decisions for the same program. On the other hand, changes in the labor supply schedule of beneficiaries may affect aggregate wage levels and thus remunerations for both recipients and non-recipients. In the presence of such effects, the identification strategy based on the random allocation of the program would be partially compromised owing to a violation of the stable unit treatment value assumption (Angrist et al. 1996). In terms of the labor supply, equilibrium effects reduce the scope for the interpretation of reduced form estimates as simple labor supply elasticities with respect to unearned income.

The combination of these four channels implies that the overall effect of CCTs on labor market outcomes for adults is ambiguous from a theoretical point of view. The presence of any impact, and its direction, is ultimately an empirical question.

2.2 The impact of CCTs on labor markets: previous findings for Latin America

Most of the literature on the impact of CCTs focuses on the programs' intended outcomes. While results vary from country to country, program evaluations reveal, to some degree, a positive effect on years of schooling, reductions in child labor, and improvements in some key health indicators (Rawlings and Rubio 2003, 2005; Bouillon and Tejerina 2006; Fiszbein and Schady 2009), as well as other related unintended effects on, for instance, fertility (Todd et al. 2011).

Effects on the adult labor supply have been partially analyzed for PROGRESA and RPS. The significant reduction in child labor found in the case of PROGRESA (Skoufias and Parker 2001) contrasts with the absence of an impact on labor market outcomes for adults in beneficiary households, according to results from Parker and Skoufias (2000) and Skoufias and Di Maro (2008). Both studies, based on probit estimations, find no significant program effects on adult labor-force participation

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[4]Yang's (2008) results for remittances in the Philippines do not point up any significant impacts of windfall income on the adult labor supply. However, the findings of Ardington et al. (2009) concerning migration from South Africa indicate that transfers may influence even more complex within-household interactions, thereby inducing unexpected labor-supply responses.

within eligible households in program localities. Also in the context of PROGRESA, Angelucci and De Giorgi (2009) find that household equivalent labor earnings for adults are not affected by the program. This study provides further results from estimations that include fixed effects at the household or individual levels. While these controls may not be strictly necessary in the context of a randomized controlled trial, they allow for better controls for baseline differences in employment (see Table A2 and Behrman and Hoddinott 2005) and may also induce a gain in precision (Duflo et al. 2008). The results include these additional controls and confirm the robustness of previous findings.

The impact of Nicaragua's RPS program on the adult labor supply is analyzed in detail by Maluccio (2007). While the studies on PROGRESA referred to in the previous paragraph concentrated on individual labor-force participation and household earnings, Maluccio (2007) studies the effect of RPS on total hours of work at the household level. The results, obtained by means of a random effects model, indicate that the program has had a small but significant negative effect on total household hours of work, with most of the negative impact relating to the amount of time spent in agricultural activities. These effects and their causes are discussed in detail below, with the evidence being presented here pointing to a household composition effect rather than a direct effect on hours worked.

There are fewer papers that draw upon evaluation data for Honduras' PRAF program. Galiani and McEwan (2012) developed an original evaluation strategy based on census data instead, which was collected shortly after the program was implemented. They report no significant effects for PRAF on the labor supply of adult women and only a small decrease (1 %) for adult males, although this estimate is not robust to alternative specifications.

The analysis presented in this study provides comparable results for the three programs. They are based on a common procedure for processing the original datasets, which leads to homogeneous definitions for dependent and independent variables. Moreover, the estimates for the three programs are derived from the same methodology and allow for the same type of controls for randomization imbalances and other issues by including individual and household fixed effects. Finally, while evaluations of PROGRESA and PRAF have concentrated on individual participation and those of RPS on household hours, the results detailed below allow for further disaggregation in order to look at participation, hours of work, sector allocation, household labor earnings, and wages (when possible) for all programs.

3 Experimental evaluation strategies and estimation methodology

3.1 The programs and their evaluations

The data used in this analysis are drawn from ad hoc longitudinal surveys carried out in order to evaluate each specific intervention. The three programs share a common evaluation methodology, with baseline and follow-up data collection being conducted in localities that were randomly assigned to program deployment and in those that were selected into the control group. The three data sources were harmonized on the

basis of a common set of criteria in order to achieve maximum comparability using the methodology described in CEDLAS (2012).

The three interventions targeted rural areas in poor regions of the respective countries. The following paragraphs briefly describe the three programs' evaluation strategies,[5] based on PROGRESA's experimental design, which randomized program deployment at the locality level.

In 1997, Mexico began implementing the first phase of PROGRESA. It was geographically targeted by locality. From an initial group of the 506 localities that were selected for the first round, 320 were randomly selected to participate in the program, which was not deployed in the remaining 186 localities. Households in the latter localities were still subject to the data collection process and thus constituted the control group for the program's evaluation. The intervention also included a targeting rule based on a proxy means test: only qualifying households in treatment localities were eligible to participate.

The data employed in this study are drawn from the PROGRESA Evaluation Survey. The estimates discussed below are based on the initial baseline survey and on three follow-up rounds[6] conducted at 6-month intervals following program implementation. The surveys collected sociodemographic and labor-market information for all households and individuals in both treatment and control communities.

Honduras' PRAF was implemented in a set of 50 randomly selected municipalities of a total of 70, with the 20 remaining municipalities forming the control group. The data used in this study correspond to a baseline survey carried out in the second half of 2000 and a follow-up survey in 2002. In contrast with PROGRESA, where all households in treatment and control localities were interviewed, the PRAF surveys covered only a representative sample of households. The corresponding sampling weights are used in the empirical work outlined below.

For the case of Nicaragua's RPS, half of the countries' poorest 42 localities were randomly assigned to the treatment group. The data used in this study are drawn from the initial baseline survey carried out in the third quarter of 2000 and the first and second follow-up surveys conducted in October 2001 and October 2002, respectively. As with the PRAF evaluation data, the survey consists of a representative sample of the population in treatment and control localities, and sampling weights are used for the estimations.

Finally, although the programs have a number of characteristics in common, it should be noted that there were significant differences in the average size of the cash transfers provided by each of the initiatives. Imputing transfer values from each program's eligibility rules to the evaluation samples used in this analysis, the transfers represented about 4 % of total household consumption for PRAF, 20 % for RPS, and

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[5]The structure of each program is detailed in the Electronic Supplementary Material to this study. Further references may also be found in Todd (2004) for PROGRESA, Glewwe and Olinto (2004) and IFPRI (2000) for PRAF, and Maluccio and Flores (2005) for RPS.

[6]Baseline data were gathered between November 1997 and March 1998. The first, second, and third follow-ups correspond to November 1998, March 1999, and November 1999, respectively.

40 % for PROGRESA.[7] The potential effect of these differences is discussed in the section that covers the empirical results.

3.2 Estimation and inference with random assignment by locality

In view of the random assignment of localities in the context of the three programs under study and the availability of repeated observations, a differences-in-differences (DD) estimation technique is the most suitable one of exploiting the evaluation design and identifying the causal effects of the programs. A standard DD model with controls takes the form of

$$Y_{ist} = A_s + B_t + cX_{ist} + \beta I_{st} + \varepsilon_{ist} \tag{1}$$

where Y_{ist} denotes the outcome variable of interest for individual (or household) i in group (or village) s at time t, I_{st} is an indicator variable representing treatment status for group s in time t (or alternatively, an interaction between a treatment group indicator and time effects), A_s and B_t are group and time effects, respectively, X_{ist} is a matrix of individual characteristics, and ε_{ist} is an error term. The estimate of the program impact is the coefficient β. Without the X_{ist} controls and with two time periods, the estimate of β by ordinary least squares (OLS) is simply the difference in changes in mean outcomes between the treatment and control groups between the two time periods. The more general case, with more than two time periods, adds a full set of time controls and interactions to account for differential evolutions over time.

The canonical DD model of Eq. 1 without including individual controls X_{ist} provides estimates of β that amount to differences in the outcomes at the locality level. The evaluation of PROGRESA, PRAF, and RPS, however, collected repeated household and individual observations, which means that a much richer set of information is available and can be exploited (Wooldridge 2001, 2007). Specifically, the inclusion of individual (or household) fixed effects in the estimation of Eq. 1 permits the identification of program effects while controlling for some of the pretreatment differences between localities (see Table 1, discussed below, and in the Electronic Supplementary Material). Moreover, this allows for a potential gain in precision (Duflo et al. 2008). While these individual fixed effects were not accounted for in the studies of labor supply reviewed in the previous section, they are routinely included in evaluations of CCT impacts on other outcomes (for instance, in Gertler's 2004 evaluation of PROGRESA's effect on health, among many others). The results discussed below provides two sets of estimates for each outcome based on Eq. 1: one with a full set of individual controls X_{ist}, and one with a full set of individual fixed effects but no time invariant X_{ist} variables.

With respect to the estimation methodology, the empirical results presented below are based on linear models—either OLS or fixed effect (FE) estimations of Eq. 1— for binary dependent variables such as labor-force participation and for continuous variables such as hours of work, wages, and income. As pointed out by Angrist

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[7]These estimates are roughly in line with others given in the literature: Maluccio (2004) reports 4 % for PRAF, 18 % for RPS, and 20 % for PROGRESA, although, for the latter, Gertler (2004) computes the average transfer as one third of total household income.

Table 1 Descriptive statistics, by program

	PRAF (Baseline: Aug.–Dec. 2000)		PROGRESA (Baseline: Sep. 1997–Mar. 1998)		RPS (Baseline: Aug.–Sep. 2000)	
	Treatment	Control	Treatment	Control	Treatment	Control
Households						
All	34,455	29,538	14,856	9,221	5,781	4,203
Eligible (%)			52.8	50.8		
Spouse present (%)	77.4 (1.06)	73.9 (1.10)	81.9 (0.32)	82.3 (0.40)	81.4 (1.37)	79.4 (1.46)
Mean household size	6.6 (0.03)	6.6 (0.03)	6.5 (0.01)	6.5 (0.01)	7.5 (0.05)	7.5 (0.05)
Mean number of children	3.4 (0.03)	3.2 (0.03)	3.3 (0.01)	3.2 (0.01)	3.3 (0.04)	3.3 (0.03)
Individuals						
Years of education						
All	3.4 (0.05)	2.9 (0.04)	5.2 (0.02)	5.1 (0.02)	2.3 (0.05)	2.2 (0.05)
Females	3.4 (0.07)	2.7 (0.06)	5.1 (0.02)	5.1 (0.03)	2.3 (0.07)	2.3 (0.07)
Males	3.4 (0.06)	3.1 (0.06)	5.2 (0.02)	5.1 (0.03)	2.2 (0.07)	2.1 (0.07)
Employment						
All	66.3 (0.78)	64.5 (0.76)	53.1 (0.24)	51.2 (0.30)	56.2 (0.98)	57.7 (1.00)
Females	34.4 (1.22)	32.3 (1.13)	19.5 (0.27)	16.8 (0.31)	20.4 (1.15)	23.2 (1.22)
Males	90.0 (0.64)	88.3 (0.67)	86.4 (0.23)	85.7 (0.30)	90.5 (0.81)	91.7 (0.79)
Households with spouse	66.0 (0.87)	65.3 (0.85)	52.4 (0.26)	50.8 (0.32)	56.0 (1.07)	57.1 (1.10)
Households without spouse	67.4 (1.77)	61.3 (1.64)	57.4 (0.62)	53.5 (0.79)	57.0 (2.54)	60.5 (2.42)
Households with children	67.2 (0.87)	66.1 (0.86)	53.2 (0.27)	51.6 (0.34)	56.8 (1.11)	58.4 (1.13)
Households without children	62.9 (1.75)	59.3 (1.59)	52.8 (0.49)	49.9 (0.61)	53.8 (2.13)	55.3 (2.19)

Table 1 (continued)

	PRAF (Baseline: Aug.–Dec. 2000)		PROGRESA (Baseline: Sep. 1997–Mar. 1998)		RPS (Baseline: Aug.–Sep. 2000)	
	Treatment	Control	Treatment	Control	Treatment	Control
Agricultural workers						
All	65.3 (0.98)	64.4 (0.95)	76.9 (0.35)	74.7 (0.44)	83.7 (0.98)	80.9 (1.06)
Females	33.0 (2.32)	33.8 (2.02)	41.8 (1.09)	32.7 (1.28)	57.2 (3.25)	42.2 (3.11)
Males	74.5 (0.99)	72.7 (0.99)	82.6 (0.34)	81.3 (0.42)	89.2 (0.90)	90.5 (0.88)
Mean hours of work						
All	38.2 (0.21)	37.7 (0.20)	43.4 (0.10)	43.7 (0.12)	39.4 (0.42)	38.0 (0.44)
Females	33.0 (0.69)	32.4 (0.59)	41.5 (0.28)	42.3 (0.39)	33.1 (1.28)	31.2 (1.17)
Males	39.6 (0.19)	39.1 (0.19)	43.8 (0.10)	44.0 (0.12)	41.0 (0.40)	40.2 (0.43)

Source: Own calculations based on program evaluation surveys
Standard errors are shown in parentheses

and Pischke (2008), linear probability model estimates do not differ substantially from those of probit or logit regressions. Moreover, coefficients for the indicator and interaction variables in Eq. 1 have a straightforward causal interpretation for linear estimates.

All the results presented below give estimates of β in Eq. 1 over the full treatment and control samples, which correspond to intention-to-treat (ITT) coefficients. In the case of PROGRESA, the dataset contains a multidimensional targeting score which was used as a proxy means test for participation within program localities, thus making it possible for the eligibility status of each household to be known. For this reason, PROGRESA's results are also computed as differences between eligible households in treatment and control localities (average treatment effect—ATE)[8] and differences between ineligible households in the two sets of localities. The latter estimates correspond to Angelucci and De Giorgi's (2009) indirect treatment effects (ITE).[9] To account for potential heterogeneous effects of the programs, the estimations are also computed by conditioning on the gender of the individual or the household head, as an alternative to the inclusion of multiple interactions (Djebbari and Smith 2008).

Finally, the standard errors in the estimations need to account for the structure of the programs' evaluation and implementation processes. In the context of the three CCTs under study, random assignment did not apply directly over beneficiary households or individuals. The allocation was instead done at the geographical level. In terms of the equation above, randomization occurs at the group (village) level (s) instead of the individual or household level (i). Since eligibility for the program is defined at the group level, the standard errors of the DD estimates should account for the likely intra-cluster correlation to avoid a potential bias. Donald and Lang (2007) attribute this bias to the fact that many of the outcomes analyzed in the literature are serially correlated, which is not usually controlled for in DD estimations (see the discussion in Bertrand et al. (2004, BDM henceforth)). This issue may be particularly important in the case of the labor-market outcomes covered in this study. A failure to account for this correlation across the randomization groups makes the usual OLS standard errors inconsistent and leads to erroneous inferences of the program's causal effects.

BDM propose two methods to correct the standard errors of estimates in Eq. 1:[10] (a) taking into account the serial correlation of the outcome variable in each group s (this is known as cluster-robust variance estimation (CRVE) and is implemented by clustering observations by the assignment groups (e.g., localities)) and (b) estimating standard errors using block bootstrap with replacement. The first method was used to arrive at the empirical results presented below; the standard errors

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[8]Since take-up was very high among eligible households, average treatment effects, and average treatment effects on the treated are roughly equivalent (Angelucci and De Giorgi 2009). For simplicity, the ATE terminology is adopted in the description of the results.

[9]Angelucci and De Giorgi (2009) exclude from their analysis a subset of those deemed ineligible in the initial phase of the program because of later changes in the eligibility rules. The analysis here follows Duflo et al. (2008) in exploiting only the primary assignment process regardless of changes in the program rules after the initial stage.

[10]BDM also propose a third correction that involves aggregating the data into group-year cells and estimating this model. However, only results from individual-level data are reported below.

are virtually equal to those obtained from block bootstrapped standard errors.[11] These corrections to the covariance matrix yield unbiased estimates of household- or individual-level outcomes in geographic targeting settings, thus accounting for potential serial correlation across groups.

4 CCTs and labor-market outcomes for adults

4.1 Descriptive statistics and random assignment processes

This section discusses the empirical evidence regarding the effect of CCT programs on labor outcomes for adults using experimental evaluation data from the three interventions detailed above: PRAF (Honduras), PROGRESA (Mexico), and RPS (Nicaragua).

Table 1 presents a series of descriptive statistics compiled at the time of the baseline survey for both treatment and control localities for these three programs. These statistics make it possible to verify the nature of the balance between the treatment and control groups in terms of observables. As expected in a rural setting in developing countries, household size in all three programs is fairly large, with an average of more than six individuals per household. About 70–80 % of these households include two spouses. The calculations in the tables show that treatment and control households are not significantly different in terms of their demographic composition, with only a few small significant differences for some variables.

Table 1 also presents average educational levels for the treatment and control localities in each program. Since the programs are targeted at poor areas in each country, the distribution of educational outcomes is concentrated in lower levels of attainment, with about 5 years of education for PROGRESA, 2.2–2.3 for RPS, and 3.4–2.9 for PRAF. The differences in educational achievement and enrollment rates between the treatment and control groups are small, except in the case of PRAF.

Finally, with respect to labor-market outcomes, the program datasets allow for nothing more than a simple definition of participation: individuals report if they work or if they do not. Employment ranges from 51.2 to 53.1 (PROGRESA) to 56.2–57.7 (RPS) and to 66.3–64.5 (PRAF). It is substantially higher for men than for women in the samples for the three countries (a difference of about 55 percentage points in PRAF and of about 70 percentage points in PROGRESA and in RPS). Employment is also higher in households with children and in single-headed households.

The unconditional means of socioeconomic and demographic statistics indicate some preprogram differences between treatment and control groups at the individual and household levels.[12] These results are generally in line with preexisting reports

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[11] The working-paper version of this document (Alzua et al. 2010) presents the two sets of standard errors, with estimates following the suggestion of Cameron et al. (2008) of reporting bootstrapped CRVE-corrected standard errors.

[12] This is also apparent in a conditional framework, as discussed in the Electronic Supplementary Material in respect of the analysis of the random assignment process, which indicates that the resulting treatment and control localities have significant differences in some dimensions for the three programs.

on these programs, which also found some significant differences between treatment and control localities (see Behrman and Todd 1999, for PROGRESA; Glewwe and Olinto 2004, for PRAF; and Maluccio and Flores 2005, for RPS). Given the nature of the random assignment process in the three programs, implemented at the locality level, these differences probably arise because of the small number of effectively randomized units (localities). These differences reflect the composition of the resulting samples rather than their selection into treatment. In any case, the estimations discussed below control for individual characteristics or, alternatively, for individual fixed effects to account for the ex post differences in the treatment and control samples.

4.2 The effect of CCTs on labor-market outcomes for adults

The analysis of labor-market outcomes for adults in the three programs is restricted to a common sample selection criterion which includes individuals between 15 and 80 years old. Estimates of household-level outcomes are restricted to household heads in the same age range.

The original evaluations focused primarily on each program's intended outcomes, such as children's health and education. The evaluation surveys have a much smaller set of labor-market indicators than larger periodic surveys use. In the three data sources employed in this study, the adult population can be divided into two alternate categories of labor-market status: those who work outside the home and those who do not.[13] The discussion refers to work, employment and labor supply interchangeably.

There are other labor-market outcomes of interest, besides employment status, that can be explored using these evaluation datasets: the number of hours worked in all occupations in a week (for those with positive hours); an indicator for employment in agricultural activities (for those employed); and the total hours worked in the household by members from 15 to 80 years of age (this variable is computed and estimated at the household level for households with positive hours).

As stated in the previous section, the results correspond to two alternative specifications for each outcome of interest. On the one hand, the tables report OLS estimates β in Eq. 1 with a series of controls:

- Controls for individual characteristics: gender (if applicable), household size, an indicator for two-parent households, number of children, age of the individual, age squared, and educational indicators (complete primary through complete university).
- Controls for household characteristics: the gender of the household head (if applicable), household size, an indicator for two-parent households, number of children of the head of household, a dummy variable indicating if at least one child in the household attends school, and indicators for the household head's educational level.

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[13] It is, thus, not possible to distinguish between inactivity and unemployment. This distinction is feasible for the RPS data, but in the interests of comparability, the results detailed below are reported for the same variable for the three programs.

Table 2 Program effect on employment: PRAF and RPS

	DD estimates					
	ITT		ITT males		ITT females	
	OLS	FE	OLS	FE	OLS	FE
PRAF (Baseline: Aug.–Dec. 2000)						
$t = 1$ (May–Aug. 2002)	−0.011	−0.015	−0.005	−0.012	−0.010	−0.018
	(0.016)	(0.015)	(0.018)	(0.018)	(0.023)	(0.028)
Observations	12,833	12,482	7,145	6,930	5,688	5,552
Groups		7,484		3,918		3,569
RPS (Baseline: Aug.–Sep. 2000)						
$t = 1$ (Oct. 2001)	−0.005	−0.002	0.009	0.006	−0.022	−0.010
	(0.020)	(0.020)	(0.020)	(0.020)	(0.030)	(0.029)
$t = 2$ (Oct. 2002)	−0.012	−0.013	−0.009	−0.005	−0.020	−0.023
	(0.018)	(0.019)	(0.014)	(0.014)	(0.030)	(0.031)
Observations	11,241	11,287	5,828	5,852	5,413	5,435
Groups		4,426		2,300		2,126

Source: Own calculations based on program evaluation surveys
Standard errors, clustered at the locality level, are shown in parentheses
$*p = 0.10$, $**p = 0.05$, $***p = 0.01$ (significant)

The FE estimations, on the other hand, do not include any individual controls, since most of those listed above are time invariant or have low variability. All estimations include time effects, treatment indicators, interactions between the two, and locality controls, with standard errors clustered at the locality level. Finally, the results present estimates for the ITT for the three programs and for males and females separately. For the specific case of PROGRESA, the availability of eligibility status data means that average treatment effects (ATE) and ITE can also be computed. The tables only report the relevant coefficient for the treatment effects (the coefficient β).

The estimates for PRAF correspond to the simple two-period case (baseline in second half of 2000, follow-up in May–August 2002), while estimates for RPS and PROGRESA include multiple consecutive follow-up surveys.[14] The RPS baseline was established in August–September 2000, with a first follow-up in October 2001 and a second one in October 2002. For PROGRESA, the baseline corresponds to September 1997–March 1998, while the follow-up data were collected in November 1998, March 1999, and November 1999.

The evidence concerning the main theoretical question—the impact of each program on employment—is presented in Tables 2 and 3, which show the estimated

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[14]The tables report the effect by round of the evaluation survey and correspond to the difference between the round and the baseline (preprogram) levels. These effects are estimated jointly by multiple time and treatment interactions, not as separate regressions by follow-up period.

Table 3 Program effect on employment: PROGRESA

	ITT		ATE		ITE		ITT males		ITT females		ATE males		ATE females		ITE males		ITE females	
DD estimates																		
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
PROGRESA (Baseline: Sep. 1997–Mar. 1998)																		
t = 1	−0.008	−0.008	−0.003	−0.003	−0.015	−0.015	−0.002	0.003	−0.012	−0.010	0.008	0.005	−0.008	−0.001	−0.012	−0.009	−0.020	−0.019
(Nov. 1998)	(0.008)	(0.008)	(0.010)	(0.010)	(0.009)	(0.009)*	(0.009)	(0.008)	(0.015)	(0.013)	(0.012)	(0.010)	(0.019)	(0.016)	(0.011)	(0.011)	(0.017)	(0.017)
t = 2	−0.014	−0.009	−0.012	−0.009	−0.026	−0.014	0.000	0.004	−0.024	−0.020	0.002	0.000	−0.019	−0.021	−0.002	−0.001	−0.046	−0.020
(Mar. 1999)	(0.007)**	(0.009)	(0.009)	(0.009)	(0.011)**	(0.010)	(0.009)	(0.007)	(0.016)	(0.011)*	(0.011)	(0.009)	(0.017)	(0.015)	(0.014)	(0.012)	(0.021)**	(0.017)
t = 3	−0.009	−0.006	−0.005	−0.005	−0.023	−0.016	0.003	−0.001	−0.020	−0.010	0.005	−0.002	−0.011	−0.003	−0.004	−0.009	−0.049	−0.035
(Nov. 1999)	(0.007)	(0.007)	(0.009)	(0.009)	(0.011)**	(0.010)*	(0.009)	(0.006)	(0.015)	(0.013)	(0.011)	(0.008)	(0.017)	(0.016)	(0.012)	(0.012)	(0.020)**	(0.020)*
Observations	227,619	232,725	141,271	143,347	84,052	87,044	114,462	114,837	113,157	113,587	70,812	71,013	70,459	70,697	42,545	42,700	41,507	41,680
Groups		72,933		50,636		34,344		61,144		60,632		40,646		40,304		26,401		26,090

Source: Own calculations based on program evaluation surveys

Standard errors, clustered at the locality level, are shown in parentheses

* $p = 0.10$, ** $p = 0.05$, *** $p = 0.01$ (significant)

coefficient of the treatment period/treatment status interaction in Eq. 1. Table 2 presents the results for Honduras' PRAF and Nicaragua's RPS programs. None of the estimates of the programs' effects on employment are statistically different from zero at standard significance levels. The estimates range from −0.5 to −1.8 percentage points for PRAF and from −0.2 to −2.3 percentage points for RPS (with positive effects of 0.6–0.9 points for males in the first follow-up survey). In all cases, the effects are higher in terms of absolute value for females than for males.

Table 3 presents the results for PROGRESA. The coefficients on employment, estimated jointly for males and females, are also negative and are in a similar range to those reported in Table 2 (from about −0.3 to −2.6 percentage points). Despite some statistically significant coefficients, there does not appear to be a consistent pattern of significant results for all three follow-up periods or for both OLS and FE estimations. The overall and average treatment effects are compatible with a setting in which income effects are either small or counterbalanced by other forces, as discussed in Section 2.

However, the ITE estimates (effects on individuals in ineligible households) exhibit a higher degree of significance, which seems to be driven mostly by a large fall in employment among ineligible females in the third follow-up survey (about 3.5–4.9 percentage points, for FE and OLS estimations, respectively). While this result may be a statistical artifact, additional results regarding type of employment, hours and household labor income indicate that there may be composition effects within households and between eligible and ineligible individuals. These overall effects are discussed in detail below following the presentation of the rest of the empirical evidence.

Besides their effect on overall employment, the programs may also affect occupational choice. For instance, Skoufias et al. (2008) find that the *Programa de Apoyo Alimentario* ("Food Support Program) (PAL) program in Mexico induced workers to move away from agricultural work, which supports the idea that this kind of work acts as food insurance. Tables 4 and 5 present the results for regressions for employed individuals, in which the dependent variable is an indicator equal to one if they work in agricultural occupations and is zero otherwise.

The coefficients reported in Table 4 indicate that neither PRAF nor RPS induced a substantial shift in labor allocation to agricultural or other sectors at the aggregate level. The coefficients for the overall population are negative for PRAF and their sign for RPS depends on the estimation method that is used, but they are not statistically significant for either of the two programs. Table 5, however, indicates a positive, significant and large effect on agricultural employment in Mexico for ineligible males in the second and third follow-up rounds (of 6.1 and 4.5 percentage points, respectively), although these effects are statistically different from zero only for the FE estimates. In contrast, the average treatment effect for males (the effect on those eligible) is substantially closer to zero (ranging from −0.8 to 1.5 percentage points) and not significant at standard levels for any of the three treatment periods. This reinforces the existing evidence of the presence of composition effects by household and by eligibility status.

A more detailed picture of labor-market effects emerges from the analysis of Tables 6 and 7, which present regressions in which the dependent variable is the

Table 4 Program effect on agricultural employment: PRAF and RPS

	DD estimates					
	ITT		ITT males		ITT females	
	OLS	FE	OLS	FE	OLS	FE
PRAF (Baseline: Aug.–Dec. 2000)						
$t = 1$ (May–Aug. 2002)	−0.028	−0.030	−0.030	−0.040	−0.036	0.010
	(0.039)	(0.039)	(0.041)	(0.043)	(0.057)	(0.047)
Observations	8,158	7,931	6,451	6,257	1,707	1,674
Groups		5,034		3,746		1,289
RPS (Baseline: Aug.–Sep. 2000)						
$t = 1$ (Oct. 2001)	−0.002	0.017	0.001	0.014	−0.004	0.041
	(0.024)	(0.020)	(0.023)	(0.020)	(0.078)	(0.078)
$t = 2$ (Oct. 2002)	−0.013	0.016	−0.002	0.010	−0.037	0.083
	(0.022)	(0.019)	(0.021)	(0.019)	(0.068)	(0.063)
Observations	6,438	6,464	5,484	5,505	954	959
Groups		2,903		2,239		664

Source: Own calculations based on program evaluation surveys
Standard errors, clustered at the locality level, are shown in parentheses
*$p = 0.10$, **$p = 0.05$, ***$p = 0.01$ (significant)

number of hours worked for individuals with strictly positive reported hours. The estimates for PRAF are consistently positive and small (from about 0.5 h to about 1.9 h per week), while those for RPS are consistently negative (from about −1.5 h to about −5.7 h) and are higher in terms of absolute values for women (−3 h to −5.7 h, depending on the follow-up and estimation method). However, none of the estimates for PRAF and RPS in Table 6 are significantly different from zero.

The estimates for PROGRESA (Table 7) are available for the first and third follow-up (information on hours worked was collected only for these surveys). The effects for all adults are substantially smaller than they are for PRAF and RPS, and they are not statistically significant for the full sample or for eligible and ineligible males.[15] However, there is a small but consistently significant positive average treatment effect of about 0.4 additional hours worked per week by female beneficiaries for the two available follow-up periods (ATE with OLS and FE estimates) and a smaller but still significant ITT estimate of 0.18–0.36 h in the third follow-up. These results are for working individuals and indicate a small adjustment in the intensive margin of labor supply for women, which is compatible with the idea that beneficiaries have more time available than before because of the increase in children's school enrollment documented for PROGRESA.

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[15] Angelucci and De Giorgi (2009) similarly fail to find significant effects on hours worked for non-eligible individuals in PROGRESA.

Table 5 Program effect on agricultural employment: PROGRESA

| | DD estimates | | | | | | | | | | | | | | | | | |
| | ITT | | ATE | | ITE | | ITT males | | ITT females | | ATE males | | ATE females | | ITE males | | ITE females | |
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
PROGRESA (Baseline: Sep. 1997–Mar. 1998)																		
$t = 1$	−0.010	−0.007	−0.015	−0.010	−0.005	0.000	−0.010	−0.006	−0.033	−0.030	−0.014	−0.009	−0.061	−0.057	−0.008	−0.001	−0.009	−0.022
(Nov. 1998)	(0.012)	(0.010)	(0.011)	(0.011)	(0.016)	(0.016)	(0.012)	(0.012)	(0.035)	(0.032)	(0.013)	(0.013)	(0.047)	(0.055)	(0.018)	(0.020)	(0.038)	(0.044)
$t = 2$	−0.007	−0.001	−0.009	−0.004	0.011	0.029	−0.004	0.002	−0.031	−0.025	−0.007	−0.008	−0.056	−0.053	0.019	0.061	0.002	−0.015
(Mar. 1999)	(0.011)	(0.010)	(0.012)	(0.011)	(0.020)	(0.020)	(0.012)	(0.012)	(0.034)	(0.033)	(0.013)	(0.014)	(0.046)	(0.054)	(0.025)	(0.025)**	(0.040)	(0.050)
$t = 3$	0.004	0.003	0.007	0.001	0.004	0.019	0.016	0.010	−0.031	−0.027	0.015	0.004	−0.046	−0.049	0.020	0.045	−0.019	−0.020
(Nov. 1999)	(0.012)	(0.011)	(0.012)	(0.012)	(0.020)	(0.021)	(0.013)	(0.014)	(0.035)	(0.033)	(0.013)	(0.015)	(0.047)	(0.054)	(0.023)	(0.026)*	(0.040)	(0.050)
Observations	84,210	86,176	52,978	53,775	30,208	31,364	51,065	51,210	33,145	33,269	31,576	31,641	21,402	21,463	18,917	18,990	11,291	11,348
Groups		38,583		25,200		17,534		30,336		24,646		19,280		16,051		13,276		9,309

Source: Own calculations based on program evaluation surveys

Standard errors, clustered at the locality level, are shown in parentheses

*$p = 0.10$, **$p = 0.05$, ***$p = 0.01$ (significant)

Table 6 Program effect on individual hours of work per week: PRAF and RPS

	DD estimates					
	ITT		ITT males		ITT females	
	OLS	FE	OLS	FE	OLS	FE
PRAF (Baseline: Aug–Dec. 2000)						
$t = 1$ (May–Aug. 2002)	0.681	0.814	0.493	0.580	1.840	1.849
	(0.644)	(0.650)	(0.621)	(0.617)	(1.335)	(1.716)
Observations	8,139	7,913	6,438	6,245	1,701	1,668
Groups		5,029		3,745		1,285
RPS (Baseline: Aug.–Sep. 2000)						
$t = 1$ (Oct. 2001)	−2.638	−2.982	−2.261	−2.667	−3.030	−4.067
	(1.846)	(1.807)	(1.620)	(1.649)	(4.473)	(4.734)
$t = 2$ (Oct. 2002)	−1.996	−1.971	−1.475	−1.672	−5.668	−4.001
	(1.890)	(1.882)	(1.799)	(1.798)	(4.116)	(4.584)
Observations	6,634	6,660	5,503	5,524	1,131	1,136
Groups		3,021		2,245		776

Source: Own calculations based on program evaluation surveys
Standard errors, clustered at the locality level, are shown in parentheses
*$p = 0.10$, **$p = 0.05$, ***$p = 0.01$ (significant)

While the former effect refers to the impact of the programs on individual hours of work, Tables 8 and 9 present the effects on total hours of work by adults in the household, per adult (these are household, not individual, estimates). The results for PRAF in Table 8 are similar to those shown in Table 6, with small and positive coefficients (in female-headed households, the coefficients are larger for OLS estimations), but the overall effects on number of hours worked are not significant. The results for RPS are also similar to those given in Table 6: there are larger negative effects in terms of the number of hours worked per adult, which was higher by the time of the first follow-up survey (a year after the baseline survey), but these estimates are not significantly different from zero. The results for RPS, however, differ from those reported by Maluccio (2007), who finds a significant fall in the hours worked by adults. The difference between Maluccio's (2007) estimates and those presented here is that the dependent variable in the regressions reported in Table 8 is the total number of hours worked by adults in the household *per adult*, while Maluccio (2007) uses total overall hours for the household. Replicating Maluccio's (2007) estimates indicates that there is indeed a negative and significant effect on total hours at the household level, but this is driven by a household composition effect: the number of adults in households fell significantly in female-headed households in RPS.[16] The results for PROGRESA

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[16]These additional results for RPS are presented in Table A4 in the Electronic Supplementary Material.

Table 7 Program effect on individual hours of work per week: PROGRESA

DD estimates

	ITT		ATE		ITE		ITT males		ITT females		ATE males		ATE females		ITE males		ITE females	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
PROGRESA (Baseline: Sep. 1997–Mar. 1998)																		
t = 1	0.019	−0.005	0.051	0.005	−0.022	−0.022	0.000	0.053	0.136	0.256	−0.014	0.026	0.417	0.484	0.024	0.078	−0.079	0.152
(Nov. 1998)	(0.051)	(0.047)	(0.058)	(0.056)	(0.065)	(0.058)	(0.053)	(0.058)	(0.109)	(0.167)	(0.067)	(0.069)	(0.157)***	(0.276)*	(0.068)	(0.076)	(0.134)	(0.236)
t = 3	0.037	0.055	0.055	0.026	−0.012	0.036	−0.015	0.037	0.184	0.353	−0.005	0.004	0.409	0.474	−0.025	−0.037	0.001	0.384
(Nov. 1999)	(0.053)	(0.051)	(0.062)	(0.063)	(0.075)	(0.079)	(0.056)	(0.057)	(0.110)*	(0.172)**	(0.067)	(0.069)	(0.157)***	(0.273)*	(0.084)	(0.106)	(0.143)	(0.246)
Observations	78,236	80,078	44,077	44,816	33,268	34,361	50,503	50,609	27,733	27,831	27,923	27,963	16,154	16,201	22,128	22,188	11,140	11,187
Groups		41,813		24,951		20,662		33,167		23,652		18,832		13,957		16,180		10,026

Source: Own calculations based on program evaluation surveys

Standard errors, clustered at the locality level, are shown in parentheses

* $p = 0.10$, ** $p = 0.05$, *** $p = 0.01$ (significant)

Table 8 Program effect on hours worked by adults in the household, per adult: PRAF and RPS

	DD estimates					
	ITT		ITT males		ITT females	
	OLS	FE	OLS	FE	OLS	FE
PRAF (Baseline: Aug–Dec. 2000)						
$t = 1$ (May–Aug. 2002)	0.451	0.304	0.466	0.331	0.871	0.122
	(0.572)	(0.609)	(0.604)	(0.648)	(1.166)	(1.326)
Observations	5,344	5,344	4,537	4,537	807	807
Groups		2,999		2,540		525
RPS (Baseline: Aug.–Sep. 2000)						
$t = 1$ (Oct. 2001)	−1.872	−1.938	−1.835	−1.903	−1.690	−2.103
	(1.157)	(1.157)	(1.128)	(1.129)*	(3.336)	(3.313)
$t = 2$ (Oct. 2002)	−1.602	−1.559	−1.559	−1.460	−1.869	−2.541
	(1.096)	(1.099)	(1.092)	(1.080)	(2.647)	(2.894)
Observations	4,124	4,124	3,652	3,652	472	472
Groups		1,525		1,331		194

Source: Own calculations based on program evaluation surveys

Standard errors, clustered at the locality level, are shown in parentheses

$*p = 0.10$, $**p = 0.05$, $***p = 0.01$ (significant)

in Table 9 indicate small and not statistically significant results for this household aggregate, even in female-headed households.

The overall results indicate that the programs did not introduce substantial disincentives to work, with no significant effects on the intensive or the extensive margin of labor supply for individuals and households in treatment localities. The small but significant increase in hours of work for female beneficiaries in PROGRESA is compatible with the presence of other factors that counterbalance the income effects, as discussed in Section 2.

However, the empirical results indicate that PROGRESA led to a substantial reduction in employment levels for ineligible women and a shift among ineligible men toward work in agricultural activities. These results are compatible with Angelucci and De Giorgi's (2009) evidence on PROGRESA's effects on ineligible individuals. The following section deals with the effect of PROGRESA on wages and labor income and provides a fuller picture of the program's effect on labor-market outcomes for adults.

4.3 The effect of PROGRESA on wages and household labor income

The discussion in Section 2 highlighted the possibility that cash transfer programs such as PROGRESA can have equilibrium effects by, for instance, shifting the aggre-

Table 9 Program effect on hours worked by adults in the household, per adult: PROGRESA

DD estimates

	ITT		ATE		ITE		ITT males		ITT females		ATE males		ATE females		ITE males		ITE females	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
PROGRESA (Baseline: Sep. 1997–Mar. 1998)																		
$t=1$	−0.094	0.098	−0.262	−0.113	−0.011	0.078	−0.199	0.159	0.117	0.974	−0.495	−0.334	0.007	−1.441	0.054	0.481	0.277	1.890
(Nov. 1998)	(0.450)	(0.446)	(0.547)	(0.547)	(0.556)	(0.539)	(0.486)	(0.501)	(0.825)	(1.241)	(0.581)	(0.604)	(1.237)	(1.831)	(0.639)	(0.657)	(1.095)	(1.781)
$t=3$	−0.384	−0.149	−0.374	−0.436	−0.886	−0.497	−0.357	−0.143	−0.288	0.723	−0.294	−0.486	−0.371	−2.222	−0.873	−0.215	−0.415	1.938
(Nov. 1999)	(0.483)	(0.492)	(0.575)	(0.621)	(0.650)	(0.689)	(0.504)	(0.522)	(0.858)	(1.220)	(0.586)	(0.638)	(1.256)	(1.918)	(0.758)	(0.902)	(1.244)	(1.796)
Observations	55,973	57,180	34,255	34,775	20,893	21,580	37,483	37,483	18,490	18,490	22,348	22,348	11,907	11,907	14,719	14,719	6,174	6,174
Groups		25,148		17,024		11,147		22,871		14,661		14,242		9,745		10,093		5,241

Source: Own calculations based on program evaluation surveys

Standard errors, clustered at the locality level, are shown in parentheses

$*p = 0.10$, $**p = 0.05$, $***p = 0.01$ (significant)

Table 10 Program effect on individual log hourly wages: PROGRESA

	DD estimates																	
	ITT		ATE		ITE		ITT males		ITT females		ATE males		ATE females		ITE males		ITE females	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
PROGRESA (Baseline: Sep. 1997–Mar. 1998)																		
$t = 1$	0.011	0.015	−0.005	0.007	0.030	0.024	0.012	0.015	−0.022	−0.094	0.003	0.011	−0.100	−0.097	0.027	0.034	0.042	−0.049
(Nov. 1998)	(0.032)	(0.030)	(0.040)	(0.034)	(0.037)	(0.039)	(0.031)	(0.033)	(0.090)	(0.074)	(0.037)	(0.041)	(0.164)	(0.130)	(0.039)	(0.042)	(0.075)	(0.101)
$t = 3$	0.051	0.057	0.050	0.069	0.052	0.036	0.054	0.075	0.011	−0.089	0.056	0.098	−0.045	−0.069	0.061	0.045	0.033	−0.068
(Nov. 1999)	(0.034)	(0.030)*	(0.040)	(0.035)**	(0.040)	(0.040)	(0.031)*	(0.035)**	(0.092)	(0.074)	(0.035)	(0.042)**	(0.164)	(0.137)	(0.042)	(0.052)	(0.077)	(0.100)
Observations	71,536	73,316	41,259	41,977	29,400	30,453	46,150	46,252	25,386	25,478	26,131	26,171	15,128	15,174	19,575	19,631	9,825	9,868
Groups		38,917		23,740		18,704		30,796		21,587		17,900		13,046		14,576		8,830

Source: Own calculations based on program evaluation surveys

Standard errors, clustered at the locality level, are shown in parentheses

*$p = 0.10$, **$p = 0.05$, ***$p = 0.01$ (significant)

Table 11 Program effect on log household labor income, per adult: PROGRESA

	ITT		ATE		ITE		ITT males		ITT females		ATE males		ATE females		ITE males		ITE females	
DD estimates	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE	OLS	FE
PROGRESA (Baseline: Sep. 1997–Mar. 1998)																		
t = 1	0.003	0.016	−0.013	0.004	0.024	0.036	−0.007	0.002	0.038	0.093	−0.019	−0.002	−0.027	0.054	0.010	0.005	0.114	0.075
(Nov. 1998)	(0.021)	(0.020)	(0.021)	(0.021)	(0.031)	(0.030)	(0.023)	(0.024)	(0.051)	(0.065)	(0.023)	(0.025)	(0.063)	(0.090)	(0.035)	(0.039)	(0.075)	(0.099)
t = 2	0.020	0.027	0.012	0.023	0.036	0.036	0.024	0.021	0.037	0.093	0.022	0.031	−0.020	0.064	0.030	−0.004	0.108	0.053
(Mar. 1999)	(0.023)	(0.022)	(0.023)	(0.023)	(0.038)	(0.039)	(0.024)	(0.025)	(0.052)	(0.066)	(0.026)	(0.028)	(0.065)	(0.092)	(0.044)	(0.048)	(0.079)	(0.109)
t = 3	0.027	0.039	0.031	0.046	−0.001	−0.006	0.018	0.031	0.061	0.097	0.026	0.052	0.014	0.083	−0.023	−0.045	0.092	0.008
(Nov. 1999)	(0.024)	(0.023)*	(0.024)	(0.024)*	(0.037)	(0.038)	(0.024)	(0.025)	(0.051)	(0.064)	(0.025)	(0.027)*	(0.065)	(0.091)	(0.043)	(0.049)	(0.076)	(0.105)
Observations	65,500	66,654	43,181	43,701	21,431	22,065	41,623	41,623	23,877	23,877	26,569	26,569	16,612	16,612	14,563	14,563	6,868	6,868
Groups		24,781		17,451		10,701		22,350		16,490		14,811		11,706		9,513		5,366

Source: Own calculations based on program evaluation surveys

Standard errors, clustered at the locality level, are shown in parentheses

*$p = 0.10$, **$p = 0.05$, ***$p = 0.01$ (significant)

gate labor supply curve by withdrawing children from the labor market, by freeing up adults' time, or by changing the latter's willingness to work through an income effect. It may also change relative remuneration levels, for instance by changing the sector allocation balance between beneficiaries and non-beneficiaries, as observed for PROGRESA in Table 5. The PROGRESA dataset provides a basis for an analysis of the program's effects on individual hourly wages and on household labor income per adult (this information was not collected by the PRAF and RPS evaluation surveys).

The results of the regressions presented in Table 10 indicate that PROGRESA had a sizeable effect on hourly wages in the treatment localities, although this effect seems to be driven mainly by eligible males (none of the coefficients for females are statistically significant, and none of the indirect treatment effect estimates are either). The ITT estimates indicate an increase in hourly wages of about 5.7 % by the time of the third follow-up survey, with a higher average treatment effect coefficient of about 6.9 % (both coefficients are significant only for the FE estimates). When restricting the sample to males, the ITT and ATE FE estimates indicate an effect of about 7.5 and 9.8 %, respectively.

Finally, these higher hourly wages are partially reflected in higher levels of household labor income per adult. This effect is reported in Table 11, which indicates an increase of about 3.9–4.6 % (for FE estimations, ITT, and ATE, respectively), which is concentrated in the third round of the follow-up (2 years after the baseline survey) and among male-headed households. These effects, however, are statistically significant only at the 10 % level and then only for fixed-effects ITT and ATE estimates. This evidence on labor income for adults is not incompatible with Angelucci and De Giorgi's (2009) results for monthly adult equivalent labor earnings in PROGRESA.[17] These effects on individual wages and household earnings are discussed in the context of the overall results for PROGRESA in the following section.

5 Discussion and conclusions

This study of the effect of welfare programs on work incentives and the adult labor supply in developing countries is based on estimates derived from the experimental evaluations of three programs implemented in rural areas: PROGRESA in Mexico, RPS in Nicaragua, and PRAF in Honduras.

The empirical results indicate that none of the three CCT programs has had any major impact on labor-market outcomes for adults, with no discernible effects on any of the outcomes considered for PRAF and RPS being detected, but with more

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[17] Angelucci and De Giorgi (2009, see Table 5) report that PROGRESA's average and indirect treatment effects on monthly adult equivalent labor earnings were not significant, based on results obtained by unconditional difference in differences estimation. However, they state in the notes to this table that they found a positive and significant (at the 10 % level) average treatment effect for the third-round estimate when they included conditioning variables in their regressions. This finding is compatible with the result reported in Table 11 in this study, which includes individual controls (for OLS regressions) and individual fixed effects (for FE regressions).

complex and nuanced patterns of response emerging in the case of PROGRESA. The overall results indicate that the programs have not introduced any substantial disincentives to work and that they have had no significant effect on the intensive or the extensive margin of labor supply for individuals or households in treatment localities. The finding that substantial monetary transfers have not had an impact on employment is compatible with a setting in which income effects (assuming that leisure is a normal good for beneficiaries) are either small or are counterbalanced by some of the other factors discussed in Section 2. This seems to be the case, for instance, with regard to the small (about 0.4 h per week) but significant increase in the number of hours of work for female beneficiaries in PROGRESA. This increase in the intensive margin of the labor supply for working women is compatible with the existence of some of the channels that may link CCTs with an increasing labor supply. More specifically, this effect can be associated with the program's positive effect on the school enrollment rate for children in eligible households, which in turn may have increased the amount of time that women have available to devote to paid employment. Indeed, previous evidence for the program found that it did, in fact, reduce women's participation in domestic work (Parker and Skoufias 2000).

The results for PROGRESA, however, point to the presence of more complex effects on labor-market outcomes. Angelucci and De Giorgi (2009) document the various types of positive welfare effects that PROGRESA has had on ineligible households in program localities; for instance, they report higher consumption, which seems to be mediated by credit markets, gifts and the overall effect of the program on the local economy. This study confirms the presence of these spillover and equilibrium effects and provides additional evidence in the form of the impact in terms of the labor-market outcomes for adults. The indirect treatment effects signal a reduction in employment, which is mostly driven by ineligible females. This contrasts with the increase in the number of hours worked by eligible females, although the latter effect is too small to suggest that eligible women fully displaced their ineligible counterparts from their jobs. The lack of a substantial effect with regard to the number of hours worked or household labor income per adult for the ineligible population indicates the presence of some reallocation of labor within ineligible households. Moreover, there was a substantially greater shift toward agricultural employment on the part of ineligible males than there was among their eligible counterparts. On the other hand, the program substantially increased the wages and labor income of males in eligible households. This evidence, taken as a whole, suggests that PROGRESA allowed eligible males to move away from agricultural work and toward higher-paying employment, with neutral sector reallocation at the locality level (as indicated by the lack of significant aggregate effects on agricultural employment). The evidence concerning the relative shift of ineligibles toward agricultural labor is consistent with the finding of Skoufias et al. (2008) that the PAL program in Mexico induced workers to move away from agricultural work. It also further supports the idea that employment in agriculture acts as food insurance. Eligible individuals in PROGRESA seemed to be able to take advantage of more risky but potentially more rewarding nonagricultural employment opportunities or at least to do so more than their ineligible counterparts could.

Taken as a whole, the evidence indicates that, while CCT programs in poor rural areas with high benefit levels do not create a major disincentive to work, they may still have effects in terms of employment-related outcomes and may nonetheless influence the equilibrium of the labor market. The presence of equilibrium effects in this context should not be surprising: for example, in the first stage of PROGRESA's implementation, about half of the households in treatment localities received a transfer equivalent to 40 % of their income. The aggregate effect of such large, widespread transfers must have had an impact at the community level, above and beyond their effect on eligible households, and this is reflected in Angelucci and De Giorgi's (2009) finding that PROGRESA has had a positive effect on the consumption levels of ineligible households.

These results have important implications for the evaluation, design, implementation and scope of future programs. Equilibrium effects complicate the interpretation of reduced form estimates from randomized controlled experiments, which is a long-standing discussion in regard to the analysis of welfare programs (see, for instance, Browning's (1971) critique of Orcutt and Orcutt's (1968) randomized negative income tax experiments based on feedback effects of wages). In terms of the empirical results presented here, the programs' impacts can be attributed to shifts in sector allocation and access to better income-generating opportunities for males in eligible households and to the increase in the amount of time available to women associated with higher school enrollment rates for children. It is not possible, however, to rule out further effects linked to program-induced aggregate changes in labor demand. Moreover, in the case of CCTs whose intended outcomes span multiple dimensions, indirect effects on labor-market outcomes could be confounded with the direct impact of the transfers and the programs' conditionalities. The results suggest that labor-market effects, apart from work disincentives, should be taken into account in the design of welfare programs in developing countries and that the evaluations of such programs should seek to disentangle the underlying mechanisms that are at work.

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