Received: 17 October 2019 | Revised: 16 November 2020 | Accepted: 29 December 2020

DOI: 10.1002/jid.3530

RESEARCH ARTICLE

WILEY

Conditional cash transfers, female bargaining power and parental labour supply

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Abstract

Recent empirical evidence shows that conditional cash transfer (CCT) programmes do not have an aggregate effect on the adult labour supply. However, little attention has been paid to the role of other intrahousehold dynamics. This paper examines how the parental labour supply response to CCT programmes varies with the bargaining power structure of households. We analyse a randomized experimental CCT design from rural areas of Honduras (PRAF) and found that women with more bargaining power in the household are four percentage points less likely to be employed than other women.

KEYWORDS

bargaining power, conditional cash transfer, labour supply

JEL CLASSIFICATION

I38; J22; D13

1 | INTRODUCTION

Conditional cash transfer (CCT) programmes are generally designed to increase children's human capital, mainly through a monetary incentive that is given to poor families when observable conditions (e.g. school attendance, vaccinations and medical controls) are regularly fulfilled. Especially in Latin America and the Caribbean (LAC), the

We would like to thank Guillermo Cruces and María Laura Alzúa for their work in a previous version of the paper; Arjun Bedi, Mark Bryan, Adeline Delavande, John Ermisch and Patrick Nolen for their very helpful comments and suggestions; and Mariano Bosch and Norbert Schady and the participants at the 7th IZA/World Bank Conference 'Employment and Development' for their helpful suggestions. Any remaining errors are our own.

positive impacts of some of these programmes include reducing child labour, increasing school enrolment and increasing health check-ups. However, CCT designs do not generally account for secondary effects that could occur (e.g. a change in adult labour supply).

According to Fiszbein et al. (2009), unforeseen and unintended individual behavioural responses are partially responsible for the fact that, for most CCTs, the impact of a CCT on consumption is smaller than the magnitude of the transfer itself. Adults in the beneficiary household may react to the CCT supplying a different amount of labour for one of several reasons: a pure income effect that increases the demand of leisure; increases in the time allocated to accomplish the programme conditionalities (e.g. bringing children to school and health centres); or adults working fewer hours to continue being or to become eligible for the programme if the CCT is means-tested. CCTs might also lead to an increase in the adult labour supply, for instance, if households need to compensate for a reduction in income associated with a decrease in child labour or afford an increase in school expenditures; or if the reduction in home-based childcare activities frees up time. Exploring labour supply responses to CCT programmes is crucial to better understanding the income-generation processes of the poor and to assess potential poverty traps. It is also important in shaping policy discussion and understanding how to better design CCTs.

Recent reviews of the effect of conditional and unconditional cash transfer programmes on labour market outcomes in developing (Baird et al., 2018; Banerjee et al., 2017) and developed countries (Marinescu, 2018) find null or small effects on average adult labour. However, there is a lack of evidence on the role of bargaining power in determining adult labour responses to cash transfer programmes.

This paper investigates the effect of CCT programmes on the labour supply of parents in rural Honduras using data from the randomized experimental evaluation design of the *Programa de Asignación Familiar* (PRAF). However, unlike other studies, we explore whether the CCT programme has heterogeneous effects due to different bargaining power structures in the household, thus shedding light on the decision-making process. Although the size of the cash transfer is small (equivalent to only 3.6% of household consumption, on average), PRAF provides a good setting to evaluate these heterogeneous effects on adult labour supply because the intended recipients of the transfers were women.

Recent studies have attempted to solve empirically the theoretically ambiguous effect of CCTs on adult labour supply (Baird et al., 2018; Banerjee et al., 2017). Alzúa et al. (2013) focus on CCT programmes in Honduras, Mexico and Nicaragua and find no discernible effects on individuals (adult men and women) or household-aggregated adult labour supply. Another branch of research allows for gender-based differences in preferences between decision-takers in the household (non-unitary models of the household) and examines how the intrahousehold allocation of resources reacts to CCT programmes. In general, this literature finds that CCT programmes change the pattern of resource allocation in relation to the gender of the parent (Gitter & Barham, 2008; Schady & Rosero, 2007).

This paper builds on the existing evidence by empirically examining a model that allows for the heterogeneous effects of CCT programmes on parental labour supply related to the distribution of power within the household. First, we control for the pretreatment effects of bargaining power on labour supply to test whether differences in fathers' and mothers' labour supply respond to differences in the distribution of power within the household.[1] Second, to account for potential heterogeneous effects of CCT programmes with regard to household power structures, we include interactions of our bargaining power measure and the treatment indicator.

Our results show that the CCT impacts on the labour supply of parents vary with the distribution of power in the household. Particularly, the reduction in the probability of being employed is considerably higher for women with more bargaining power than for women with lower intrahousehold bargaining power. It is plausible to argue that the reduction of employment allow mothers to substitute for children's time in non-remunerated activities, as in the case of Mexico's *Oportunidades* (Rubio-Codina, 2010), which might have contributed to the positive effects of PRAF on children school enrolment and the reduction on child labour (Galiani & McEwan, 2013).

This paper contributes to the scarce literature evaluating the heterogeneous impacts of CCT programmes in relation to the distribution of power within the household. Despite the increasing amount of research on the impact of CCTs on parental labour supply, no studies look at whether having more power allows partners to change their labour supply differently. In doing this, we bring structural concepts taken from 'non-unitary' models of the household to the estimations of effects in randomized evaluation designs. This paper also contributes to expanding the literature on the effects of social programmes on labour supply in developing countries (Baird et al., 2018), which has been mainly focused on developed countries (Blundell & Hoynes, 2004; Marinescu, 2018; Meghir & Phillips, 2008; Michalopoulos et al., 2005; Moffitt, 2002; 2003).

The rest of this paper is structured in the following way. Section 2 discusses the theoretical impacts of CCT programmes on adults' labour supply and the findings in the empirical literature about the impacts of CCT programmes on labour supply. Section 3 briefly describes the most relevant features of PRAF. Section 4 describes the empirical approach. Section 5 shows the results, and Section 6 concludes.

2 | CCTS AND LABOUR SUPPLY

Economic theory suggests several channels through which CCT programmes might affect the work decisions of beneficiary households. In addition to those suggested by the 'unitary' model of the household (Becker, 1991), the 'collective' model of the household (Chiappori, 1988; 1992) suggests an additional effect of the CCT on adults' labour supply related to the distribution of power in the household. From a theoretical point of view, the combined effect of all the potential channels is ambiguous and therefore should be resolved empirically.

First, the CCT represents for beneficiary households a direct, exogenous increase in non-labour income and is, therefore, a pure income effect that relaxes the budget constraint. If leisure is a normal good, this effect is expected to reduce labour supply. The second channel is related to the potential decrease in household income due to the reduction in child labour. If this is the case, we might expect a positive response of adult labour supply to compensate for the reduction in total household income, which might mitigate the negative impact of the transfer on the adult labour supply. Third, fulfilling CCT conditionalities might require behavioural responses in the adult labour supply. For instance, a CCT with conditionalities on school attendance might reduce the time an idle child spends at home, reducing the parental time dedicated to childcare, which may now be spent in the labour market. CCTs might also cause parents to increase the amount of time dedicated to childcare (e.g. bringing children to school and health centres). This applies to any direct and indirect time cost associated with fulfilling the programme's conditionalities. Additionally, Fiszbein et al. (2009) distinguish a 'price effect' channel, through which households (beneficiaries or not) might modify their supply of labour to become eligible for the programme or continue receiving it.[2]

Moving beyond the standard utility model, the 'collective' model of the household posits a utility function for each agent in the household (Chiappori, 1988; 1992). In opposition to unitary models of the household, 'collective' models suggest that the household allocation of resources results from the interaction of the utility function of different agents in the household, which are aggregated into a household's utility function through the weighted average of the different agents' utilities. Weights consist of distributional factors reflecting individuals' distribution of power in the household decision. Among the main factors determining power distribution are unearned income (Hoddinott & Haddad, 1995; Thomas, 1990; 1994), relative education (Beegle et al., 2001; Frankenberg & Thomas, 2003; Thomas, 1994) and assets brought to marriage (Fafchamps & Quisumbing, 2002).

In contrast to the 'unitary' model, in 'non-unitary' models, the identity of the transfer recipient matters. Duflo (2003) exploits a natural experiment in South Africa to find that pensions women receive improve children's (particularly girls') anthropometric status, whereas those men received had no such effect. Similarly, for the United Kingdom, Lundberg et al. (1997) find that a policy change that redistributed the child allowance from husbands to wives resulted in a substantial increase in expenditures on children's clothing. Similarly, unearned income under the control of women (Thomas, 1990) and maternal education (Emerson & Souza, 2007; Thomas, 1994) have been found to have a greater effect on children's health and educational outcomes, relative to the equivalent for fathers.

Based on similar empirical evidence and similarly to most programmes in the region, the design of PRAF targeted women as the recipients of the transfer to maximize the programme's effects on child development (Bouillon &

Tejerina, 2007). This special feature in CCT's design might also modify the allocation of a household's resources depending on the initial intrahousehold power distribution. A measure of bargaining power, which has been extensively used in the literature and is used in this study, corresponds to the ratio of the number of years of school the mother completed and the number of years of school the father completed (Basu & Ray, 2002; Beegle et al., 2001; Gitter & Barham, 2008; Schady & Rosero, 2007; Thomas, 1994). The main assumption behind this indicator is that a larger ratio of female to male education reflects that women have more bargaining power. Intuitively, having relatively more education suggests that women have better options outside of marriage, such as opportunities in the labour market, and, thus, better potential gains if an agreement with the partner is not reached (called *threat points* in bargaining models). Theoretically, in a cooperative household model setting, parental relative education may represent a *distribution factor* affecting the Pareto weights associated with parents' utilities. These weights summarize the intrahousehold decision process and finally determine the location of the household resource allocation on the Pareto-efficient frontier. Browning et al. (2011) argue that the Pareto weights have a natural interpretation in terms of decision powers. For instance, an increase in a wife's Pareto weight results in a move alongside the Pareto set in the direction of higher utility for her (and lower utility for her husband). Then, in a purely economic sense, a larger weight would correspond to more power and better outcomes for the wife.

The expected effect of maternal bargaining power on parental labour supply is also ambiguous and depends, among other factors, on women's preferences. On the one hand, an increase in women's bargaining power might lead them to decrease their supply of labour and leads to either an increase in their consumption of leisure or time dedicated to household chores, including accomplishing the conditionalities of the CCT programme (e.g. bringing children to school or health centre). On the other hand, having more power might allow women to overcome traditional gender roles and supply more labour.

3 | THE CCT PROGRAMME: *HONDURAS'* PRAF

The PRAF was initially implemented by the Government of Honduras in the early 1990s as a compensatory mechanism to mitigate the impact of macroeconomic adjustments on the poor and to alleviate structural poverty.[3] The objective of the programme is to encourage poor households to invest in human capital (primarily education and health) through CCTs.

The programme was relaunched in 2000 as PRAF II (hereafter PRAF) with the same objective of increasing children's health and education in poor rural communities in Honduras. The second phase of PRAF is geographically targeted at 70 municipalities in the poorest region of the country. According to Bouillon and Tejerina (2007), with a yearly budget of US$17 million (0.2% of the country's GDP), the second phase of PRAF benefits 47 800 rural households.

The experimental evaluation of PRAF consisted originally of three different types of interventions (a demand-side intervention only, a supply-side intervention only and a combination of both), assigned to three different subgroups of treatment households. Fifty randomly selected municipalities formed the treatment group (with 20, 20 and 10 municipalities assigned to each of the above interventions, respectively), of a total of 70, with the 20 additional municipalities forming the control group. However, the supply-side intervention was never implemented, and the mixed intervention was implemented in just a few communities (Glewwe & Olinto, 2004). Therefore, the empirical results presented in this paper are based on the 20 municipalities in the control group and the 20 municipalities in the first treatment subgroup.

The demand-side intervention takes the form of a cash transfer conditioned on school attendance and health clinic visits by the recipient. The educational component consists of a monetary payment to households with children at school in Grades 1–4, of approximately US$5 per month per child, up to three children per family. The health and nutritional components provide US$4 per month per person to children less than 3 years old (up to two children per family) and pregnant mothers, conditional on attending medical visits at health centres.

This paper uses the baseline survey carried out in the last quarter of 2000 and a follow-up survey in 2002. In the 2002 follow-up survey, we observe about 92% of the 3181 households interviewed in the 2000 survey.[4]

4 | EMPIRICAL STRATEGY

This section presents the empirical strategy for estimating the effect of the CCT programme on the adult labour supply while accounting for differences in the power share in the household. The experimental design provides a strong identification strategy that allows us to attribute the differences in labour supply between treatment and control groups to the impact of the CCT programme.

To ensure that the beneficiary's parents have direct access to the CCT, which, in turn, can make them change their labour supply, we restrict the original sample to households where one of the parents of the CCT beneficiary is the head of household. We also exclude from the sample single-parent households and households where the beneficiary's parents are not observed living together during the entire period of analysis. The final sample consists of 1622 households.

In Equation 1, L^s_{ict} represents the supply of labour of adult i (father or mother), in community c at time t; A is a dummy indicating the post-treatment year 2002; P_c is a dummy indicator for whether the adults live in a community of intervention; $Treat_{ct}$ is the interaction term between the treatment year and the treatment status of the household (P_c). The vector **H** includes characteristics of the household at baseline, such as the number of children aged 0–2 and aged 3–5; boys and girls aged 6–7, 8–12 and 13–18; men and women aged 19–54; and men and women over the age of 55. The set of covariates E includes individual characteristics of both father and mother in the household, such as the years of schooling, age and age squared. Finally, v_{ict} represents an unobserved idiosyncratic and time-variant error term.[5]

$$L^s_{ict} = \alpha_0 + \alpha_1 A + \gamma_1 P_c + \delta Treat_{ct} + H'\beta + E'\theta + v_{ict}$$

The outcomes of interest correspond to an indicator for whether the individual is employed and the weekly working hours in all occupations. Given the data restrictions, the first indicator corresponds to a simple definition of participation (i.e. it is not possible to distinguish between unemployed and inactive workers). The second outcome, the number of working hours, is defined only for those individuals with a strictly positive number of hours.

In Equation 1, δ corresponds to the difference-in-difference (DID) estimator of the programme impact. Providing that P_c is randomized, $E(v_{ict}|\ Treat) = 0$, and therefore, the parameter δ is consistently estimated. The estimates of the parameter δ shown below correspond to comparisons of households according to whether they were offered treatment (i.e. whether they were randomly assigned to a treatment group), known as intention-to-treat (ITT) effects. Given the random assignment to the treatment group, the ITT effect can be interpreted as the causal effect of the offer of CCT on parental labour supply. However, it is important to recall that some of those offered CCT might have declined to participate.[6]

We use the DID model to control for possible pre-existing differences between the treatment and the control group, because the randomization process may not be perfect.[7] The DID method assumes that, in the absence of treatment, changes in labour supply (employment and weekly hours) in the control group would adequately represent changes in those variables for the treatment group (parallel trends assumption). Adding observed covariates to Equation 1 contributes to the plausibility of this assumption and improves the precision of the programme's estimated impact.

As a measure of bargaining power, this paper considers the ratio of the school years completed by the mother and the father at baseline. Similar measures have been used in other studies in developing countries (Basu & Ray, 2002; Beegle et al., 2001; Gitter & Barham, 2008; Schady & Rosero, 2007; Thomas, 1990; 1994). Following

Gitter and Barham (2008), a constant equal to 1 is added to both the numerator and the denominator to avoid an undefined ratio in those households where fathers have 0 years of schooling.[8]

$$BP = \frac{(mother's\ years\ of\ schooling + 1)}{(father's\ years\ of\ schooling + 1)}$$

Thus, including this expression in Equation 1, we obtain

$$L^s_{ict} = \alpha_0 + \alpha_1 A + \gamma_0 BP + \gamma_1 P_c + \delta Treat$$

$$+ \varphi(Treat * BP) + H'\beta + E'\theta + v_{ict}$$

In addition to including the years of schooling of both parents in the bargaining power indicator, *BP*, they are included separately to account for different opportunities in labour markets. The impact of pre-existing conditions regarding the distribution of power in the household is captured by the coefficient on the indicator of relative education (γ_0). The interaction term allows us to identify whether the CCT programme has any differential impact on labour supply according to the maternal bargaining power.[9]

Equations 1 and 3 are separately estimated for mothers and fathers using ordinary least squares (OLS), with standard errors clustered at the community level.

5 | EMPIRICAL RESULTS

5.1 | Bargaining power and parental labour supply: Descriptive evidence

This section describes the distribution of our indicator of bargaining power (Table 1) and discusses the relation between power and the selected labour outcomes (working hours and employment). It additionally presents the pre-treatment association between maternal bargaining power and adults' labour supply for treatment and control groups (Table 2).

Table 1 shows the proportion of households where mothers have more education ($BP > 1$), less education ($BP < 1$) and equal education ($BP = 1$) than their partners, among both control and treatment groups. In our sample, 38% of women have less education than men.

TABLE 1 Distribution of maternal bargaining power by treatment group

Maternal bargaining power (*BP*)[a]	Control mean	Treatment mean	*diff*[b]	All
BP < 1	0.369	0.396		0.383
	(0.483)	(0.489)		(0.486)
BP = 1	0.319	0.267	**	0.292
	(0.466)	(0.443)		(0.455)
BP > 1	0.312	0.337		0.325
	(0.464)	(0.473)		(0.469)

[a]Dummy variables created according to the value of *BP* at baseline. BP = [(years of schooling completed by mother + 1)/(years of schooling completed by father + 1)]. Standard deviation in parentheses.
[b]*t*-test on the equality of means between control and treatment households.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

TABLE 2 Outcomes' baseline means

Maternal bargaining power (BP)	(1) % employed	(2) Working hours per week	(1) % employed	(2) Working hours per week
	Females		Males	
BP < 1	0.214 (0.410)	28.307 (14.993)	0.915 (0.278)	39.884 (9.694)
BP = 1	0.229 (0.421)	30.775 (12.001)	0.925 (0.264)	39.599 (8.120)
BP > 1	0.264 (0.441)	32.452 (14.464)	0.901 (0.300)	39.993 (7.195)
Total	0.235 (0.424)	30.544 (14.123)	0.913 (0.281)	39.835 (8.491)
Number of HH	1172	271	1605	1461

Note: Mean value at baseline for all (control and treatment) households. Standard deviation in parentheses. The average working hours variable is calculated for individuals who are employed.

The average maternal bargaining power, measured as in Equation 2, is 1.43, with differences between treatment and control groups that are not statistically significant.[10] Figure A1, in the Appendix, shows the distribution of our indicator of maternal bargaining power. Nearly one-third of women with more education than their husbands show levels of schooling that are considerably higher than that of their husbands. About two-thirds of women are less or equally ‘powerful’ as their husbands, but in those households where women have more education than their husbands, maternal power is relatively higher.[11]

As discussed in Section 2, the theoretical relationship between the bargaining power structure and parental labour supply is ambiguous and needs to be verified empirically. The pretreatment association between maternal bargaining power and adults' labour supply is explored in Table 2. Table 2 shows the employment rate (Column 1) and the average weekly worked hours (Column 2) of mothers and fathers at the baseline, according to the distribution of power in the household. As bargaining power in the household becomes more balanced or shifts towards women, the labour supply of mothers seems to increase in the extensive margin (employment), which might suggest that more powerful mothers are able to overcome traditional gender roles and supply more labour. It can also be observed an increase in the intensive margin: In households with more powerful women (*BP* > 1), women work 4 h more than in households where women are less powerful (*BP* < 1).[12]

5.2 | Econometric results

This section discusses the estimation of Equations 1 and 3 and includes controls for potential differences between control and treatment groups at the baseline for two main indicators of labour supply: employment (Table 3) and weekly worked hours (Table 4).[13] We use regression DID to account for the evidence shown in the previous section about a pretreatment association between maternal bargaining power and adults' labour supply and to control for other characteristics, accounting for any systematic differences in the baseline.

We first estimate a specification that does not account for heterogeneous effects of the CCT related to the distribution of power in the household (Equation 1). These estimations are similar to those previously estimated in the literature under a ‘unitary’ setting of the household. Thus, they help to contextualize this study among previous studies and offer a benchmark for the following discussion on the differential effect of maternal bargaining power.

The first two rows in Tables 3 and 4 correspond to the coefficient α_1 in Equation 1, which represents the time trends common to the control and treatment groups. The third row corresponds to the coefficient γ_1 in Equation 1, which represents pretreatment differences between the treatment and control groups. As discussed before, despite randomization, treatment and control households were different with regard to some characteristics at baseline. In

TABLE 3 Effect of CCT on parental employment—no maternal bargaining power

Linear probability model	Males	Females
Post-treatment period = 1	0.023** (0.010)	0.022 (0.032)
If CCT treatment group = 1	0.006 (0.017)	–0.057 (0.042)
CCT treatment group*post-period	–0.006 (0.019)	0.001 (0.039)
Observations	5844	4518
Clusters	42	41
R^2	0.053	0.086

Note: Additional controls include paternal and maternal age, age-squared and years of education; the number of family members aged 0–2 and 3–5; and the number of male and female family members aged 6–7, 8–12, 13–18, 19–54 and 55 or more. Household composition and parents' education are measured at baseline. Equations in Columns (1) and (2) are estimated using ordinary least squares (OLS) with standard errors clustered at the community level (in parentheses).
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

TABLE 4 Effect of CCT on parental worked hours—no maternal bargaining power

OLS	Males	Females
Post-treatment period = 1	–2.825*** (0.481)	–4.148*** (1.472)
If CCT treatment group = 1	0.730 (0.603)	0.138 (1.794)
CCT treatment group*post-period	0.502 (0.747)	–0.574 (2.259)
Observations	5368	1114
Clusters	42	40
R^2	0.053	0.096

Note: Additional controls include paternal and maternal age, age-squared and years of education; the number of family members aged 0–2 and 3–5; and the number of male and female family members aged 6–7, 8–12, 13–18, 19–54 and 55 or more. Household composition and parents' education are measured at baseline. Equations in Columns (1) and (2) are estimated using ordinary least squares (OLS) with standard errors clustered at the community level (in parentheses).
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

particular, the difference in employment rate and worked hours shown in Table A1, in the Appendix, is reflected in these coefficients. Finally, the coefficients on the interaction terms (*Treat*) correspond to δ in Equation 1 and represent the DID estimation of the programme's impact.

Despite the differences in sample definition and econometric specifications, the estimations here are reasonably similar to those previously found in the literature. Alzúa et al. (2013) also find the impact of PRAF statistically insignificant. The other covariates included, when statistically different from 0, show the expected relationship with parental labour supply.

We now turn to the estimation of Equation 3, which accounts for potential heterogeneous effects related to the distribution of power in the household, of the CCT on employment (Table 5) and worked hours (Table 6). As before, the first two rows show time trends common to the control and treatment groups, and the fourth rows represent pretreatment differences between treatment and control groups. This specification allows us to separately identify the effect of pretreatment maternal bargaining power (coefficient on *BP*) and whether the CCT has a heterogeneous effect related to bargaining power (coefficient on *Treat**BP*) on the labour supply of both parents. Given that, by definition, our indicator of maternal bargaining power cannot be 0, BP has been centred to its mean value (1.4).

TABLE 5 Effect of CCT on parental employment—with maternal bargaining power

Linear probability model	Males	Females
Post-treatment period = 1	0.023** (0.010)	0.022 (0.032)
Maternal bargaining power (BP)	−0.008 (0.006)	0.002 (0.011)
If CCT treatment group = 1	0.006 (0.017)	−0.057 (0.042)
CCT treatment group*post-period	−0.006 (0.019)	0.001 (0.039)
Treat*BP	0.008 (0.006)	−0.042*** (0.012)
Constant	0.693*** (0.088)	0.014 (0.132)
Observations	5844	4518
Clusters	42	41
R^2	0.054	0.090

Note: Maternal bargaining power (BP) centred at its mean value on each country. Additional controls include paternal and maternal age, age-squared and years of education; the number of family members aged 0–2 and 3–5; and the number of male and female family members aged 6–7, 8–12, 13–18, 19–54 and 55 or more. Household composition and parents' education are measured at baseline. Equations in columns (1) and (2) are estimated using ordinary least squares (OLS) with standard errors clustered at the community level (in parentheses).
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

TABLE 6 Effect of CCT on parental worked hours—with maternal bargaining power

OLS	Males	Females
Post-treatment period = 1	−2.827*** (0.481)	−4.075*** (1.482)
Maternal bargaining power (BP)	0.118 (0.218)	−0.503 (0.586)
If CCT treatment group = 1	0.729 (0.603)	0.130 (1.785)
CCT treatment group*post-period	0.501 (0.746)	−0.771 (2.325)
Treat*BP	−0.050 (0.198)	−0.365 (1.170)
Constant	37.632*** (2.983)	24.286* (12.831)
Observations	5368	1114
Clusters	42	40
R^2	0.053	0.098

Note: Maternal bargaining power (BP) centred at its mean value on each country. Additional controls include paternal and maternal age, age-squared and years of education; the number of family members aged 0–2 and 3–5; and the number of male and female family members aged 6–7, 8–12, 13–18, 19–54 and 55 or more. Household composition and parents' education are measured at baseline. Equations 1 and 2 are estimated using ordinary least squares (OLS) with standard errors clustered at the community level (in parentheses).
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

Table 5 shows the effect of the CCT programme on parental employment.[14] Even though the coefficients on *Treat* in Tables 3 and 5 do not show a statistically significant treatment effect of PRAF on adults' labour supply, Table 5 shows evidence that the treatment effect, among those treated, varies with *BP*. Among PRAF-treated households, the coefficient on *Treat*BP* shows that the CCT is associated with a reduction (by 4.2 percentage points) in the probability of working for mothers as maternal bargaining power marginally increases. The magnitude of this effect is not neglectable when compared with the null effects for females found by evaluations of PRAF and other CCT programmes (Alzúa et al., 2013; Banerjee et al., 2017; Galiani & McEwan, 2013).

In terms of hours worked, Table 6 examines heterogeneous treatment effects among the treated (differences in the slope coefficient *Treat*BP*). No evidence of heterogeneous impacts of CCTs related to the distribution of power in the household on adults' working hours is found. Table A3, in the Appendix, shows differences evaluated at different levels of *BP*.[15]

6 | CONCLUSIONS

This paper examines the effect of CCT programmes on the labour supply of adults in rural areas in Honduras. We argue that in addition to the regular channels through which CCTs might affect adult labour supply, which has been explored in the previous literature using mainly the 'unitary' model of the household, the distribution of power in the household might play a role in explaining unintended behavioural responses capable of offsetting CCTs.

A special feature in the design of this programme allows for the exploration of these potential effects: With the object of maximizing effects (mainly, on child human capital accumulation), women were targeted as the recipient of the transfer. Considering the household's decision-making process in a cooperative setting, such a change is likely to produce different reactions depending on the distribution of power within the household and women's preferences. We calculate our measure of bargaining power as the ratio of the number of years of school completed by the mother and the number of years of school completed by the father to explore how it could potentially lead to different impacts of CCTs on two main indicators of labour supply: employment (extensive margin) and weekly worked hours (intensive margin).

Overall, as previous literature has found, CCTs seem to have relatively limited effects on adult labour supply. However, we do find that the CCT has a heterogeneous effect related to maternal bargaining power. Women with more bargaining power in the household are less likely (four percentage points) to be employed than other women.

Fiszbein et al. (2009) offer some explanations for the absence of larger effects of CCTs on adult labour supply. It might be that the income elasticity of leisure is very low among poor households. It is also likely that accomplishing the CCT conditionalities implies increasing costs in an amount similar, or higher, than the transfer itself. It might also be that the transfer is perceived by households as temporary, rather than permanent. Finally, as the authors suggest, it could be that labour supply effects are being studied too soon after the implementation of the programmes, and therefore, potential long-run effects are not being captured.

ACKNOWLEDGEMENTS

We would like to thank Mark Bryan and Patrick Nolen for the extensive discussion and comments and Mariano Bosch and Norbert Schady and the participants at the 7th IZA/World Bank Conference 'Employment and Development' for their helpful suggestions. We also want to thank Guillermo Cruces and María Laura Alzúa for their contribution on a previous version of the paper. Finally, we want to thank two anonymous referees for valuable feedback. The authors are responsible for all the results and views, which do not necessarily represent the view of any of the institutions where they are affiliated.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon reasonable request.

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ENDNOTES

[1] In the paper, we use the terms mother (father) and wife (husband) to emphasize that we are interested in studying intrahousehold bargaining dynamics between the parents of the child who makes the household eligible to receive the CCT. Section 4 presents the criteria used for including households in the analysis.

[2] Parker and Skoufias (2000) rule out this option in the case of PROGRESA, because the transfer is given to the households for 3 years, irrespective of the family income, which eliminates any disincentive effect on work. They also show evidence that PROGRESA made mothers modified the allocation of time to fulfil programme obligations.

[3] For complete description of the programme, see Glewwe and Olinto (2004).

[4] Overall attrition (7.9%) is close to the benchmark considered low (Gertler et al., 2016). To test for possible bias due to attrition, we estimated a model for attrition on observable baseline characteristics. The specification was based on the best subset selection among 26 predictors (including treatment status) using Bayesian Information Criterion (BIC). Results of the *F*-test show that covariates do not jointly significantly explain the variation in the dependent variable.

[5] Equation 1 does not intend to separately identify the income and non-income effects of CCT programmes on the labour supply of adults. After including household consumption in an equation similar to (1), Hoddinott and Skoufias (2004) and Gitter and Barham (2008) argue that the associated coefficient represents the non-income effects of CCT programmes. Including measures of consumption or income would not be appropriate in our case, given the very likely reverse causality problem between household income and labour supply. Therefore, in this paper, the coefficient on the variable Treat, δ, should be interpreted as the total average effect of the CCT programme on the labour supply of parents.

[6] The fact that some members of the treatment group fail to receive treatment does not invalidate the randomized design, provided that all those who were initially assigned to treatment are included in the impact analysis. Also, a high degree of compliance with assignment is expected in the case of PRAF, because conditionalities were not strongly enforced.

[7] Table A1 explores the balance of the randomization process for variables in Equation 1 across levels of maternal power in the household. Overall, samples seem to be balanced. However, there are some significant differences between treatment and control groups in female and male employment rate.

[8] In the PRAF sample, 30% and 32% of fathers and mothers, respectively, reported not having formal education in the baseline year.

[9] BP enters Equation 3, in a simple, linear way that does not have a casual interpretation. To account for non-linearities, in a different specification, we include three separate dummies for mothers with less (base category), equal and more power than their husbands (as in Table 1) and their interactions by Treat. Results are presented in Tables A2 and A3.

[10] The *t*-statistic associated to the mean comparison of BP across treatment and control households is 0.18.

[11] Among the group of households with more powerful women, the education of women is, on average, 2.94 times higher than the education of their husbands.

[12] The mean-comparison *t*-test is significant at the 5% level.

[13] A comparison of conditional outcomes such as hours worked is problematic when the programme affects the probability of being employed. In our case, however, PRAF appears to have no aggregated employment effects (see Table 3), implying that comparisons of hours worked between treatment and control groups are valid.

[14] Table A2, in the Appendix, shows that this negative effect is constant at different levels of BP. Moreover, the evidence shown in Table A2, in the Appendix, reaffirms that PRAF does not affect differently, the employment probability of parents in treatment and control groups, when evaluated at different levels of BP.

[15] In particular, we evaluate the treatment effect at three values of BP: (i) at the mean value of BP for those households where mothers have less power than their husbands ($BP < 1$); (ii) at the mean value of BP for those households where power between parents is equally distributed ($BP = 1$); and (iii) at the mean value of BP for those households where mothers have more power than their husbands ($BP > 1$). The mean values of *BP* corresponding to the three types of households, according to the bargaining power distribution, are 0.47; 1; and 2.91.

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How to cite this article: Novella R, Ripani L, Vazquez C. Conditional cash transfers, female bargaining power and parental labour supply. *J. Int. Dev*. 2021;1–15. https://doi.org/10.1002/jid.3530

APPENDIX A.

TABLE A1 Baseline means and randomization balance by maternal bargaining power

	BP < 1			*BP* = 1			*BP* > 1		
Baseline characteristics	**Control means**	**Treatment means**	***diff* (C-T)**[a]	**Control means**	**Treatment means**	***diff* (C-T)**	**Control means**	**Treatment means**	***diff* (C-T)**
Household composition									
Members 0–2	0.867	0.867	0.000	0.77	0.746	0.024	0.805	0.881	−0.076
Members 3–5	0.696	0.793	−0.097*	0.653	0.689	−0.036	0.672	0.668	0.004
Males 6–7	0.232	0.259	−0.027	0.25	0.221	0.029	0.232	0.257	−0.025
Females 6–7	0.219	0.221	−0.002	0.177	0.186	−0.009	0.203	0.213	−0.01
Males 8–12	0.491	0.489	0.002	0.463	0.532	−0.069	0.455	0.413	0.042
Females 8–12	0.509	0.456	0.053	0.517	0.482	0.035	0.432	0.499	−0.067
Males 13–18	0.499	0.418	0.081	0.493	0.418	0.075	0.46	0.387	0.073
Females 13–18	0.371	0.392	−0.021	0.39	0.35	0.04	0.432	0.431	0.001
Males 19–54	1.139	1.14	−0.001	1.097	1.118	−0.021	1.147	1.081	0.066
Females 19–54	1.045	1.078	−0.033	1.027	1.021	0.006	1.116	1.156	−0.04
Males +55	0.179	0.157	0.022	0.227	0.243	−0.016	0.223	0.21	0.013
Females +55	0.117	0.116	0.001	0.173	0.161	0.012	0.161	0.148	0.013
Individual characteristics and outcomes									
Maternal education	1.634	1.763	−0.129	2.009	2.452	−0.443*	4.464	4.437	0.027
Paternal education	4.350	4.509	−0.159	2.009	2.452	−0.443*	1.464	1.598	−0.134
Maternal employment	0.262	0.183	0.079**	0.272	0.185	0.087**	0.343	0.281	0.062
Paternal employment	0.920	0.915	0.005	0.919	0.928	−0.009	0.854	0.912	−0.058**
Maternal work hours	29.051	31.379	−2.328	30.462	33.676	−3.214	35.515	32.294	3.221
Paternal work hours	39.468	40.125	−0.657	38.983	40.224	−1.241*	39.256	40.378	−1.122*

[a]*t*-test on the equality of means.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

TABLE A2 Effect of CCT on parental employment at different levels of BP

Treat at the mean values of BP ...	Males	Females
For households where mothers have less power (*BP* < 1)	0.004 (0.024)	−0.018 (0.058)
For households where mothers have equal power (*BP* = 1)	0.023 (0.033)	0.055 (0.059)
For households where mothers have more power (*BP* > 1)	−0.054 (0.042)	−0.021 (0.059)

Note: Each row corresponds to a separate regression where maternal bargaining power (BP) has been centred at its mean value within each type of household (those where mothers have less power than their husbands; those where power is equally distributed; and those where mothers have more power than their husbands). The full model is available on request. Standard errors clustered at the community level in parentheses.
*Significant at 10%
**Significant at 5%,
***Significant at 1%.

TABLE A3 Effect of CCT on parental weekly worked hours at different levels of BP

Treat at the mean values of BP ...	Males	Females
For households where mothers have less power (*BP* < 1)	−0.559 (0.918)	−1.913 (3.944)
For households where mothers have equal power (*BP* = 1)	1.858 (1.301)	−0.462 (3.038)
For households where mothers have more power (*BP* > 1)	0.415 (1.301)	1.711 (4.417)

Note: Each row corresponds to a separate regression where maternal bargaining power (BP) has been centred at its mean value within each type of household (those where mothers have less power than their husbands; those where power is equally distributed; and those where mothers have more power than their husbands). The full model is available on request. Standard errors clustered at the community level in parentheses.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

FIGURE A1 Distribution of maternal bargaining power (BP)