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

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

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Abstract

The Red de Protección Social (RPS) in Nicaragua is one of a growing number of conditional cash transfer programs that pay households cash stipends in exchange for school attendance and regular visits to health clinics. A key feature of these programs is that the cash is given to the woman head of household. The rationale stems from previous research in the developing world that transfers to women are more likely to be spent on health, nutrition, and education of children and thus to reinforce the goals of these programs. One concern is that less powerful women may not be as impacted by program funds, because males in those household will decrease their contributions in response to the transfers. Utilizing randomized experimental data from RPS, we test for heterogeneous program impacts on school enrollment and spending based on a woman's power as proxied by her education relative to her husband . Our results confirm previous findings that generally more household resources are devoted toward children when women are more powerful; however, when women's power greatly exceeds her husband's additional female power lowers school enrollment., Additionally we find that RPS' impacts on schooling are substantially larger than expected income effects estimated from the control group, although we do not find evidence that female power alters RPS impacts on school enrollment. In terms of spending effects, RPS increased food and education expenditures in all households, yet this impact is attributable mostly to income effects. Finally, we find RPS had non-income impacts on milk expenditures particularly for less powerful women..

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Many poverty alleviation programs in developing countries stipulate that payments or benefits be given to a woman head of household (Rawlings and Rubio, 2005) The justification for targeting women is based on theoretical models and empirical findings which 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 et al. 1997; Basu, 2006 and for empirical see: Schultz, 1990; Thomas, 1990; and Doss, 1996). This paper explores the impact of this requirement in a program in Nicaragua, the Red de Protección Social (RPS), which is a conditional cash transfer (CCT) program that gives women a cash payment if they meet requirements that include child school attendance and regular visits to health care clinics.

Empirical evidence for the effectiveness of targeting CCTs to women is limited in terms of school enrollment and other consumption outcomes. Three critical components of CCTs confound efforts to cleanly identify impacts on school enrollment. First is the basic difference between income and non-income effects. Would a cash-transfer without conditions achieve a similar school enrollment outcome because education is a normal or even superior good in the demand profile of low-income families? Then, non-income effects of targeted CCTs 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, education outcomes are also shaped by two distinct effects that are both part of the program's treatment. Alternatively, an ideal randomized experiment aimed at identifying intrahousehold impacts of cash transfers targeted to women would include randomly targeting some transfers to men and others to women to see how impacts differ. Absent that kind of study design, one can examine

household spending patterns of the treatment group (that are not conditional) to see whether intrahousehold effects matter and in what ways. One can also look at the effects of intrahousehold differences in the control group (or the baseline data of the treatment group) to see whether pre-existing differences in education and spending patterns are consistent with gender power differences.

The goal of this article is to explore how RPS shapes education and spending patterns with an eye toward all three effects: income, conditionality, and intrahousehold impacts. On the intrahousehold side, the intention is both to identify whether pre-existing gender power structures are at work and whether they are mitigated by the program (either through conditionality in the case of schooling) and/or by targeting transfers to women. Although transfers are targeted directly to women, it is important to note that household resources are potentially fungible. This feature raises a common concern that other family resources may be reallocated away from children, effectively offsetting the impact of the transfer. Empirically, this concern could be captured by demonstrating smaller effects of CCTs on key outcomes in households where men have more power. Clearly, by targeting transfers to women, RPS has the implicit goal of helping to insure 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 CCT programs could be higher in male dominated households if they have the effect of changing behavior in the family that previously cut against salutary outcomes for women and children.

The empirical analysis utilizes experimental methods that compare treatment and control groups so that we can estimate program impacts. We add to previous CCT impact studies by

estimating heterogeneous program impacts according to intrahousehold power differences. The power measure used in this paper - as in Ravallion et al. (2003)and Felkey (2006) - is based on the ratio of years of school completed by the female and male heads of household. We assume that the female's intrahousehold increases as the female to male education ratio rises. The measure is arguably better in terms of exogeneity than male and female wage earnings sometimes used in other studies, because earnings are endogenous to intrahousehold decision-making and correlated with child wages, both of which could impact schooling decisions.

The remainder article is organized as follows. The first section places this work within the context of the current literature and identifies the conceptual contributions of this article. 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 offered in Section V.

I: Literature Review

This paper links three related lines of literature. The first is an intra-household bargaining literature, which suggests heterogeneous preference between men and women can lead to different household decisions depending on power relationships. The second attempts to measure the impacts of conditional cash transfer programs, with a particular focus on which aspects of the program (conditions or cash) are more effective in obtaining the desired results. Finally, the third line seeks to find whether there are demonstrable effects of targeting CCTs to women.

The theoretical and empirical literature on how households make decisions is well developed (Basu, 2006; Schultz, 2002). Two basic types of household models have been

deployed to study decisions on child schooling versus labor and the allocation of consumption expenditures between private and shared goods. Unitary models are canonical (Becker, 1981); they assume either a benevolent dictator, or that household members share the same preferences and pool their resources to maximize a single household utility function. In these models, targeting transfers to women should have no impact on a household's allocation of spending other than through household income effects (Attanasio and Lechene, 2002).

Non-unitary models generally examine decisions made by a man and a woman who have distinct preferences and make decisions somewhere along a spectrum of full cooperation or some mix of cooperation and conflict (Chiappori, 1992; McElroy and Horney, 1981, Basu 2006). Differences in bargaining power influence whose preferences gain greater expression in the household's choices. Typically, these models assume that women have stronger preferences for child schooling and health outcomes and thus predict distinct effects of increases in non-wage income, such as a conditional cash transfer program, based on who receives the transfer. In effect, conditional cash transfer programs have motivated their decision to target transfers to women on these non-unitary models, with the assumption that women's higher propensity to spend on household shared goods will augment program effects.

Empirically, power relationships between fathers and mothers have been shown to affect child schooling outcomes (Binder, 1999; Adato et al., 2003; Iyigun and Walsh, Forthcoming), with relative income increases for women raising child school attendance. For example, Thomas (1990) and Schultz (1990) both show that non-wage income received by mothers was more likely than income received by fathers to be spent on children's health or schooling. More recent work by Duflo (2003) has shown 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 relationship. Her finding suggests the added nuance that impacts of transfer payments may also vary depending on the child's gender. Similarly in Brazil, Emerson and Souza (2007) find that father's education has a greater impact on sons than mother's education and the reverse is true for daughters.

Two studies have examined explicitly the impacts of conditional cash transfers using an intra-household framework, Adato et al. (2003) and Attanasio and Lechene (2002). Both consider Progresa (now known as Oportunidades), a Mexican conditional cash transfer program. Using a qualitative approach, Adato et al. (2003) finds that Progresa decreased the likelihood that the husband would report himself as the sole decision maker in terms of spending on child health care, school attendance, and clothing, suggesting that women's bargaining power is improved by the targeted cash transfer. In a more quantitative approach, Attanasio and Lechene (2002) 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 role of women's power is supported by results that show an increase in the relative income of women, including the increase from Progresa's targeted cash transfer, has a positive relationship on the share of expenditures on children's clothing and food.

One critical methodological and empirical issue in the intrahousehold literature is how to measure bargaining power. Adato et al. (2003) suggest that each member's bargaining power will be based on four factors: who controls which resources, influences used to alter the bargaining process, interpersonal networks, and basic attitudinal attributes. Most works suggest

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 that sense, conditional cash transfers could increase the utility of the women's exit option and her bargaining power, as long as women would still receive the transfer even if they left the household.

This article utilizes a ratio of the number of years of school completed by the female to the male head of household as a measure of power. We assume that as the female to male education increases, the femaile is likely to have more decision-making power. In addition, education is likely to be positively correlated with both current income potential and assets before marriage as people from wealthier families are more likely to attend school. More education may also provide power as Ravallion et al. (2003) find when one household member is literate and others are not, that the literate member can withhold information from the illiterate members to gain an advantage. The power measure also has the advantage of being exogenous to current income levels because as Hoddinott and Haddad (1995) suggest the relative wage/income measure is endogenous to other household decisions. In the RPS sample data, the relative wage measure would not be effective in any event, because women's labor force participation rates are around 10% for both literate and illiterate women, which would render potential or actual income largely unobserved.[2]

A great majority of the intrahousehold literature suggests that female power is both positively and monotonically related to spending on children and school enrollment. However, this assumption has been question by a few recent works (Basu, 2006; Lancaster et al. 2006; Felkey, 2006). Basu (2006) in an intra-household theoretical framework demonstrates that if female power is above that of the male's then 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 to child labor, while the benefits of schooling may stay the same. He therefore concludes if women become sufficiently more powerful than men, then additional female power may actually decrease school enrollment. Lancaster et al. (2006) and Felkey (2006) provide empirical evidence in support of Basu's hypothesis using samples from India and Bulgaria, respectively. In Nicaragua even when women have equal education to their husbands they still may not have equal power due to culture norms. However, at a certain point, women with more education then their husbands could have sufficient power to sustain the non-monotonic result suggest by Basu. We provide a test of Basu's hypothesis by examining non-linear effects of the female to male education ratio. Additionally, our paper adds to these previous works by testing whether a cash payment made to the mother is likely to increase her power.

Previous works have shown that RPS and the comparison Mexican program, Progresa, have been effective at increasing school enrollment rates and encouraging spending on food (for RPS see Maluccio and Flores, 2005; for Progresa see Schultz, 2004; Hoddinott and Skoufias, 2004). The regression specification utilized by Hoddinott and Skoufias (2004) includes total consumption (including the transfer) as well as program participation indicator variables. This combination helps to provide estimates, respectively, for income and non-income impacts of Progresa on food spending. They find, for example, that for total food expenditures non-income effects account for around half of the total impact of Progresa and a higher percentage for expenditures on fruit/vegetable and animal products. They place much of the credit for these impacts on lectures women received as part of Progesa 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 the payment to women, and clearly 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 might also have been viewed implicitly by the recipients as part of the conditionality of Progresa. In a simulation of Bolsa Escolar, a Brazilian CCT, Bourguignon et al. (2003) find that both the conditionality of school attendance and income effects increase school enrollment.

Finally, a third line of literature suggests that pre-existing household conditions can shape the impact of a transfer. De Janvry and Sadoulet (2006) argue that CCTs like RPS and Progresa may improve upon their results by moving from a uniform transfer size to one tied to easily observable household characteristics that alter program impacts. We utilize parental education as an easily observable characteristic that may create heterogeneous impacts based on differences in preferences and power among men and women. Although Attanasio and Lechene (2002) find payments made to women increase expenditures on food and schooling by increasing women's power (as measured by the ratio of female/male income), they do not test for non-linearities in this relationship. One possibility is 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 et al. (2004). Another possibility is that less powerful women might not be able to keep the whole transfer or males might withdraw funds from the households.

It should also be noted that De Janvry and Soudelet (2006) include parental literacy in their estimation of the impact of Progresa on the child schooling decision. However, the impact

of the mother and father's literacy are estimated as separate effects and not relative to one another as a measure of power. They find that both father and mother's literacy increases schooling and decreases the size of the transfer required for the child to attend school. However, their regression does not include controls for income, so parental schooling may also be capturing an income effect. Most importantly, they do not compare, as we do below, across households with distinct female to male education ratios to explore the potential of this measure as a proxy for bargaining power.

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

We explore three components of household schooling and resource allocation decisions. First, we test if education outcomes and household spending patterns are impacted by power structures *ex-ante* of program effects. The goal of this test is to see if our power measure provides consistent results with the previously cited literature that more powerful women's children are more likely to attend school and receive a larger share of resources.

The second component is to devise an estimate that identifies income and non-income effects. To do so, we utilize the control group to estimate income impacts on schooling and household spending. The income effects of a cash transfer the size of RPS in the control group are then compared to total effects of RPS, with the difference of these two being an estimate of non-income effects. The third component is to examine whether women's power may affect program impacts in terms of school attendance and household spending. This is done by interacting variables which measure program impact and the power measure to test for heterogeneous program impacts by power.

The conventional approach to analyzing treatment effects of conditional cash transfer programs such as RPS is to use cross-sectional or panel data to compare outcomes in treatment and control groups. When the dependent variable of interest (e.g. school enrollment or consumption share) is not substantially different in the baseline year in control and treatment communities, then program impacts can be measured using cross-sectional data in the treatment year. However, if initial conditions (in either dependent or independent variables) are different in treatment and control communities, then the full panel data should be utilized. Difference-in-difference (DID) is the standard method used to measure impacts when initial conditions are not the same in control and treatment communities. The DID 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. For example, if 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) then the difference-in-difference program impact, denoted δ_1, is determined using the following result, $\delta_1 = (I_1 - I_0) - (C_1 - C_0)$. If through randomization in the baseline, the outcome of interest is equally likely in both groups, then the DID is equivalent to $(I_1 - C_1)$.

Maluccio and Flores (2004) in their analysis of RPS present a basic estimation equation for DID, which is presented in equation 1 below. Program impacts are measured using the DID variables, δ_1 the coefficient on the term *Treat* which is the interaction of two binary dummy variables for treatment year (T = 1) and if the household is in a treatment community (RPS =1). [3]

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

Where:

E_{ict} = outcome variable of interest for household (or individual) *i* in community *c* at time *t*

A_1 = (1) if Year 2001

A_2 = (1) if Year 2002

Treat = (1) if Treatment Year (2001 or 2002) & if household is in RPS intervention in community *c*

μ_{ic} = all (observed and unobserved) household- (or individual-) level time-invariant factors

ν_{ict} = unobserved idiosyncratic household (or individual) and time-varying error

and all of the α's and δ's are unknown parameters.

In order to measure power we create a ratio we will henceforth refer to as relative Female Power by Schooling Years (rFPSY). The ratio is 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. Since 49% of males have zero years of school completed, we add one to both number of school years to create a defined ratio for all households. Therefore rFPSY can be calculated as follows

$$rFPSY = \frac{(\#of\ Years\ School\ Completed\ Female\ Head\ +1)}{(\#of\ Years\ School\ Completed\ Male\ Head\ +1)}$$

The variable rFPSY is used to measure the impact of female power on school enrollment and household expenditures. In addition we include the square of rFPSY in order to test for the possible non-linearity of the relationship between power and these outcomes. Next to estimate the interactive effects of the power measure (rFPSY) and RPS we interact the power measure

with the treatment impact measure *Treat*. Additionally we interact the square of the power measure and treatment impact measure to test for a non-linear relationship between power and RPS impacts. Finally, in order to estimate and control for income effects including those from RPS transfers, we include total per capita consumption (PCC). When PCC is included the estimated impacts of non-income effects T*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.

$$E_{ict} = \alpha_0 + \alpha_1 A_1 + \alpha_2 A_2 + \alpha_3 rFPSY + \alpha_4 rFPSY\text{^}2 + \delta_0 RPS + \delta_1 T * RPS + \delta_2 T * RPS * rFPSY + \delta_3 T * RPS * rFPSY\text{^}2 + \beta_1 \ln Consumption_{ct} + \beta_2 \ln Size_{ct} + u_{ic} + v_{ict} \quad (2)$$

E_{ict} = In community *c* at time *t* is:

If child "*i*" enrolled in school (1) else zero or

household expenditures on item (E) by household *i* ,

F_i = (1) if Female adult is literate

M_i = (1) if Male adult is literate

MF_i= (1) if Male and Female adults are literate

lnConsumption = ln(total consumption) for household c in year t (baseline)

RPS_c = (1) if RPS intervention in community *c*

$lnSize_t$ = ln (household size) in year t.

For the first two components, the above regression specification is similar with some important distinctions to Hoddinott and Skoufias (2004) who estimate Progresa's impact on food consumption. Their specification includes household characteristics including the education of the head, while our specification includes the education of both the head and their spouse. To separate income effects from non-income effects, we use the same method as Hoddinott and

Skoufias (2004), including total consumption in the regression as a control for income (including the transfer) as well as program effect measures. As they note, if a conditional cash transfer alters consumption other than directly through transfers, then total consumption becomes endogenous and may bias the results. However, this does not appear to be the case, as Maluccio and Flores (2005) 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 to measure heterogeneous impacts of RPS based on household characteristics. Our approach is similar to two previous studies which measure the effect of economic shocks (Maluccio, 2005 and De Janvry et al. 2006) on the impacts of RPS and Progresa, respectively. In these studies the heterogeneity among households is determined by exposure to these shocks. A measurement for exposure to shocks is then interacted with the program eligibility variable. Our approach is similar with the only difference being that our heterogeneity comes from the power measure rather than exposure to shock.

All of the models (school enrollment and expenditure levels and shares) are estimated using OLS. Due to the interaction terms, estimating marginal effects becomes quite difficult using qualitative variable methods. In Gitter and Barham (2006), we 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 between communities. Finally, since the household decision on school attendance may be different for boys and girls, we also perform separate estimates based on the gender of the child.

III. Summary of the RPS Program and Descriptive Statistics

The Red de Protección Social (RPS) was implemented in 21 randomly selected communities in Northwestern Nicaragua (Madríz and Matagalpa). An additional 21 communities in the region provide a control group. Three survey rounds were taken in all 42 communities, one in the year 2000 before program implementation and two surveys during the program one each in the years 2001 and 2002. This analysis uses a sub-sample of the 1300 total households where there is head of household who is married or has a spouse, which accounts for 1129 households.[4]

Participation in treatment communities was extremely high as uptake rates were over 95% of those eligible participated.[5] Benefits include a C$2,880 ($224) annual food security transfer.[6] Households with children ages 7-13 who have not completed the fourth grade were eligible for a bi-monthly transfer for school attendance of C$1440 per year and an additional C$275 for school supplies. The average household received C$3885 ($302), or about 18% 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. For example, the average school enrollment for children of eligible age (7-13) in the baseline sample was 77%, with about a 0.1% difference between treatment and control groups. The difference in aggregate total consumption and other consumption measures in treatment and control groups are not significantly different from zero.

The impact of relative education of the male and female household head is shown in Tables 1 and 2. Both tables utilize parental literacy in place of the rFPSY measure for ease of

presentation.[7] The parental literacy measure suggests that relative female power based on education is a useful measure as it is generally exogenous to consumption and treatment. Furthermore, the variable is consistent with the previously cited literate that suggests children of more powerful women receive a larger share of household recourses. Of all the households in 23% both adults are literate, 47% both are illiterate, 14% women only literate, and in 16% male only literate. Similar to other key measures there are no statistically significantly differences in percentage of households in any one power group between treatment and control communities.

One possible concern with the literacy measure is that it is tied to income, thereby creating endogeneity problems. A comparison in Table 1 of total consumption and the literacy measure shows the two measures are not strongly linked, as would be expected. In particular, the difference in total consumption between the two groups that are likely to have divergent power (male only and female only) is not statistically significant. There is less than a 10% difference in total consumption for households with one member literate and those with neither adult head being literate; however, that difference is not statistically different from 0 at the 5% level. Households where both members are literate have average consumption over 20% greater than those where one household head is literate and close to 30% greater than where neither male nor female head is literate.

Previously cited literature suggests that female power is linked to higher school attendance and spending on children. The relative literacy measure in Table 1 shows the predicted relationship in terms of school enrollment as households with a literate female (both or female only) had child school enrollment rates between 82% - 87% in the baseline year, compared to between 71% and 75% in households without a literate female. In particular, the

difference in school enrollment rates for children in the baseline between female only and male only literate households is ten percentage points.

The relationship between power and spending can be seen in some of the explanatory variables (See Table 2). The previously cited literature suggests that households where females have more power will spend more on food and education of their children. However, in the RPS sample there is weak evidence in terms of total food spending, though it is worth highlighting that food expenditures account for such a high proportion of total consumption (70%) that the deep poverty of these Nicaraguan families might blunt differences in food expenditures evident in other places. Unfortunately, we do not have data on individual food consumption as children in households with a powerful female might receive a greater proportion of food. We, therefore, also look specifically at milk consumption (including infant formula), which is more likely to benefit children. Milk consumption does shows signs of being related to women's power; in the baseline data female-only literate households consume more milk than do male-only literate households.

In their analysis of the total impact of RPS, Maluccio and Flores (2004) use difference-in-difference estimates to measure program outcomes. Tables 1 and 2 provide basic difference-in-difference estimations for each of the four literacy groups for the outcomes of school enrollment, expenditures, and expenditure shares respectively. In terms of school enrollment, the impacts are larger in households where the female is not literate. This could be due to either intrahousehold effects or conditionality requirements. Because enrollment was at or above 95% in all of the power groups in treatment communities, we suspect that conditionality plays the dominant role. Nonetheless, the difference in school enrollment in control groups in 2001 and

2002 results in greater estimated RPS impacts occurring in households without a literate female adult.

One common concern is that men might withdraw money from the household as women receive income from the transfer. If this concern was evident in the data, we would expect male only literate households to have smaller expenditure impacts from RPS. However, in all cases, impacts from RPS treatment as measured by difference-in-difference estimates shows larger impacts for male only literate households than in females only literate households (except food expenditures 2002). This outcome 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 pre-transfer power advantages.

IV. Econometric Results

This section presents the results of estimations of influences on child schooling and household spending. There are three major components of these influences: the effect of female power *ex-ante* of program impacts, income versus non-income impacts of the program, and variation in program impacts by female power. Two sets of regression results are reported: impacts on school enrollment in Table 3, impacts on per capita expenditures for food, education and milk in Table 4. The regression specification is supported by the finding of *ex-ante* impact of female power for child school enrollment and household spending on education. Our results also show both income and non-income effects from RPS, with non-income effects being more important for schooling and income effects being more important for household spending.

We begin with the econometric analysis of school enrollment outcomes for children ages 7-13, which is presented in Table 3. The table includes three sets of regressions: one for all

children and one each for boys and girls. The impact *ex-ante* of gender power differences can be seen through the two rFPSY measures, which increase as females have a higher ratio of education to their male partners. These results are also consistent with the non-monotonic relationship between power and schooling suggested by Basu (2006). The coefficients on both rFPSY (positive) and rFPSY^2 (negative) are statistically significant for the sample of all children and girls. Generally, children's schooling is positively associated with maternal power, except when the rFPSY ratio is larger than 5 (this constitutes about 3% of the children), at which point further maternal schooling begins to decrease enrollment. For boys, the quadratic term is not statistically significant, and the results suggest a positively monotonic relationship between female power (rFPSY) and school enrollment.

The second component of interest - the comparison of income and non-income effects – is captured by the RPS impact measures (*Treat)*, because we control for income effects by using total household consumption (including RPS transfers). The RPS non-income impacts on school enrollment for both years were measured at 15.5% for the total sample with the impact on girls slightly higher, but not statistically significantly so. This estimate compares to estimated total impacts (income and non-income) of 22% and 18%, for 2001 and 2002 respectively by Maluccio and Flores (2004). This difference between our non-income estimates and the Maluccio and Flores (2004) total estimates suggests that the income effects are on the order of 2.5 to 5.5 absolute percentage points, or about 25-33% of the non-income effects.

Another way to estimate income effects is to utilize the coefficient estimate on the variable of the natural log of total household consumption *lnConsumption*. In 2001 the difference in the average *lnConsumption* between treatment and control was 0.35, while it was

0.24 in 2002. With a coefficient estimate of .09 on total household consumption, these differences would suggest that a transfer the size of RPS would increase schooling on the order of two or three percentage points.[8] This magnitude is less than but consistent in magnitude with the difference between our estimated non-income effects and the Maluccio and Flores (2004) total effects.

We now examine the combined impacts of power and RPS on school enrollment through the interaction of the non-income treatment impact measure (*Treat) and the* power ratio (*rFPSY)*. This interaction term and its square are not statistically significant, which suggests that the impacts of RPS treatment do not vary depending on the power of the female head of household. In the model with both boys and girls, the linear interaction term is negative and close to significant at the 10% level (p = .107), but this result may stem from preexisting differences in enrollment by the power measure, that diminish the potential for impact.

We now turn to the estimation of the impacts of power, RPS, and income on three types of expenditures (education, food, and milk). These results are presented in Table 4. Consistent with the enrollment results, we find the female power as measured by rFPSY for most households has a positive relationship with spending on education, with a negative quadratic effect. Similar to the enrollment results, the inflection point of more powerful women occurs at a rFPSY of about four (about 4% of the sample). Unlike education, spending on food or milk in particular do not show statistically significant relationships with power.

An examination of the non-income impacts of RPS as measured by the variable *Treat* shows significant positive impacts on spending for milk and food, but not education. The non-income impacts on milk and food are substantial. The estimated non-income impact on milk

expenditures per capita was $C 73, which is more than double the average baseline consumption. Although not as dramatic, the estimated impact of RPS on food consumption per capita of $C 437 that is nearly 15% increase of baseline consumption. For all three types of expenditures (food, schooling and milk) in terms of both total spending, the sign of the coefficient on *lnConsumption* is positive and significant for all three variables, suggesting that as households' total consumption levels increase so does spending on these items.

Finally, we examine the interactive effects of female power and RPS on the three types of expenditures. For both education and food the interactions of power and RPS in per capita expenditures does not show a statistically significant coefficient. In terms of milk expenditures the effects are at first negative but then turn positive, with non-linear effects working but in the opposite fashion as above. The inflection point is at 2.8 rFPSY; once women's power surpasses that point, more women's power actually increases impacts.

In our empirical analysis of RPS, we have examined three main areas. First, we found that generally *ex ante* more female power leads to higher school enrollment and greater spending on education. However, consistent with an emerging literature, we found that for households with extremely powerful women more female power can begin to reduce schooling. Second, we found non-income effects of RPS to be extremely important in terms of school enrollment, which is not surprising given the conditionality of the program. In addition, we found non-income effects on spending on both food and milk per capita. Although the RPS program encourages spending on these items, it was not required, which suggests that other non-income effects besides conditionality had an impact. Two likely possibilities are the targeting to women or the accompanying nutrition education programs. Finally, we examine the interaction of power and

the impact of RPS. We do not find evidence of decreased impact of RPS when women are less powerful. In fact in terms of spending on milk we find households with less powerful females see greater impacts, with the exception of when women are extremely powerful. Overall, these results support the hypothesis that the goals of schooling and nutrition can be improved by directing funds to women and requiring conditionality of school enrollment.

V. Conclusions

A large literature on intra-household bargaining suggests a positive relationship between women's power and the amount of resources devoted to children. In this paper we utilize a power measure based on the ratio of male and female head years of schooling to study the impacts of a conditional cash transfer program (RPS) in Nicaragua. This measure is generally consistent with the excepted positive relationship between women's power and child schooling. However, as suggested by Basu's (2006) model, we also find that in some cases very high levels of women's power can begin to have negative effects on schooling.

By targeting transfers to women, RPS and other conditional cash transfer programs have the goal of increasing their potential to spend money on children's schooling and other goods such as food, which can improve their human capital. To test RPS' impact on women and children, this paper estimated its effects on two key household decisions: child schooling and household expenditures on food and education. The results of these estimations provide supporting evidence of the effectiveness of RPS transfers in improving the allocation of household resources toward women and children. It appears likely that these effects are driven mostly by the non-income effects of conditionality and targeting to women. In particular, we find

that the non-income effects of the program were responsible for the majority of the nearly 20% increases in school enrollment.

When we separate the enrollment regressions by gender of the child, we find that the mother's education *ex-ante* of the program always had a positive impact boys education. However, the results for girls are consistent with the non-linearity suggested by Basu (2006), in that when female power passes a certain threshold female power and schooling begin to have a negative relationship. 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 non-monotonic relationship for girls but not boys suggests that when girls leave school that the percentage of the benefits received by the female head of household are larger in proportion to boys. This result is not surprising as these girls are likely to help out in the home relieving female heads of work.

The expenditure data further supports the effectiveness of targeting transfers to women, as RPS' non-income effects accounted for a more than doubling of milk expenditures and 15% increase in food expenditures. These results suggest that targeting transfers to women has been effective at increasing key welfare outcomes for all households, even those with more 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 transfer to women, more in-depth questions regarding how

households allocate their resources, or possibly experiments which provide targeted and non-targeted transfers, should be used in future program evaluations.

Table 1: Descriptive Statistics of Total Household Consumption and School Enrollment

	Parental Literacy	Baseline Control	Baseline Treatment	T-Stat#	DID-2001##	DID-2002###
Total Household Consumption	Total	23147	23623	-0.7	5850*	4280*
	Neither	22531	20713	1.9*	6844*	4554*
	Female Only	20141	24075	-2.7*	-2348	1139
	Male Only	21955	24524	-1.4	5960*	2358*
	Both	29113	26340	1.8	8521*	6792*
School Enrollment Ages 7-13	Total	77%	77%	0.04	17%*	11%*
	Neither	73%	71%	0.9	22%*	13%*
	Female Only	82%	86%	-0.8	15%*	3%
	Male Only	72%	75%	-0.5	18%*	20%*
	Both	87%	85%	0.5	8%*	6%

T-statistics is a comparison of baseline control and treatment
Difference-in-Difference is $(Treatment_{2001} - Control_{2001}) - (Treatment_{2000} - Control_{2000})$
Difference-in-Difference is $(Treatment_{2002} - Control_{2002}) - (Treatment_{2000} - Control_{2000})$
* significant difference at 5% level

Table 2: Descriptive Statistics of Consumption Per Capita (Food, Milk, and Schooling)

	Parental Literacy	Baseline Control	Baseline Treatment	T-Stat#	DID-2001##	DID-2002###
Per Capita Food Expenditures	Total	2801	2969	-1.2	618*	514*
	Neither	2602	2506	0.6	776*	410
	Female Only	2891	2610	0.7	446	765*
	Male Only	2830	2355	1.7	715	558
	Both	3011	3070	-0.3	966*	591*
Per Capita Milk Expenditures	Total	49	50	-0.2	48*	42*
	Neither	26	31	-0.5	41*	45*
	Female Only	89	89	0.6	88*	-5
	Male Only	68	50	0.1	97*	22*
	Both	59	63		3	68*
Per Capita School Expenditures	Total	56	65	0.7	2	43
	Neither	38	26	1.5	24	38*
	Female Only	55	36	1.5	22	38
	Male Only	41	51	1	30	64*
	Both	89	89	0.06	-20	54

T-statistics is a comparison of baseline control and treatment
Difference-in-Difference is $(Treatment_{2001} - Control_{2001}) - (Treatment_{2000} - Control_{2000})$
Difference-in-Difference is $(Treatment_{2002} - Control_{2002}) - (Treatment_{2000} - Control_{2000})$
* significant difference at 5% level

Table 3: Regression on School Enrollment: Impacts of Power and RPS

	All Children		Boys		Girls	
Definition	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
RPS (1) if treatment group	-0.008	0.017	0.034	0.025	**-0.054**	**0.024**
(1) if year 2001	**0.062**	**0.017**	**0.074**	**0.024**	**0.046**	**0.023**
(1) if year 2002	**0.076**	**0.017**	**0.092**	**0.024**	**0.055**	**0.023**
Relative Female Power by Schooling Years (rFPSY)	**0.040**	**0.013**	**0.037**	**0.020**	**0.040**	**0.017**
rFPSY^2	**-0.004**	**0.002**	-0.002	0.003	**-0.006**	**0.003**
(1) if RPS group and treatment year (Treat)	**0.155**	**0.030**	**0.141**	**0.045**	**0.173**	**0.041**
Treat*rFPSY	-0.041	0.026	-0.050	0.041	-0.032	0.033
Treat*rFPSY^2	0.005	0.005	0.005	0.008	0.006	0.006
Ln(household consumption)	**0.094**	**0.010**	**0.094**	**0.014**	**0.094**	**0.013**
Ln(household size)	-0.039	0.016	**-0.045**	**0.022**	-0.033	0.022
Constant Term	-0.115	0.095	-0.148	0.140	-0.078	0.129
	R-Squared = .09		R-Squared = .10		R-Squared = .09	
	N =4593		N = 2337		N = 2256	

Bold figures are significant at the 5% level

Table 4: Regression on Per Capita Expenditures by Category: Impacts of Power and RPS

	Food Per Capita		Education Per Capita		Milk Per Capita	
Definition	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
RPS (1) if treatment group	55.4	106.1	1.3	12.0	1.5	9.4
(1) if year 2001	**-369.7**	**99.5**	**33.3**	**11.2**	**20.3**	**9.2**
(1) if year 2002	**-424.3**	**97.1**	**55.7**	**11.0**	3.7	9.2
Relative Female Power by Schooling Years (rFPSY)	-44.0	81.6	**19.9**	**9.2**	1.7	7.7
rFPSY^2	5.4	12.8	***-2.7***	***1.4***	0.8	1.3
(1) if RPS group and treatment year (Treat)	**437.9**	**176.1**	-0.8	19.9	**72.9**	**16.7**
Treat*rFPSY	-232.2	144.7	-0.4	16.4	**-45.1**	**14.1**
Treat*rFPSY^2	23.3	24.5	0.3	2.8	**8.0**	**2.5**
Ln(household consumption)	**1919.1**	**51.6**	**119.4**	**5.8**	**27.1**	**4.7**
Constant Term	**-15814.2**	**511.5**	**-1122.2**	**57.8**	**-217.8**	**46.8**
	R-Squared = .37		R-Squared = .16		R-Squared = .03	
	N =2550		N =2550		N =2550	

Bold figures are significant at the 5% level

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Endnotes:

[1] Unfortunately we are unable to provide a comparison to their study as wage data are not available in the RPS sample.

[2] Additionally, we do not have individual or community wage data for the RPS sample.

[3] An argument could be made to estimate the impact of RPS in each of the two treatment years separately. We find similar impacts in 2001 and 2002. For ease of interpretation we combine both years into a single measure of the impact of the treatment in a treatment year since, the results do not change substantially by combining both years of data into one measure.

[4] See Maluccio and Flores (2004) for further information on the program design. Additionally, Maluccio and Flores (2004) show that sample attrition rates were similar in both control and treatment communities.

[5] 95% of households were eligible to participate (see Maluccio and Flores, 2004). Program participation does not appear to have been impacted by adult literacy, household income, or marital status.

[6] (C$) is September 2000, Nicaraguan códdobas, $1 U.S. is about C$12.85

[7] A previous version of the paper used relative literacy in place of the rFPSY measure for power. The key relationships are not impacted by the choice of the power measure. The results are available upon request.

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