Women's Power, Conditional Cash Transfers, and Schooling in Nicaragua

Seth R. Gitter and Bradford L. Barham

The Social Safety Net (Red de Protección Social, RPS) program in Nicaragua is one of many conditional cash transfer programs that pay households cash stipends in exchange for school attendance and regular visits to health clinics by the children. A key feature is that payments go to the female head of household. Previous research suggests that exogenous transfers to women are more likely to be spent on their children's health, nutrition, and education and thus to reinforce the goals of these programs. Randomized experimental data from RPS are used to test for heterogeneous program impacts on school enrollment and spending based on a woman's power, as proxied by her years of schooling relative to her husband's years of schooling. The results confirm previous findings that more household resources are devoted to children when women are more powerful. However, when a woman's power greatly exceeds her husband's, additional female power reduces school enrollment. RPS impacts on schooling are much larger than the expected income effects estimated from the control group, although no evidence is found that female power alters the impact of RPS on school enrollment. The conditionality of RPS is probably decisive. While RPS significantly increases food and education expenditures, the impact is attributable primarily to income effects. JEL codes: D13, H31, I20

Many poverty alleviation programs in developing countries stipulate that payments or benefits be given to the female head of household (Rawlings and Rubio 2005). The justification for targeting women is based on theoretical models and empirical findings that show that payments received by women are more likely to be spent on improving the welfare of children (for theoretical work, see Kanbur and Haddad 1994; Haddad, Hoddinott, and Alderman 1997; Basu 2006; for empirical research, see Schultz 1990; Thomas 1990; Doss 1996). This article explores the impact of this requirement in Nicaragua's

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Social Safety Net (Red de Protección Social, RPS), a conditional cash transfer program that pays women cash if their children attend school and they make regular visits to health care clinics.

Empirical evidence is limited on the effectiveness of targeting conditional cash transfers to women in order to raise school enrollment and affect other consumption outcomes. Three critical components of conditional cash transfers confound efforts to cleanly identify the impacts on school enrollment: income and two nonincome effects, conditionality and intrahousehold effects. The nonincome effects of targeted conditional cash transfers potentially include both the conditionality requirements of program participation (essentially a price effect) and the intrahousehold effects of providing women with the transfer. In other words, the education outcomes are also shaped by two distinct effects that are both part of the program's treatment.

Would a cash transfer without conditions achieve similar school enrollment outcomes (because education is a normal or even superior good for low-income families)? As for identifying the intrahousehold impacts of cash transfers targeted to women, an ideal experiment would randomly provide some transfers to men and some to women to determine how the impacts differ. Absent such a study design, one could examine household spending patterns of the treatment group (that are not conditional) to determine whether intrahousehold effects matter and in what ways. One could also look at the effects of intrahousehold differences in the control group (or the baseline data of the treatment group) to determine whether preexisting differences in education and spending patterns are consistent with power differences between men and women.

This article explores how RPS shapes education and spending patterns, with an eye on all three effects: income, conditionality, and intrahousehold impacts. On the intrahousehold side, the intention is to identify whether preexisting gender power structures are at work and to determine whether they are mitigated by the program, either through conditionality or by targeting transfers to women. By providing transfers directly to women, RPS also has the potential to empower women by increasing the resources they control. However, household resources are potentially fungible, raising a concern that other family resources may be reallocated away from children, offsetting the impact of the transfer. This phenomenon could be captured empirically by demonstrating smaller effects of conditional cash transfers on key outcomes in households in which men have more power. By targeting transfers to women, RPS has the implicit goal of helping ensure that money is spent on women and children, who might otherwise receive smaller shares of household resources in male-dominated households. Thus, it is also possible that the impacts of conditional cash transfer programs could be higher in male-dominated households if the transfers have the effect of changing behavior in the family that did not contribute to salutary outcomes for children.

The empirical analysis uses experimental methods that compare treatment and control groups. It adds to previous studies of the impact of conditional cash transfers by estimating heterogeneous program impacts based on

intrahousehold power differences. The power measure used is based on the ratio of years of school completed by the female and male heads of household. Women's intrahousehold power is assumed to increase as the female to male education ratio rises. This measure is arguably better in terms of exogeneity than male and female wage earnings used in some other studies, because earnings are endogenous to intrahousehold decision making and correlated with child wages, both of which could affect schooling decisions.

The article is organized as follows. Section I places this work within the context of the current literature and identifies its conceptual contributions. Section II presents the empirical approach to analyzing the impact of power and RPS on schooling and household spending. Section III provides background information on RPS along with descriptive statistics on variables of interest. Results of the estimations are reported in section IV, with conclusions and suggestions for further study provided in the last section.

I. Literature Review

This article links three related streams of literature. The first is the intrahousehold bargaining literature, which suggests that heterogeneous preferences between men and women can lead to different household decisions depending on power relations. The second attempts to measure the impacts of conditional cash transfer programs, with a focus on which aspects of the program (conditions or cash) are more effective in obtaining the desired results. The third seeks to determine whether there are demonstrable effects of targeting conditional cash transfers to women.

The theoretical and empirical literature on how households make decisions is well developed (Schultz 2002; Basu 2006). Two basic types of household models have been used to study decisions on child schooling and labor and the allocation of consumption expenditures between private and shared goods. Unitary models assume either that there is a benevolent dictator or that household members share the same preferences and pool their resources to maximize a single household utility function (Becker 1981). Households with heterogeneous preferences and a set balance of power are guided by a single utility function, even when one member is a nonbenevolent dictator. In these models, targeting transfers to women should have no impact on a household's allocation of spending (except through household income effects; Attanasio and Lechene 2002).

Nonunitary models generally examine decisions made by men and women who have distinct preferences and make decisions somewhere along a spectrum between full cooperation and conflict (McElroy and Horney 1981; Chiappori 1992; Basu 2006). Differences in bargaining power influence whose preferences gain greater expression in the household's choices. These models often assume that women have stronger preferences for child schooling and health outcomes; they therefore predict distinct effects of increases in nonwage income

depending on who receives the transfer. The motivation for giving conditional cash transfers to women is the assumption that women's higher propensity to spend on household shared goods will augment program effects.

Power relations between fathers and mothers have been shown empirically to affect child schooling outcomes (Binder 1999; Adato and others 2003; Iyigun and Walsh 2007), with relative income increases for women raising child school attendance. Thomas (1990) and Schultz (1990) show that nonwage income received by mothers is more likely than income received by fathers to be spent on children's health or schooling. The child's gender may also affect the resources received. Thomas (1994) shows that Brazilian mothers' nonwage income positively affected their daughter's health but not their sons'. Duflo (2003) shows that the impacts of exogenous income transfers through old-age pensions in South Africa were more likely to increase health outcomes of granddaughters of grandmothers than any other grandparent–grandchild relation. Emerson and Souza (2007) find that in Brazil fathers' education has a greater impact than mothers' education on sons' attainment, while mothers' education matters more to daughters' attainment.

Attanasio and Lechene (2002) and Adato and others (2003) examine the intrahousehold decision-making effects of conditional cash transfer programs. Both consider Progresa (now known as Oportunidades), a Mexican conditional cash transfer program. Attanasio and Lechene test the impact of Progresa and women's bargaining power as measured by the relative wages (potential and actual) of men and women on the share of household expenditures devoted to different goods (food, alcohol, transportation, services, and clothing).[1] The importance of women's power is supported by results that show that an increase in the relative income of women, including from Progresa's targeted cash transfer, has a positive relation to the share of expenditures allocated to children's clothing and food. Using a qualitative approach, Adato and others (2003) find that Progresa decreased the likelihood that husbands reported being the sole decision maker regarding spending on child health care, school attendance, and clothing, suggesting that the targeted cash transfer increased women's bargaining power.

One critical methodological and empirical issue in the intrahousehold literature is how to measure bargaining power. Adato and others (2003) suggest that each member's bargaining power is based on four factors: control over resources, influence over the bargaining process, interpersonal networks, and basic attitudinal attributes. Most research suggests that those with greater own assets or income (actual or potential) can exert more power, because they can withdraw from the household more easily (Doss 1996). In this sense, conditional cash transfers could increase the viability of women's exit options and

1. Because wage data are not available in the RPS sample, the results of Attanasio and Lechene cannot be compared with those obtained here.

strengthen their bargaining power, as long as women receive the transfer even if they leave the household.

This article uses the ratio of the number of years of school completed by the female to the number of years of school completed by the male head of household as a measure of power.[2] It assumes that as the female to male education ratio increases, women are likely to have more decision-making power. This measure is similar to but less crude than the literacy ratio Basu, Narayan, and Ravallion (2001) use (they use literacy because they assume that a literate member can withhold information from illiterate members to gain an advantage). One advantage of the education ranking approach over power measures that rely on relative wages or income is that education is exogenous to current income levels, which are themselves endogenous to fundamental household decisions regarding labor allocation. (Wages could not have been used in any case, because the RPS sample data did not collect wage information.)

Many studies suggest that women's power is both positively and monotonically related to spending on children and school enrollment. This assumption has been questioned by some recent work, however (Felkey 2005; Basu 2006; Lancaster, Maitra, and Ray 2006). Using an intrahousehold theoretical framework, Basu (2006) shows that if the woman has more power than the man, she will garner a greater share of the income produced by child labor. Based on this result, he posits that as her power continues to increase, she will receive more benefits from child labor, while the benefits of schooling may stay the same. He therefore concludes that if women become sufficiently more powerful than men, additional female power may actually result in a decline in school enrollment. Lancaster, Maitra, and Ray (2006) and Felkey (2005) provide empirical evidence in support of Basu's hypothesis, using samples from India and Bulgaria.

In Nicaragua even when women have as much education as their husbands, they still may not have equal power, because of cultural norms. However, at a certain point women with more education than their husbands could have sufficient power to sustain the nonmonotonic result suggested by Basu.

Basu's hypothesis is tested here by examining the nonlinear effects of the female to male education ratio on child school enrollment and household spending outcomes. The article also adds to these previous studies by testing whether cash payments made to mothers are likely to increase their power.

Previous work has shown that RPS and Progresa have been effective at increasing school enrollment rates and encouraging spending on food (for RPS, see Maluccio and Flores 2004; for Progresa, see Schultz 2004; Hoddinott and Skoufias 2004). The regression specification used by Hoddinott and Skoufias includes total consumption (including the transfer) as well as program

2. The female–male power ratio is usually given on a 0 to 1 scale. The ratio here ranges from 0 to 9. Though it could be normalized to a 0 to 1 scale, a non-normalized ratio is used for easier interpretation of the coefficients.

participation indicator variables. This combination helps provide estimates for the income and nonincome impacts of Progresa on food spending.

Hoddinott and Skoufias (2004) find that nonincome effects account for about half of the total impact of Progresa for total food expenditures and a higher percentage for expenditures on fruit, vegetables, and animal products. They place much of the credit for these impacts on lectures women received as part of Progresa that encourage proper nutrition through expenditures on fruit, vegetables, and milk. Attanasio and Lechene (2002) contend that the impacts may also be tied to targeting payments to women. Both could be correct: the health education lectures provided by Progresa could shape preferences, and targeted transfers could enhance women's bargaining power and thus their capacity to reveal those preferences. What is not clear is whether those expenditures may also have been viewed implicitly by the recipients as part of the conditionality of Progresa. In a simulation of the Bolsa Escola Program, a Brazilian conditional cash transfer, Bourguignon, Ferreira, and Leite (2003) find that both the conditionality of school attendance and income effects increase school enrollment.

Other research suggests that preexisting household conditions can shape the impact of a transfer. de Janvry and Sadoulet (2006) argue that conditional cash transfer programs can improve their results by moving from a uniform transfer size to one tied to easily observable household characteristics that alter program impacts. The relative education levels of parents are used here as an easily observable characteristic that may create heterogeneous impacts based on differences in preferences and power between men and women. Attanasio and Lechene (2002) find that payments made to women increase expenditures on food and schooling by increasing women's power (as measured by the ratio of female to male income), but they do not test for nonlinearities in this relation. It is possible that transfers to less powerful women may increase their power enough to participate in decision making and thus augment the targeting effect (as suggested by Adato and others 2003). Another possibility is that less powerful women may not be able to keep the whole transfer or that men may withdraw funds from the household to increase personal expenditures or leisure time. In the case of RPS, there is little evidence for this occurring, as increases in total household consumption were equivalent to the size of the transfer (Maluccio and Flores 2004).

de Janvry and Sadoulet (2006) include parental literacy in their estimation of the impact of Progresa on the child schooling decision. The impacts of the literacy of the mother and father are estimated as separate effects, not relative to one another as a measure of power. They find that both father's and mother's literacy increase schooling and decrease the size of the transfer required for the child to attend school. Their regression does not include controls for income, however, so parental schooling may well be capturing an income effect. Most important, they do not compare across households with

different female to male education ratios or other relative power measures to test for intrahousehold effects.

II. An Empirical Strategy for Estimating the Impacts of Power and RPS

Three components of household schooling and resource allocation decisions are examined here. The first is the effect of power structures ex ante of program effects on education outcomes and household spending patterns. The goal of this test is to see whether the power measure provides results that are consistent with the previously cited literature—that is, whether children of more powerful women are more likely to attend school and receive a larger share of resources.

The second component is an estimate that identifies income and nonincome effects. The control group is used to estimate income impacts on schooling and household spending. The income effects of a cash transfer in the control group which is the size of that of the RPS are then compared with the total effects of RPS, with the difference being an estimate of nonincome effects.The third component is the effect of women's power on program impacts on school attendance and household expenditure patterns. This component is measured by interacting variables that measure program impact and the power measure to test for heterogeneous program impacts by power.

The conventional approach to analyzing the treatment effects of conditional cash transfer programs is to use cross-sectional or panel data to compare outcomes in treatment and control groups. When the dependent variable of interest (school enrollment or consumption share) is not substantially different in the baseline year in control and treatment communities, program impacts can be measured using cross-sectional data in the treatment year. However, if initial conditions (in either the dependent or independent variables) are different in the treatment and control communities, then the full panel data should be used.

Difference-in-difference is the standard method used to measure impacts when initial conditions are not the same in control and treatment communities. This method measures the difference in the changes of the outcome of interest in treatment and control communities between the first year of treatment (year 1) and the baseline (year 0). If, for example, the outcome of interest in time period t is denoted as C_t for control communities and I_t for those in the treatment (intervention) group, the difference-in-difference program impact, denoted δ_1, is determined by $\delta_1 = (I_1 - I_0) - (C_1 - C_0)$. If through randomization in the baseline the outcome of interest is equally likely in both groups, the difference-in-difference impact is equivalent to $I_1 - C_1$.

Maluccio and Flores (2004) present a basic estimation equation for difference-in-difference (equation 1). Program impacts are measured using the difference-in-difference variables; δ_1 the coefficient on the term *Treat*, which is the interaction of two binary dummy variables for treatment year ($T = 1$); and the treatment status of the household ($RPS = 1$ for households in a treatment community).[3]

$$E_{ict} = \alpha_0 + \alpha_1 A_1 + \alpha_2 A_2 + \delta_0 RPS + \delta_t Treat + u_{ic} + v_{ict} \tag{1}$$

where E_{ict} = outcome variable of interest for household (or individual) i in community c at time t, $A_1 = 1$ if year is 2001, $A_2 = 1$ if year is 2002, $Treat = 1$ if treatment year is 2001 or 2002 and household is in RPS intervention in community c, u_{ic} = all (observed and unobserved) household-level (or individual-level) time-invariant factors, v_{ict} = unobserved idiosyncratic household (or individual) and time-varying errors, and α's and δ's = unknown parameters.

The number of years of school completed by the female head of household divided by the number of years of school completed by the male head of household—relative female power by schooling years (*rFPSY*)—is used to measure power. As 49 percent of males have completed zero years of school, 1 is added to both numbers of school years to create a defined ratio for all households:

$$rFPSY = \frac{(Number\ of\ years\ of\ school\ completed\ by\ female\ head\ +\ 1)}{(Number\ of\ years\ of\ school\ completed\ by\ male\ head\ +\ 1)}$$

The variable *rFPSY* is used to measure the impact of female power on school enrollment and household expenditures. The average *rFPSY* of both control and treatment groups was 1.4; comparison between treatment and control groups does not show statistically significant differences between the two groups.[4] The square of *rFPSY* is also used in order to test for the possible nonlinearity of the relation between power and these outcomes. The power measure is interacted with the treatment impact measure *Treat* to estimate the interactive effects of the power measure and *RPS*. The square of the power measure and the treatment impact measure are interacted to test for a nonlinear relation between power and *RPS* impacts. Schooling of the male and female heads of household (male_schooling and female_schooling) is added directly into the equation to control for the impact of the individual education levels. Finally, total per capita consumption (PCC) is included to estimate and control for income effects, including those from RPS transfers. When PCC is included,

3. For ease of interpretation, the two years are combined into a single measure of the impact of the treatment in a treatment year; doing so does not substantially affect the results.

4. A simple *t*-test of the mean of *rFPSY* between the treatment and control group yields a *t*-statistic of 0.37.

the estimated impacts of nonincome effects *Treat*RPS* in equation (2) for all households is represented by δ_1. The estimated impacts of power on RPS effects are represented by δ_2 and δ_3, respectively.

$$
\begin{aligned}
E_{ict} = {} & \alpha_0 + \alpha_1 A_1 + \alpha_2 A_2 + +\alpha_3 Male\_Schooling \\
& + \alpha_4 Female\_Schooling + \alpha_5 rFPSY + \alpha_6 rFPSY\text{^}2 \\
& + \delta_0 RPS + \delta_1 Treat + \delta_2 Treat * rFPSY + \delta_3 Treat * rFPSY\text{^}2 \\
& + \beta_1 \ln Consumption_{ct} + \beta_2 \ln Size_{ct} + u_{ic} + v_{ict}
\end{aligned} \tag{2}
$$

where $E_{ict} = 1$ if child i in community c at time t is enrolled in school, and 0 otherwise;

or, for expenditure data, E is expenditures for household i in community c at time t; $\ln Consumption = \log$ (*total consumption*) for household c in year t (baseline); and $\ln Size_t = \log$(*household size*) in year t.

For the first two components, this regression specification is similar to that of Hoddinott and Skoufias (2004), who estimate the impact of Progresa on food consumption—with some important distinctions. Their specification includes household characteristics, including the education of the head; the specification presented here includes the education of both the household head and his or her spouse separately and as a power measure. The same method used by Hoddinott and Skoufias (2004) is adopted here to separate income effects from nonincome effects by including total consumption in the regression as a control for income (including the transfer) as well as program effect measures. As Hoddinott and Skoufias note, if a conditional cash transfer alters consumption other than directly through transfers, total consumption becomes endogenous and may bias the results. This does not appear to be the case, as Maluccio and Flores (2004) find that the ex post increases in consumption for the treatment group are not statistically significantly different from the transfer.

The final component of the specification is the measurement of heterogeneous impacts of RPS based on household characteristics. The approach used is similar to that of two previous studies that measure the effect of economic shocks on RPS (Maluccio 2005) and Progresa (de Janvry and others 2006). In these studies, the heterogeneity across households is determined by exposure to these shocks. A measure of exposure to shocks is then interacted with the program eligibility variable. The approach here is similar, except that heterogeneity comes from the power measure rather than exposure to shock.

Both types of models (school enrollment and expenditure levels) are estimated using ordinary least squares (OLS). Estimating marginal effects is difficult using qualitative variable methods because of the interaction terms. Gitter and Barham (2006) find that OLS estimations of the enrollment impacts of

RPS are similar to probit predictions. In all of the estimations, errors are clustered at the community level to control for unobserved heterogeneity across communities. Because the household decision on school attendance may be different for boys and girls, separate estimates are performed for boys and girls.

III. Summary of the RPS Program and Descriptive Statistics

The RPS was implemented in 21 randomly selected communities in northwestern Nicaragua (in Madríz and Matagalpa). Another 21 communities in the region served as the control group. Three survey rounds were conducted in all 42 communities, one in 2000, before program implementation, and two during the program, in 2001 and 2002. This analysis uses a subsample of the 1,300 total households in which there is a head of household who is married. This subsample includes 1,129 households.[5]

Participation in treatment communities was extremely high, with uptake rates of more than 95 percent of those eligible to participate.[6] Benefits include a C$2,880 ($224) annual food security transfer.[7] Households with children ages 7–13 who had not completed the fourth grade were eligible for a bimonthly transfer for school attendance of C$1,440 per year and an additional C$275 for school supplies. The average household received C$3,885, or about 18 percent of total annual household consumption expenditures.

Baseline comparisons between treatment and control groups on outcomes and explanatory variables support the use of experimental methods to test for impact results. The average school enrollment for children of eligible age in the baseline sample was 77 percent, with about a 0.1 percent difference between treatment and control groups. The difference in aggregate total consumption and other consumption measures in treatment and control groups was not significantly different from zero.

In more than 40 percent of the households, male and female heads had completed the same number of years of school. The other 60 percent of households were divided evenly between those in which the male head had more schooling and those in which the female head had more schooling (table 1). The control and treatment groups had a similar average *rFPSY*. However, the control group had slightly more (45 percent compared with 40 percent) households in which the male and female households had the same number of years of completed

5. See Maluccio and Flores (2004) for information on the program design. They show that sample attrition rates were similar in both control and treatment communities.

6. Ninety-five percent of households were eligible to participate (Maluccio and Flores 2004). Program participation does not appear to have been affected by adult literacy, household income, or marital status.

7. Exchange rate of C$12.85 = US$1 is as of September 2000.

TABLE 1. Descriptive Statistics of Total Household Consumption and School Enrollment

Item	rFPSY[a]	Baseline			Difference-in-difference	
		Control	Treatment	*T*-statistic[b]	2001[c]	2002[d]
Total household consumption (córdobas)	$rFPSY < 1$	25,160	24,427	0.51	6,694**	2,773**
	$rFPSY = 1$	22,206	21,634	0.64	6,492**	5,114**
	$rFPSY > 1$	24,291	24,051	0.81	4,164**	4,489**
	Total	23,624	23,147	0.70	5,851*	4,284**
School enrollment, ages 7–13 (percent)	$rFPSY < 1$	78	78	−0.04	17**	14**
	$rFPSY = 1$	72	74	−0.65	15**	10**
	$rFPSY > 1$	86	80	1.50	20**	12**
	Total	77	77	−0.21	17**	11**

*Significant at the 10 percent level.

**Significant at the 5 percent level.

[a](years of schooling completed by female head +1)/(years of schooling completed by male head +1).

[b]Comparison of baseline control and treatment.

[c]$(\text{Treatment}_{2001} - \text{Control}_{2001}) - (\text{Treatment}_{2000} - \text{Control}_{2000})$.

[d]$(\text{Treatment}_{2002} - \text{Control}_{2002}) - (\text{Treatment}_{2000} - \text{Control}_{2000})$.

Source: Authors' analysis based on data described in text.

schooling, while the treatment group had slightly more (31 percent compared with 26 percent) households in which women had more years of schooling.

Consumption in households with more powerful females ($rFPSY > 1$) is similar to that in households with more powerful males ($rFPSY < 1$)[8]; consumption is lower in households in which $rFPSY = 1$. This result likely reflects the fact that this group includes a significant number of households in which neither spouse completed a year of school.

Previously cited literature suggests that female power is linked to higher school attendance and spending on children. The predicted relation is found in table 1, which shows that households with more powerful women ($rFPSY > 1$) have average baseline school enrollment rates of 82 percent (86 percent and 80 percent for the control and treatment groups, respectively), while households in which $rFPSY < 1$ have school enrollment of 78 percent. A *t*-test on average enrollment between the two groups yields a *t*-statistic of 1.78.

The relation between power and spending can be seen in some of the other explanatory variables (table 2). The previously cited literature suggests that households in which women have more power spend more on food and education of their children. However, in the RPS sample, there is weak evidence in

8. A *t*-test comparing the total consumption of households with $rFPSY < 1$ and $rFPSY > 1$ yields a *t*-statistic of 0.6. Relative to households with unequal levels of schooling, households in which $rFPSY = 1$ have a *t*-statistic of 3.6.

TABLE 2. Descriptive Statistics of PCC of Food, Milk, and Education (córdobas)

Item	rFPSY[a]	Baseline			Difference-in-difference	
		Control	Treatment	*T*-statistic[b]	2001[c]	2002[d]
Per capita food expenditures	$rFPSY < 1$	2,838	3,100	−0.97	673**	716
	$rFPSY = 1$	2,755	2,829	−0.31	745**	321
	$rFPSY > 1$	2,744	2,919	−0.77	611	603
	Total	2,801	2,969	1.20	618**	514
Per capita milk expenditures	$rFPSY < 1$	53	64	−0.65	57**	64**
	$rFPSY = 1$	29	30	−0.09	24	34**
	$rFPSY > 1$	75	66	0.52	67**	31**
	Total	49	50	0.20	48**	42**
Per capita education expenditures	$rFPSY < 1$	70	52	−0.93	23	75**
	$rFPSY = 1$	30	51	−1.90	7	19
	$rFPSY > 1$	78	94	−0.50	−22	47
	Total	56	65	−0.70	2	43

**Significant at the 5 percent level.

[a](years of schooling completed by female head +1)/(years of schooling completed by male head +1).

[b]Comparison of baseline control and treatment.

[c]$(\text{Treatment}_{2001} - \text{Control}_{2001}) - (\text{Treatment}_{2000} - \text{Control}_{2000})$.

[d]$(\text{Treatment}_{2002} - \text{Control}_{2002}) - (\text{Treatment}_{2000} - \text{Control}_{2000})$.

Source: Authors' analysis based on data described in text.

terms of total food spending, though food expenditures account for such a high proportion of total consumption (70 percent) that the deep poverty of these families may blunt differences in food expenditures evident elsewhere. Expenditure data come from self-reported household surveys on food consumption over a two-week period, which was scaled up for a year (Maluccio and Flores 2004).

Children in households with a powerful woman might receive a larger proportion of the household's food. Unfortunately, data on individual food consumption are not available, however. One way of determining whether this is the case is to look at milk consumption (including infant formula), which is more likely to benefit children. Milk consumption does appear to be related to women's power: in the baseline data, households with $rFPSY > 1$ consume more milk than those with $rFPSY < 1$ (the difference is significant at the 10 percent level using a simple *t*-test).

Maluccio and Flores (2004) use difference-in-difference estimates to measure program outcomes in their analysis of the total impact of RPS. Tables 1 and 2 provide basic difference-in-difference estimations for each of the *rFSPSY* measures for the outcomes of school enrollment, expenditures, and expenditure shares. In terms of school enrollment, the impacts are larger in households in

which the woman is powerful. All households saw at least a 15 percentage-point increase in enrollment the first year and a 10 percentage point increase the second year. Given that enrollment was at least 95 percent in all treatment communities, conditionality appears to be playing the dominant role. The effects were greater, however, in households with more powerful women.

One common concern is that men might withdraw money from the household for shared goods as women receive income from the transfer and use it for private consumption. If this concern were evident in the data, one would expect male-dominated households to have smaller expenditure impacts from RPS. In fact, in all cases except milk expenditures in 2002, impacts from RPS treatment as measured by difference-in-difference estimates show larger impacts for male-dominated households than for female-dominated households. This suggests that RPS transfers to women are having the intended impact of strengthening their potential to influence household consumption and investment choices rather than being captured by men who had pretransfer power advantages.

IV. Econometric Results

This section presents the results of econometric estimations of factors shaping school enrollment and household expenditures. There are three major components of these influences: the effect of female power ex ante of program impacts, income versus nonincome impacts, and variation in program impacts by female power. Two sets of regression results are reported: impacts on school enrollment and impacts on per capita expenditures for food, education, and milk. The regression specification is supported by the finding of an ex ante impact of female power on school enrollment and household expenditures on education. The results also show both income and nonincome effects from RPS, with nonincome effects being more important for schooling and income effects being more important for household spending patterns.

The econometric analysis of school enrollment outcomes for children ages 7–13 includes three sets of regressions: one for all children and one each for boys and girls (table 3). The impact ex ante of gender power differences can be seen through the two *rFPSY* measures. The coefficients on both *rFPSY* (positive) and $rFPSY^2$ (negative) are statistically significant for the sample of all children and girls. Children's schooling is positively associated with maternal power, except when the *rFPSY* ratio is larger than 5 (the case for about 3 percent of the children in the sample), at which point further maternal schooling begins to reduce enrollment. These results are consistent with the nonmonotonic relation between power and schooling found by Basu (2006). However, as the negative effect is observed only at the far tail of the distribution, it could also indicate that there are monotonic but diminishing returns to power and schooling. For boys the quadratic term is not statistically significant, and the results suggest a positively monotonic relation between female power (*rFPSY*) and school enrollment.

TABLE 3. Regression on School Enrollment: Impacts of Power and RPS

	All Children		Boys		Girls	
Variable	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error
RPS 1 if treatment group	−0.018	0.018	0.026	0.025	−0.068**	0.024**
1 if year 2001	0.048**	0.016**	0.055**	0.024**	0.036	0.022
1 if year 2002	0.062**	0.016**	0.074**	0.024**	0.044**	0.022**
Male household head school years	0.016**	0.004**	0.022**	0.006**	0.010	0.006
Female household head school years	0.008**	0.004**	0.008	0.006	0.009	0.006
rFPSY	0.053**[a]	0.018**	0.056**[a]	0.028	0.044**[a]	0.024**
rFPSY^2	−0.006**	0.002**	−0.004	0.004	−0.007**	0.003**
1 if RPS group and treatment year (*Treat*)	0.166**	0.030**	0.153**	0.045**	0.184**	0.041**
*Treat***rFPSY*	−0.030	0.026	−0.034	0.041	−0.025	0.033
*Treat***rFPSY*^2	0.004	0.005	0.003	0.008	0.005	0.006
Ln(*household consumption*)	0.054**	0.010**	0.047**	0.015**	0.063**	0.014**
Ln(*household size*)	0.000	0.016	−0.002	0.022	−0.003	0.023
Constant	0.153	0.098	0.172	0.146	0.137	0.132
R-squared		0.09		0.10		0.09
Number of observations	4,593		2,337		2,256	

**Significant at the 5 percent level.

[a]Joint *F*-test of coefficients on *rFPSY* = *rFPSY*^2 = 0 significant at the 5 percent level.

Source: Authors' analysis based on data described in text.

Additional controls were added to the regressions for the number of school years completed by the male and female heads of household. The pooled regression shows that an additional year of school for the male head is twice as important as an additional year for the female head. These differences break mainly along child gender lines. In the sample with just boys, schooling of the male head has a larger impact (0.022) for each year of schooling compared with an extra year for the female head (0.008), although an *F*-test of *male_years* = *female_years* is not statistically significantly different from zero. This result suggests that additional school years of the male head and female head of household may have equal impacts. The coefficient estimates on both heads of household are equal for girls, although neither is statistically significant.

The second component of interest—the comparison of income and nonincome effects—is captured by the RPS impact measures (*Treat*), because income effects are controlled for by using total household consumption (including RPS transfers). The RPS nonincome impacts on school enrollment for both years are measured at 16.6 percent for the total sample, with the impact on girls slightly higher but not statistically significantly so. This estimate is lower than that of Maluccio and Flores (2004), who estimate total impacts (income and nonincome) of 22 percent for 2001 and 18 percent for 2002. This difference suggests that the income effects are on the order of 1.4–5.4 percentage points, or about 25–33 percent of the nonincome effects.

Another way to estimate income effects is to use the coefficient estimate on the variable of the natural log of total household consumption, ln*Consumption*. The difference in the average ln*Consumption* between treatment and control was 0.35 in 2001 and 0.24 in 2002. With a coefficient estimate of 0.054 on total household consumption, these differences would suggest that transferring the size of RPS would increase schooling by 1–2 percentage points.[9] This impact is slightly less than but consistent in magnitude with the difference between the estimated nonincome effects obtained here and the total effects obtained by Maluccio and Flores (2004).

The combined impacts of power and RPS on school enrollment are examined through the interaction of the nonincome treatment impact measure (*Treat)* and the power ratio (*rFPSY)*. This interaction term and its square are not statistically significant, suggesting that the impacts of RPS treatment do not vary depending on the power of the female head of household. Furthermore, when the interaction of *Treat* and $FPSY^2$ is omitted, the relation between RPS impacts and power (*Treat*rFSPY*) is negative but not statistically significant.[10]

9. One concern is that with treatment the impact of total consumption on schooling may vary when compared with ex ante consumption patterns. Models that separately estimate the impact of consumption on schooling for only the control group yield coefficients that are not substantially different from the model presented above. These results are available from the authors on request.

10. These results are omitted because of space constraints; they are available from the authors on request.

TABLE 4. Regression on per Capita Expenditures by Category: Impacts of Power and RPS

	Per Capita Spending on Food		Per Capita Spending on Education		Per Capita Spending on Milk	
Variable	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error
RPS 1 if treatment group	56.3	106.0**	1.4	11.9	1.8	8.7
1 if year 2001	−370.8**	99.3**	33.3**	11.2**	21.1**	8.0**
1 if year 2002	−423.0**	96.9**	56.4	10.9**	3.4	7.8
Male household head school years	−20.7	25.8**	4.6	2.9	1.1	3.1
Female household head school years	58.2**	25.0**	5.7**	2.8**	9.6**	4.4**
Relative female power by schooling years (*rFPSY*)	−171.3	113.1	18.2	12.7	−11.3	11.7
rFPSY^2	16.7	14.7	−2.7	1.7	1.6	1.4
1 if RPS group and treatment year (*Treat*)	445.2**	175.9	−0.7	19.8	72.0**	26.6**
*Treat*rFPSY*	−237.0[a]	144.7	1.1	16.3	−45.6[a]	32.3
*Treat*rFPSY*^2	25.3	24.5	0.1	2.8	8.4	7.0
Ln(*household consumption*)	1,895.3**	52.3**	113.0**	5.9**	24.0**	7.2**
Constant	−1,5524.0	521.0	−1,078.7	58.6	−193.8	70.2
R-squared		0.37		0.16		0.03
Number of observations		2,550		2,550		2,550

**Significant at the 5 percent level.

[a]Joint *F*-test of coefficients on *rFPSY* = *rFPSY*^2 = 0 or Treat**rFPSY* = Treat**rFPSY*^2 = 0 significant at 5 percent level.

Source: Authors' analysis based on data described in text.

The impacts of power, RPS, and income on three types of expenditures (education, food, and milk) are estimated next (table 4). Consistent with the enrollment results, for most households female power as measured by *rFPSY* has a positive relation with spending on education and a negative quadratic effect. Similar to the enrollment results, the maximum value of power for enrollment occurs at an *rFPSY* of about 4 (which applies to about 4 percent of the sample). Unlike education, spending on food or milk in particular does not show a statistically significant relation with power. However, all three expenditure categories show statistically significant impacts of the number of years of schooling of the female household head. The interactive effects of female

power and RPS on the three types of expenditures do not yield statistically significant coefficients.

The impacts of RPS on expenditures can be seen through two variables: *Treat*, which represents nonincome effects, and ln*Consumption*, which captures income effects measured through household consumption. Consumption of food, education, and milk increased with an increase in total household consumption, including consumption increases from RPS. Examination of the nonincome impacts of RPS as measured by the variable *Treat* shows significant positive impacts on spending for milk and food but not for education. The nonincome impacts on milk and food are substantial. The estimated nonincome impact on milk expenditures per capita is $C72, more than twice the average baseline consumption. The estimated impact of RPS on food consumption per capita is $C445, nearly a 15 percent increase over baseline consumption.

The empirical analysis yields three main results. First, more female power generally leads to higher school enrollment and greater spending on education. However, consistent with the emerging literature, for households with extremely powerful women, more female power may begin to reduce schooling or at least have no additional marginal impact. Second, nonincome effects of RPS are extremely important for school enrollment, which may not be surprising given the conditionality of the program. Nonincome impacts are evident on both food and milk per capita expenditures. Although the RPS program encourages spending on these items, such spending is not required, suggesting that nonincome effects other than conditionality had an impact. Two likely possibilities are the targeting of transfers to women and the nutrition education programs. Third, there is no evidence of a decreased impact of RPS on spending or schooling when women are less powerful.[11] Overall, these results support the hypothesis that the goals of school enrollment and nutrition can be improved by directing funds to women and requiring school attendance.

V. Conclusion

A large body of literature on intrahousehold bargaining suggests a positive relation between women's power and the amount of resources devoted to children. This article uses a power measure based on the ratio of years of schooling of female to male household heads to study the impacts of a conditional cash transfer program in Nicaragua. This measure is generally consistent with the expected positive relation between women's power and child schooling, although, as suggested by Basu's (2006) model, past a certain

11. In a separate model that omits the quadratic interaction term, *Treat*FPSY*^2, the linear term is negative and statistically significant.

point the marginal impact of additional female power on children's enrollment may be zero or negative.

In targeting transfers to women, RPS and other conditional cash transfer programs seek to increase women's potential to spend money on children's schooling and other goods, such as food, that can improve children's human capital. The analysis provides evidence of the effectiveness of RPS transfers in improving the allocation of household resources toward women and children. The nonincome effects of the program are responsible for most of the nearly 20 percent increase in school enrollment; the targeting of transfers to women plays a secondary role.

Running the enrollment regressions separately for girls and boys reveals that the mother's relative education level always has a positive impact on boys' education outcomes. The results for girls are consistent with the nonlinear relation suggested by Basu (2006): when women's power passes a certain threshold girls' enrollment falls. Basu hypothesizes that parental power may influence the percentage of benefits from child labor garnered by each adult. This percentage may also depend on the child's gender. The nonmonotonic relation for girls but not boys suggests that when girls leave school, the percentage of the benefits received by the female head of household is larger than it is for boys.

The expenditure analysis supports the effectiveness of targeting transfers to women: RPS nonincome effects accounted for a more than doubling of milk expenditures and 15 percent of the increase in food expenditures. This effect may be shaped as much by women's education as it is by their power. However, the expenditure analysis shows that the education level of the female head has a positive impact on expenditures, but that the impact of their relative power is weaker. Overall, the empirical results suggest that targeting transfers to women has been effective at increasing key welfare outcomes for all households, even those with greater male power. But these estimates are inferences from econometric analyses and not direct measures of treatment effects of targeting transfers to women from a randomized experiment. If one goal of conditional cash transfer programs is to strengthen and broaden the quality of information regarding the efficacy of targeting transfers to women, more detailed questions on how households allocate their resources or possibly experiments that provide targeted and nontargeted transfers should be used in future program designs.

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