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by

Guy Stecklov, Paul Winters, Jessica Todd and Ferdinando Regalia

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Demographic Externalities from Poverty Programs in Developing Countries: Experimental Evidence from Latin America[*]

Guy Stecklov, Paul Winters, Jessica Todd and Ferdinando Regalia[**]

Abstract

Conditional cash transfer programs have been shown to be effective development strategies for raising human capital investments in children in many LDCs. In this paper, we use experimental data from cash transfer programs in three Latin America countries to assess the potential, unintended impact of conditional cash transfers programs on childbearing. Because cash transfer programs both affect household resource levels as well as possibly shape parental preferences for quality versus quantity of children, they may prove to have unintended demographic externalities. Our findings show that the program in Honduras, which may have inadvertently been designed to create incentives to have children, may have in fact raised fertility by somewhere between 2-4 percentage points – a non-negligible impact in a country where fertility is relatively high. In the two other countries where the programs did not include the same unintentional incentives, Mexico and Nicaragua, we found no net impact of the programs on fertility. Our analysis also explored the potential mechanisms through which fertility in Honduras may have risen and we find that marriage rates may have increased. Furthermore, there is some indication in the other two countries that contraceptive use rose but this might be simply to counteract the impact of reduced spousal separation – another possible unintentional impact of the poverty programs.

Key words: fertility, cash transfers, poverty programs, impact evaluation

JEL classification: J13, O22

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[**] Guy Stecklov, Department of Sociology and Anthropology, Hebrew University, Israel. Paul Winters and Jessica Todd, Department of Economics, American University. Ferdinando Regalia, Inter-American Development Bank. Contact: Paul Winters, Department of Economics, American University, 4400 Massachusetts Avenue, NW, Washington, DC 20016-8029. Email winters@american.edu.

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1. Introduction

Few demographic questions have generated so much debate over so long a period as whether or not transfers to poor parents increase or decrease childbearing. Malthus provided an elegant and *dismal* assessment that increased support to the poor would only exacerbate their poverty by allowing them to marry earlier and hence bear more children. The most recent evidence suggests Malthus may have been right at least in his assessment of birth rates in nineteenth century England (Boyer 1989). This debate continues to hold the interest of researchers but in a different context; namely in the examination of government policies that explicitly or implicitly affect childbearing behavior. Researchers have shown that providing direct incentives to bear children are not entirely effective in that they only have a minimal impact on fertility rates (Gauthier and Hatzius 1997). Furthermore, evaluations of welfare policies that provide payments to young mothers find that there is no significant impact of payment levels on the childbearing decisions of recipients (Acs 1996). However, others have shown that government policies such as personal tax exemptions for dependents are implicitly pronatalist and lead to a positive impact on aggregate birth decisions (Whittington, Alm and Peters 1990).

This debate has generally focused on developed countries where fertility rates have already dropped and not on high fertility developing countries in the midst of a transition to lower fertility rates. That is – outside the case of China where very strong "incentives" were shown to be very effective in reducing childbearing – there has been little evidence on how transfers might affect reproductive behavior. In this paper, using data from conditional cash transfer (CCT) programs in Mexico, Honduras and Nicaragua, we seek to shed light on the relationship between conditional cash transfers and childbearing and we hope to also provide fresh insight into the economic determinants of childbearing in settings where large families are the norm.

For developing countries, the challenge of identifying the impact of transfers on the childbearing of poor households is just as great, or even greater, than in developed countries: how does one obtain a valid estimate of the effect of transfers on childbearing? In this paper, we make use of three experimentally designed programs – the Education, Health and Nutrition Program (PROGRESA) of Mexico, the Family Assistance Program (PRAF) in Honduras and the Social Protection Network (RPS) in Nicaragua – to evaluate how transfers affect the childbearing decisions of the poor. In each case, communities were randomly assigned to treatment and control groups. Households in the treatment group received transfers under the condition that their children enroll in and attend school and that family members obtain health care. The experimental design enables us to provide a true experimental test of the impact of transfers on childbearing. Furthermore, because data are collected over time, we are able to control for pre-existing differences across households and communities that may exist despite the randomization prior to receipt of transfers.

The objective of this paper is then to examine the impact of PROGRESA, PRAF and RPS on the fertility decision of women in beneficiary households in order to provide insights into the debate of how support to the poor influence childbearing. Towards this end, section 2 details how PROGRESA, PRAF and RPS operated, discusses the connection between these programs and childbearing and describes fertility behavior in Mexico, Honduras and Nicaragua. Using the details of the program as the basis, section 3 discusses the theoretical relationship between this type of cash transfer program and the fertility decision. The experimental nature of the data collection and a description of the data are presented in section 4. Section 5 then discusses the empirical approach which makes use of the experimental nature of the data. The results of the analysis of the data are presented in Section 6 and conclusions and policy implications are discussed in the final section.

2. Fertility and the design of conditional cash transfer programs

CCT programs are a class of anti-poverty programs that seek not just to reduce poverty but to invest in the long-term human capital development of the children of the poor. The PROGRESA program in Mexico was the first of its kind in Latin America and the one upon which other programs were modeled including PRAF and RPS. The programs tend to focus on chronically poor rural households using a variety of targeting methods. For example, selection for inclusion in PROGRESA was based on a combination of geographical and household proxy means tests. First, potential recipient communities were identified as poor based on an index of marginality developed from the national population census. To select beneficiaries within marginal communities, a proxy means test was calculated for each household using discriminant analysis on data collected from a community-level census and households above the cut-off point were selected as beneficiaries. PRAF and RPS followed similar procedures first identifying poor communities and then identifying eligible households. Within those eligible households, it was the primary female that was designated as the beneficiary and who received transfer payments unless no such female was in the household.

CCT programs have two principal components: 1) a health/nutrition component, and 2) an education component. In some cases, these components include both demand-side and supply-side interventions although the focus and majority of funding has been on the demand-side. For each component, the demand-side intervention involves a payment to the beneficiary provided certain conditions are met. For the health/nutrition component, one key condition is regular health check-ups for all family members with more frequent check-ups for infants and young children as well as pregnant and lactating women. Another condition on the health transfer is attendance by the beneficiary at public health/nutrition lectures that cover a range of topics including family planning. For the education transfer, the beneficiary households are required to have all eligible children enroll in school and attain a specified attendance rate. The motivation for the education transfer was to reduce dropouts from school by lowering the costs of education for the poor. Failure to meet the conditions of the program should lead to the beneficiary household being expelled from the program. The value of transfers to recipient households can be substantial. In the case of PROGRESA, in 1999 the value of transfers was estimated to be approximately 20% of the mean value of consumption (Skoufias 2005). For RPS, a similar estimate puts the amount at 21% (Maluccio and Flores 2004). Estimates for PRAF show much lower shares but this may partially reflect differences in the consumption data.[i]

On the demand-side, the principal difference between the three programs we analyzed relate to eligibility and whether the household roster was closed or open.[ii] The level of eligibility depends both on being declared eligible by the targeting process and the composition of the household. The roster being open or closed refers to whether households could become eligible for a component of the program by changing household composition (i.e. by having an additional child).

While the three programs are quite similar in their design, objectives and conditions, there are a few key differences that can lead to significant differences in impacts. In the case of RPS and PROGRESA, the health/nutrition transfers are lump sum amounts and are unaffected by household composition. Additionally, PROGRESA cannot be increased by the addition of eligible or targeted individuals, at least during the first three years of the program. However, although RPS only provided a lump sum transfer to households, the roster was left open to some households, and in particular there appears to have been an incentive for households without children to have children and receive benefits. In PRAF, the amount of the health/nutrition voucher was determined by the number of children under age 3 and pregnant women per household – up to a maximum of two per household – and the roster was open to women in treatment communities. While the addition of targeted individuals into a household increases the conditions imposed on the household, the amount received also increases for the first and second person. Therefore, the fact that the transfer amount can be increased by the addition of a pregnant woman and/or new child in PRAF creates an incentive for households/women to bear more children, especially women in households with low income. To summarize, it appears that through the health/nutrition transfer PRAF provided the greatest incentive for increased childbearing, followed by RPS and then PROGRESA.

For the education transfers all three programs have caps on total transfer provided, with PRAF capping the number of eligible children, PROGRESA the total payment per month and RPS having a flat rate for everything but school supplies. At least during the initial two-year period of each program, the education roster was fixed and could not be expanded by having more children. Whether the education transfer creates an incentive to have more children for long-run benefits depends partly on whether beneficiaries perceive that the program will be in place for a longer period and the roster will be updated.

On the supply side, although PRAF was designed to include additional payments to the suppliers of health and education services, these were not implemented in the first two years and are thus irrelevant for this study. For RPS, during the scheduled preventive healthcare appointments for children under age 5, children received vaccinations and were provided with antiparasites, vitamins, and iron supplements. Healthcare services were provided free of charge to beneficiary households by private providers financed by the program. On the education supply side, eligible households were given a US$5 per child per year transfer that was delivered to teachers or to parents' school committees to increase resources for the school. For PROGRESA, similar health care benefits in terms of vaccinations and nutritional supplements to children and pregnant women were provided. Additionally, health care service providers were given additional supplies as necessary and extra training for doctors and nurses in program areas. For education, there were also attempts to improve materials supplied to schools.

The description of the CCT programs show that while they included some similarities there are crucial differences that may influence fertility outcomes. There are also substantial differences in terms of existing reproductive patterns across the countries: fertility in Honduras and Nicaragua is high especially when compared to Mexico. According to the most recently available data from the United Nations, the total fertility rate (TFR) for both Honduras and Nicaragua is around 3.7 births per women which is much higher than the rate for Mexico of 2.5 births per women. Note, however, that the TFR has dropped in both Nicaragua and Honduras over the last decade with Nicaragua reporting a fall from 4.6 in 1993 to the current 3.7 and Honduras reporting a drop from 5.4 in 1990 to the current level (UNFPA 2004). This data suggests that Honduras and Nicaragua are in the transition towards lower fertility levels but continue to show moderately high TFR levels. Mexico on the other hand has already gone through much of this transition and is approaching replacement levels. Given the cash transfer programs evaluated in this paper target rural poor households, we find that fertility rates are actually substantially higher in the target population.

3. Conditional cash transfers and the economics of fertility

To consider how CCT programs may influence fertility, it is necessary to review the theoretical literature on fertility decisionmaking and pinpoint how CCT programs influence this process. We do this by first considering a household's desired number of children – the demand side – and then take into account other factors which determine the supply of children.

Demand for children

Through the conditions they impose, CCT programs seek to improve the "quality" of children as measured by their human capital. Economic theory and empirical evidence suggest that there is a special relationship between the quality and quantity of children and, in particular, that quantity and quality are substitutes (see Schultz 1997). If this is the case, an increase in the quality of children that results from these transfer programs is likely to reduce fertility. However, the shift from high quantity-low quality to low quantity-high quality that occurs as income rises with development occurs over generations and it is unclear whether such changes may occur over the relatively short periods over which these programs have operated. To examine how cash transfers may influence fertility, we turn to examining a model of fertility, based on Becker (1960) and Becker and Lewis (1973), that explores this quantity-quality trade-off and how changes in income and relative prices of quality and quantity influence the fertility decision.

We begin by considering a lump sum cash transfer that is not conditioned on household behavior; that is, a transfer that only increases income. Suppose a household gets utility from the number of children *n*, their quality *q* (which is assumed to be the same for all children),[iii] and the rate of consumption of a composite good *y*, so that the utility function is as follows:

$$U = U(n, q, y) \tag{1}$$

The household budget constraint can be expressed as follows:

$$I = \pi nq + p_y y \tag{2}$$

where *I* is full income, p_y is the price of the composite commodity *y*, and π is a price index of goods purchased that are related to "child services" provided to the household which is measured by *nq*. Solving the model leads to the following result:

$$\alpha(\varepsilon_n + \varepsilon_q) + (1 - \alpha)\varepsilon_y = 1 \tag{3}$$

where α is the share of family income spent on children ($0 < \alpha < 1$) and ε_i for *i*=*n, q*, and *y* are, respectively, the income elasticities of the number of children, quality of children and the composite good (Hotz, Klerman and Willis 1997). If children are normal goods, by definition the sum of the income elasticities of quantity and quality must be positive. However, the specification of the model leads to the possibility that the income elasticity of quantity could be negative provided the elasticity with respect to quality is sufficiently positive (so that the sum remains positive). This suggests that under certain circumstances an increase in income will result in a reduction in the number of children and a substantial increase in the quality of children. Note that it is the non-linearity of the budget constraint (2) that produces the ambiguity with respect to the effect of income on the demand for children. Under a standard linear budget constraint where prices are fixed and thus not dependent on quality, an increase in income should lead to an increase, or at least not a decrease, in the number of children demanded if children are normal goods (Razin and Sadka 1995). The nonlinearity in the budget constraint provides for the possibility that income increases may lead to an increase in quality and a decrease in quantity.

Thus, it is entirely possible that an unconditional lump sum transfer to households will lead to a decrease in the demand for children and an increase in the demand for quality children. Whether this occurs depends largely on the preferences of the parents, which of course are influenced by the social context in which they live.

Now consider a transfer which is linked to the behavior of households and is conditioned on the health and schooling outcomes of children. Because of the link to household behavior, the transfer in effect reduces the price of children. To explore the effects of this transfer on the behavior of households, following Becker and Lewis (1973), consider the following generalized budget constraint:

$$I = \pi_n n + \pi_q q + \pi nq + p_y y \qquad (4)$$

where π_n is the 'fixed' price associated with the number of children that is independent of quality, π_q is the 'fixed' price associated with the quality of children that is independent of quantity. To understand the relationship between quantity and quality more fully note that the shadow prices for *n* and *q*, p_n and p_q respectively, can be expressed as follows:

$$p_n = \pi_n + \pi q \text{ and } p_q = \pi_q + \pi n . \qquad (5)$$

The shadow prices for *n* and *q* therefore contain a fixed component and a component that depends on the value of the other commodity. The fixed component for *n* could include the cost of contraceptives which are independent of quality. For *q*, the fixed component could include the cost of improving household public goods, such as the mother's knowledge of hygiene and hygienic practices, that generally improve the quality of children in the household. The price component that includes π indicates, for the price of *n*, that the higher the quality of children the greater the shadow price of *n* and, correspondingly, for the price of *q*, the greater the number of children the higher the shadow price of *q*. Becker and Willis (1973) assume that the fixed component is generally more important for quantity than quality such that $\pi_n > \pi_q$.

The specific transfer mechanisms that PRAF and RPS use to promote human capital formation will decrease the shadow prices p_n and p_q through both components. Education of the mothers in health and hygiene is a family public good that may reduce π_q, payments and assistance during pregnancy may reduce π_n and conditions requiring health check-ups and school attendance clearly reduce π. Using the model, we want to examine how these changes in prices may affect fertility.

Given that cash transfers, at least in the case of PRAF, provide cash when pregnancy occurs, in our case we want to consider the effect of an exogenous decrease in π_n. Since this would decrease the shadow price of the quantity of children (p_n) relative to the shadow price of quality (p_q) and the shadow price of the composite good (p_y), *n* should increase. The rise in the number of children, however, will induce an increase in the shadow price of quality, which will induce a substitution effect away from quality. This means that not only does the reduction in the fixed price of quantity directly increase the demand for children, it also leads to a particularly strong pressure away from investment in quality which induces a greater increase in the demand for *n*. Similarly, an exogenous reduction of π_q will bring about a direct increase in quality demanded and an induced increase in quality leading to a relatively large increase in the demand for child quality.

The next step is to consider a general decrease in the prices associated with child services. To do this, we examine an equal percent decrease in π_q, π_n and π which can be treated as relative increase in p_y. A relative rise in p_y should initially induce equal percent increase in *n* and *q* if they were equally good substitutes for *y*. However, if as noted above $\pi_n > \pi_q$, then an equal percent increase in *n* and *q* will result in a greater increase in p_q relative to p_n and *n* will rise more than *q*. The rise in the demand for quantity is relatively larger than the rise in the demand for quality when a general decrease in the price of child services occurs.

To summarize, based on the model presented thus far, the following conclusions can be reached:

1. An unconditional transfer to poor households will lead to a relatively greater increase in the demand for quality children relative to the quantity demanded and under certain conditions may lead to a decrease in the quantity of children demanded – that is, a reduction in fertility.

2. The effects of a conditional transfer to poor households depends on the specifics of the program and the relative emphasis on conditions that are directly linked to purely the costs of childbearing (π_n), pure costs of quality (π_q) or general costs of child services (π). If equally weighted, a reduction in the costs of children is likely to increase quantity more than quality and thus lead to higher fertility. An emphasis on reducing the direct costs of child bearing (π_n) is likely to induce a greater increase in fertility while an emphasis on pure costs of quality (π_q) is likely to reduce the overall effect of the program on fertility.

Supply of children

At first glance the above standard economic model of fertility would appear sufficient to understand the impact of CCT programs on fertility since they are likely to impact fertility principally through an income and substitution effect. However, the analysis is more complex since in addition to transfers the programs included interventions, such as family planning training and information on health and nutrition (including breastfeeding), that may influence the biological supply of children. Furthermore, the program may alter other economic incentives that could indirectly influence fertility. We turn to considering how these factors may influence the fertility outcome by making the number of children (*n*) endogenous.

Following the model of fertility control presented by Rosenzwieg and Schultz (1985), assume that fertility is a random variable that can be reduced by the use of resources to control fertility as follows:

$$n = \mu + \varepsilon - \beta z \qquad (6)$$

where μ is couple-specific fecundity or number of births without any control, ε is an independently distributed random component recognizing that the biological capacity to bear children is stochastic and z is a measure of resources used to control births and β a parameter that reflects the effectiveness of such control. The household budget constraint would then need to be augmented to include the costs of such control; that is, $p_z z$ where p_z is the price of control. CCT programs, by providing information on family planning, may reduce the cost of such control by lowering the transactions cost associated with obtaining the control (lowering p_z) or by improving the effectiveness of control (increasing β). It can be shown that a decrease in the price of family planning or an increase in the effectiveness of control will lead to a decrease in fertility.

The program may also influence the supply of children through influencing couple-specific fecundity, μ. Along with providing information on family planning, the public lectures included as part of the CCT programs may influence the breastfeeding practices of participants by providing information to parents of the health benefits of breastfeeding. Breastfeeding expands the duration of postpartum infecundability and has been shown to influence fertility (Bongaarts 1982). This, in effect, reduces fecundity (μ) and will lead to lower fertility rates. Another mechanism by which fecundity may be altered is through changes in migration patterns. Evidence suggests that PROGRESA reduces rural migration particularly to international destinations (omitted to avoid self-identification). One of the proximate determinants of fertility is fecundability or frequency of intercourse (Bongaarts 1982). By reducing migration patterns, a cash transfer program may increase fecundability and thus fertility. Of course with proper information about the effect of breastfeeding on fertility and on the frequency of intercourse on fertility, households may respond by altering the use of contraceptives thus influencing fertility outcomes. It may be the case however, particularly for younger couples, that information on their couple-specific fertility is limited. Rosenzweig and Schultz (1985) show that couples alter their contraceptive methods over their life cycle based on their fertility supply experience. For those younger couples in which the male has migrated and returned or has decided against migration based on the program, this information may be limited and thus the effect on the supply of children may be stronger.

Theory and the CCT programs

For each transfer program, it is very difficult to disentangle how conditions will influence the pattern of child costs. Preliminary evidence suggests the programs have had some effect on the investment in quality (IFPRI 2003; Maluccio and Flores 2004; Schultz 2000). It could be the case that PRAF, RPS and PROGRESA all increase fertility rates as well. However, given the description of the design of the programs provided in the previous section, our expectation is that PRAF would have the greatest positive incentive for increasing fertility, followed by RPS and then PROGRESA. Furthermore, if programs induce sufficient quality gains to induce lower fertility rates, the expectation is that the reduction would be greatest for PROGRESA given the emphasis on family planning and the transition that has occurred to lower fertility rates, followed by RPS and then PRAF.

4. The design of the data collection and description

We use panel data collected as part of a randomized impact evaluation strategy employed by all three programs. Each program first identified a set of communities that were eligible for the program and then randomly assigned eligible communities into control and treatment groups. In the case of PROGRESA, 302 communities were randomly assigned to treatment and 186 were randomly assigned into the control communities. For PRAF, 40 eligible communities were assigned to treatment and 30 into control. In the case of RPS, the random assignment divided the communities into 21 treatment and 21 control communities.

Prior to treatment, household surveys were conducted with households randomly selected from the control and treatment communities for inclusion in the sample. Given the random assignment, the expectation is that household characteristics should not significantly vary between the control and treatment household. After treatment occurred, follow-up surveys were conducted. To date, a total of two rounds of survey data have been collected for PRAF (2000 and 2002), three rounds for RPS (2000 and 2001, and 2002), and six rounds for PROGRESA (two each in 1998, 1999 and 2000) along with a baseline census in 1997. To maintain consistency in our analysis, we consider the impact of the program for approximately two years after implementation using the surveys as appropriate. For PRAF, the panel includes 5,096 households and 28,931 individuals, for RPS there is data on 8,918 individuals in 1,434 households and for PROGRESA 124,881 individuals in 20,496 households.[iv]

Measuring the impact of a program on childbearing is relatively more complicated than estimating program impact on other indicators. Even if the demand for children changes immediately upon receipt of the transfers, there is a likely waiting period before conception since it is a probabilistic event. Studies show that less than half of couples are able to get pregnant within 3 months and the mean is around 5-6 months (Juul et al 1999). Thus, the chances of observing a birth roughly nine months after the initial entry into the program is rather small. And the standard measure of fertility which is whether a woman had a birth over a certain period may therefore be too limiting. Our solution is to use two measures of fertility. The first is whether or not a woman has given birth in the past 12 months, P(B) prior to the follow-up survey. The second is whether or not a woman has given birth in the past 12 months or is currently at least three months pregnant, P(BCP).

The sample used for this analysis is all women between 12 and 47 years old in the baseline (1997 for PROGRESA and 2000 for PRAF and RPS), who would then be between 14 and 49 years old in the follow-up survey (1999 for PROGRESA and 2002 for PRAF and RPS). For each of the programs the survey instruments included data on births and pregnancies over the period in question. For PRAF there were 6,456 women, for RPS we have 2,409 women and for PROGRESA we observe 8,817 women.

Along with the fertility data, information on the characteristics of the women and the household are available in the baseline surveys. Furthermore, there is also some data on the proximate determinants of fertility in the PROGRESA data. From the PROGRESA fertility module it is possible to determine whether or not the woman used modern contraception during each of these 'conception' periods. In addition, there was limited information on the duration of breastfeeding (in months) of children born during specific periods. We use the breastfeeding data to provide a sense of whether breastfeeding patterns changed in treatment and control communities.

5. Approach to examining the fertility decision

The randomized and panel nature of the data used in this evaluation provides a unique opportunity to use experimental methods to assess the impact of changing childbearing incentives on reproductive behavior. Our reproductive outcomes, P(B) and P(BCP), are measured both post-treatment and pre-treatment for both the treatment and control group. This allows the option to use either a single post-treatment evaluation (first difference since it compares the difference between control and treatment) or a before-after comparison of control and treatment (difference-in-difference or double difference since it compares the difference between control and treatment as well as before and after).

Our main specification is the difference-in-difference (DD) model, whose major advantage is that it allows us to control for initial time-invariant unobservable differences between the treatment and control group prior to the onset of the experiment in the event that randomization is imperfect. In contrast, the first difference (FD) design, which we also employ, relies entirely on randomized selection of the treatment and control groups and therefore assumes control and treatment groups necessarily have equal levels of the outcome variable prior to treatment. When this assumption is mistaken, the estimation of the treatment effect can be inaccurate. Given that in our case the experimental design randomly assigned municipalities/comarcas rather than individual households to treatment and control, it is critical to be cognizant of potential problems in the randomization of the data.

Despite the advantages of DD with experimentally designed data, it is important to note that this method also makes an assumption which if incorrect can bias this method and perhaps make it less reliable than the FD approach. The assumption is that there is no possibility of 'contamination' prior to treatment. If, for example, groups know that they are to be selected for treatment prior to being treated, they might anticipate the impacts of the program which could affect the DD estimates. Thus, it is important to be clear about how the experiment was designed and operationalized and explore both FD and DD approaches.

In the FD method, a standard regression model can include a dummy variable for treatment which captures both the magnitude of the impact of treatment as well as the statistical significance. This model can be specified as follows:

$$f_i = \beta_0 + \beta_1 P_i + \varepsilon_i \qquad (6)$$

where f_i is 0 is dummy variable indicating whether woman *i* had a birth and/or was pregnant during the period in question, P_i is an indicator of program participation by household *i*, and ε_i is an error term. In the FD specification the coefficient β_1 estimates the magnitude of the program impact on fertility and is used to test for statistical significance.

In the DD estimator, dummy variables are included for time, treatment and the product of time and treatment as follows:

$$f_i = \beta_0 + \beta_1 t + \beta_2 P_i + \beta_3 t * P_i + \varepsilon_i \qquad (7)$$

where *t* refers to whether the period is pre treatment (t=0) or post-treatment (t=1). The coefficient on the time variable (β_1) captures changes that occur over time that are independent of the program, the coefficient on the treatment variable (β_2) captures the initial difference between the treatment and control women and the coefficient on the interaction of time and treatment (β_3) provides an estimate of the impact of the program on fertility.

The outcome variable of interest that is used as a measure of fertility is in both cases dichotomous, leading us to use probit models. Our probit results are presented in terms of the marginal effects. The DD estimator is typically used with linear models, making it rather straightforward to interpret the results in terms of how treatment affects the change in the outcomes variables over time. The probit estimation of the DD model, however, is intended to measure how the treatment changes the change in the probability of the outcome across time. The use of a non-linear DD model introduces the possibility that our estimates might be unduly affected by the underlying non-linearity of the probit estimator. This possibility is tested using a heteroskedasticity-corrected linear probability model whose results are compared to the DD probit specification (available from authors).

We present p-values and follow convention in describing the 5% level as significant and under 10% as marginally significant. Nonetheless, our interest is in the magnitude of the effects and we believe the experimental design provides us with greater leverage than is generally available to claim that estimated effects are accurate representations of the true effects, unbiased by many of the potential confounders that are present in most econometric studies. Finally, we note that our standard error estimates are adjusted for correlation existing within municipalities or comarcas, the sampling unit of the survey, in order to avoid downward biased standard errors.

6. Analysis and estimation results

To begin the analysis, Table 1 provides an overview of the characteristics of the women used for the analysis as well as characteristics of the women’s household. The summary statistics provide support for the random allocation of communities to treatment and control groups. Most variables in the three datasets are statistically indistinguishable between treatment and control groups – a promising beginning. This effect is measured after controlling for autocorrelation of households within communities. The reported p-values shown in Table 1 are based on the significance following probit regressions of each of the variables with a single explanatory variable: whether or not the household is part of a treatment group. The only exceptions are some age structure variables which show slight differences which never exceed two percent in the PRAF and PROGRESA data and which are significant in three cases. Furthermore, comparison of the baseline childbearing measures used in the analysis suggests that there are no significant pre-treatment differences. Thus, the data provide support for the random allocation in these data and thus the use of the experimental design.

The foremost question we wish to address is how the CCT programs impact childbearing in each of the three countries. The results with respect to all four of the models are presented in Tables 2 and 3 – that is P(B) and P(BCP) using both FD and DD specifications. Beginning with the FD specifications for the Honduran data (see Table 2), we find that women in treatment communities in 2002 report probabilities of birth that are higher by 3.1 percentage points and highly significant (p=0.000). This estimate provides a good indication that PRAF increases childbearing and that the effect is non-trivial. The impact appears even larger when we examine the effect of PRAF on P(BCP) which shows a rise of 4.8 percentage points (p=0.000). In contrast to these clear and significant effects of PRAF in the FD specification, the effects of both RPS and PROGRESA are very small and insignificant. Thus, while we find strong evidence for PRAF increasing childbearing, we find no parallel effect resulting from RPS or PROGRESA.

Table 3 presents the estimated effects in the case of the DD model, which includes three separately estimated coefficients. The first of these coefficients is the treatment coefficient, which measures the difference in fertility between treatment and control communities prior to treatment. In Honduras, we find that the treatment coefficient in both the P(B) and P(BCP) models is positive but insignificant. Both coefficients from the RPS sample are negative and insignificant and both coefficients from PROGRESA are positive but insignificant. Thus, we find no indication of any significant differences in fertility between control and treatment groups prior to the initiation of the programs.

The *Post* variable captures the change in the P(B) or P(BCP) over time. Table 3 shows that between the 1999-2000 and 2001-2002 period, the probability of a birth in a given year in the Honduran sample declined by an average of 4.6 percentage points (p=0.000) and the probability of birth or current pregnancy declined by 6.7 percentage points (p=0.000). Both dependent variables show a strong decline, although the decline is larger with the P(BCP) outcome. We estimated a decline in TFR levels for our PRAF sample of about one child, so this large reduction in probabilities is not surprising. The PROGRESA data also point to a substantial decline in childbearing over the period of the experiment with the results suggesting a 3.8 percentage point decline in P(B) and a 7.3 percentage point decline in P(BCP) – both of these being highly significant (p=0.000). However, in contrast to the observed decline over time in Honduras and Mexico, the Nicaraguan data show little clear patterns over the period of observation. Between 2000 and 2002, P(B) is seen to rise insignificantly by 0.2 percentage points and P(BCP) is seen to decline insignificantly by 1.6 percentage points.

The test of whether and by how much PRAF, RPS or PROGRESA affected fertility change over time is represented by the interaction between treatment and time (*Treatment x Post*). For the

Honduran sample, the probability of childbearing is declining for all women during this period, but the decline appears substantially smaller for women living in treatment communities. According to Table 3, the P(B) is higher by 2.1 percentage points (p=0.053) and P(BCP) is higher by 4.3 percentage points (p=0.001) for women in treatment communities as opposed to the control group. This effect is highly significant in the case of P(BCP) and right at the line between marginal and standard significance for P(B). Moreover, these estimates are consistent with the results noted in the FD models. The lower level of significance obtained for the P(B) effect is not particularly surprising. A variety of specifications that we tested generally showed this result to be consistently positive but not estimated with as much precision as the highly significant effect of PRAF on P(BCP). It may be that that attempting to capture program effects on births in such a short period is difficult and fraught with error due to random variation, and thus adding the extra pregnancy period is quite useful in capturing the causal impact. Overall, PRAF appears to be associated with a rise of between 2-4 percentage points in the probability of birth. In contrast, the effects of the RPS and PROGRESA programs on fertility are weak and appear to be essentially zero. The estimated effect of RPS on the P(B) in the past year is 0.2 percentage points (p=0.925) and the effect of RPS on P(BCP) is slightly larger (1.3 percentage points) but also insignificant (p=0.479). While fertility appears to have fallen during the late 1990s in Mexico, the PROGRESA program appears to have had no distinct impact on fertility. Table 3 shows that P(B) is higher in treatment communities by 0.1 percentage points (p=0.939) and P(BCP) is lower by 0.3 percentage points (p=0.851).

The results consistently point to a strong effect of PRAF on fertility, particularly when we allow for a longer period to capture the reaction to the transfers (using P(BCP) rather than P(B)). The magnitude of the change is quite substantial. In the case of the P(B) for the DD specification, the magnitude of the coefficient would imply that more than 45% of the decline in the single year probability of childbearing that was experienced was offset by the PRAF program's impact on treatment communities. In the case of the P(BCP) outcome, almost 2/3 of the estimated decline was offset by the impact of PRAF on fertility. Cash transfers associated with PRAF, provided in an experimental evaluation, appear to have led to a substantial positive increase in the probabilities of childbearing. In the case of Honduras, fertility generally in decline during this period, so the impact of the program appears to be to have slowed down the decline. In both Nicaragua and Mexico, there are no signs of an effect of transfers on fertility outcomes. The difference in the results is not entirely surprising, as we had expected a stronger effect of transfers on fertility in the PRAF program. The difference might be explained by the fact that parents were not able to enter the RPS and PROGRESA programs after they had begun, suggesting that the program operated more like a direct income transfer which also subsidized quality. On the other hand, PRAF may have unintentionally included a loophole where parents could bear children and become transfer recipients or increase the amount they received.[v] The effect was thus not only to subsidize quality, but also to offer a direct incentive for more children as well.

Our findings suggest that the CCT programs may have had very different effects on fertility depending on the program structure, but our analysis so far has done little to explore the potential mechanisms through which the CCT programs may have operated. As we discussed earlier, the effect may be caused by a change in the demand or supply of children or a change in both. Our study of at least several of the potential pathways through which fertility may have been affected can help us to isolate the principle source of change in reproductive behavior.

We begin by examining fertility rates by age category to see whether changes in fertility are concentrated among certain age groups. The age-specific fertility rates for each of the data sets are calculated and presented in Figures 1-3. Figure 1 clearly shows the rapid decline in fertility rates at all ages in Honduras, although it is also clear that the major decline is concentrated in the older ages. This is not surprising given prior research that shows that most women, even in high fertility Latin American countries, focus on limiting their childbearing once they have reached the desired number, rather than on spacing as is common in sub-Saharan Africa (Westoff and Bankole 2000).

The RPS data in Figure 2 show a similar lack of change at the younger ages, where fertility levels are low in any case. There also appears to be considerably more variability in this graph, given the smaller number of cases. This is particularly clear with the older ages. We notice a rise in fertility, as opposed to a decline, focused primarily in ages 30 and above. Note, however, this rise may be do to limitations in the calculation of base year fertility since questions in the baseline survey were not as carefully constructed as in the later round.

The PROGRESA data in Figure 3 are clearer and show that age-specific rates are almost identical for treatment and control groups at each point in time. At the same time, there is a pronounced decline in fertility rates at all ages. Somewhat surprisingly, there is no clear indication in the figure that the fall is greater at the older ages, as observed in Honduras, and in fact the decline appears more pronounced at the younger age groups.

We also separately estimated the DD models for P(B) and P(BCP) for women in different age categories. While we tested various possible cutoffs, we ended up dividing women into three age groups: under 20, 20-29, and 30 and over. The results were somewhat sensitive to the specific ages we used as cut-offs, but we found that women in their 20s showed the greatest impact of the program with the estimates in Honduras and Nicaragua hovering near 5 percentage points and either marginally significant or nearly so. In some cases, women 30 and over showed a similar impact. Interestingly, we found no case where fertility rose for women under 20 years of age and in fact the effect tended to be negative but insignificant. In one case, Mexico, we found that PROGRESA lowered fertility for women under 20 by over 2 percentage points.

Demographic research has shown that almost all the variation in TFR levels across countries can be explained by 4 of the 7 proximate determinants of fertility (Bongaarts 1978). These include exposure to marriage, contraceptive use, postpartum ammenhorea, and abortion.[vi] Since none of the surveys was designed to evaluate this level of demographic change, we must make do with partial answers. Nonetheless, we can make considerable progress in certain directions. For example, we have no data in any of the surveys on abortion, which leads us to put aside this potential explanation. However, abortion is a sensitive topic in Latin America where church influences are strong and there is little reason to expect a large change in abortion practices due to any of the programs. There is, however, some data on the other three primary proximate determinants and they can be used to shed light on the role of these mechanisms on fertility change.

The first such mechanism is marriage. One straightforward analysis of marriage is to analyze whether or not the programs are associated with a change in the probability of being married over time (see DD specification in Table 4). The results indicate there is no difference in any of the countries in the probability of marriage, P(marriage), between treatment and control groups prior to the programs (*Treatment* variable). The table also shows that there has been a slight increase in the P(marriage) over time (*Post* variable) but this change is only significant in Nicaragua, where P(marriage) has risen by 4.2 percentage points (p=0.005). There is a marginally significant rise in P(marriage) in Honduras where the probability has risen by 1.1 percentage points and there is an insignificant rise of 0.3 percentage points in P(marriage) in Mexico. The real test is provided by the program interaction with time (*Treatment x Post*). This interaction is only significant (p=0.007) for the case of Honduras where the probability of marriage appears to have risen in treatment communities by 2.1 percentage points relative to control communities. This impressive rise is unmatched in the other two countries where the p-values are nowhere near significant and the estimated coefficients are far smaller. Taken at face value, the coefficient from PRAF provides a potentially good explanation for the observed rise in fertility. However, further support for the marriage explanation would come from testing whether we observe an effect of PRAF on women that are married prior to the onset of the program. If the increase in marriage is the main explanation, we would expect to find little or no remaining impact of PRAF on the childbearing of married women. Instead, both the coefficients in the P(B) and P(BCP) DD models get stronger, although the significance declines which is not unexpected given that the sample size also drops by almost one half (results available upon request). We now find that PRAF increases P(B) by 2.6 percentage points (p=0.212), which is definitely lower than before but that the effect of PRAF is now to increase P(BCP) by 5.7 percentage points (p=0.022). Substantively the impacts are at least as large, although they do not appear to be estimated as accurately as before. Nonetheless, this analysis would not appear to provide strong support to claim that the primary effect of PRAF on fertility is due to its marriage impact.

Although a change in the use of contraceptive methods could indicate a change in the demand for children, it would not be surprising if the CCT programs affected contraceptive use directly since the programs did require women to visit health clinics more frequently. Changes in contraceptive use therefore can not directly be attributed to changes in the demand for children, although they may be indicative. The effects of the programs on contraceptive use is possible to gauge only in Nicaragua and Mexico, although the analysis in Nicaragua is limited to FD specification because the contraceptive use question was only asked in the last round of data collection. Despite these obvious shortcomings, the results are interesting (see Table 5). In Nicaragua, a substantively large increase of 6.4 percentage points in the probability of using artificial contraceptive methods over the past 18 months is found, although the effect is only marginally significant (p=0.071). Also, because it is a FD model, it is not possible to verify whether initial contraceptive use levels differed substantially across treatment and control groups. However, given that the prior tests showed that the randomized allocation worked well, this effect is likely to be robust, although its magnitude might not be very accurately estimated. In Mexico, estimating a DD specification is possible and in that case the program appears to have increased the probability of contraceptive use by 2 percentage points (p=0.01). This effect is also substantial and significant. What is surprising is that childbearing did not decline due to treatment in either of these two countries. Thus, it would have been useful to have similar data for Honduras where we showed that PRAF raised fertility.

The rise in contraceptive use in Mexico associated with PROGRESA is interesting. One potential explanation is that this rise in contraceptive use is a response to the decline in international migration associated with the PROGRESA program [[omitted to avoid self-identification (2005)]]. Such a decline in migration could potentially raise fertility if it raises the exposure of women to pregnancy by reducing in the duration of spousal separations. Thus, one interpretation of these data is that women in PROGRESA are experiencing greater exposure to pregnancy due to lower migration levels but they are counteracting this effect by raising their level of contraceptive use. The cost of raising contraceptive use in this environment may be particularly inexpensive given the increased exposure to reproductive health services.

The only country with useful breastfeeding data is Mexico. When we include all births occurring after May 1997, the average – though censored – duration of breastfeeding is an identical 13 months for both treatment and control communities. While a better analysis would profit from using data collected at a later period of time as well as using hazard analysis to correct for censoring in the data, the simple analysis provides no evidence to suggest that the duration of breastfeeding is significantly changed for treatment communities relative to women in the control communities.

Taken together the results indicate that only PRAF had a significant impact on fertility – part of which may be related to a rise in marriage although is likely attributable to other sources as well. Unfortunately, data on contraceptive use is not available for PRAF. For Mexico, we do see a rise in contraceptive use but no change in fertility suggesting the rise in contraceptive use may have been induced by a decline in labor migration rather than a change in the demand for children. While these experimental analyses have considerable advantage in terms of the causal validity, we are restricted in our ability to interpret the changes to the period of the experiment. Thus, one needs to be cautious and interpret these findings as absolute indication of a change in what demographers term the "quantum" of fertility. It may be that parents in Honduras are responding to PRAF by advancing their childbearing to take advantage of the program. Such "tempo" effects have been much studied in recent years but their analysis goes back to Norman Ryder's seminal studies of U.S. fertility patterns (Ryder 1956; 1964). It will take a period of time before we know whether the effect of PRAF is really to change the quantum or tempo of fertility in

Honduras in the long-run, particularly since it has recently changed its program design in an attempt to limit any fertility impacts.

7. Discussion

CCT programs have emerged as a central strategy for reducing poverty and improving human capital outcomes in poor countries. Alongside the massive investment entailed in these projects, many countries have also incorporated experimental evaluations to allow researchers to gauge the impact of the programs. Our study focuses on the potential effect of these anti-poverty programs on outcomes that might be affected by the program but may not necessarily be either intended or anticipated. One such outcome of particular importance is childbearing. Because the programs target women with children, there is a possibility that the calculus of childbearing is to be affected by the programs. This is a possibility that follows all the way back to Malthus' argument against the Poor Laws in nineteenth century England.

In this paper, we use experimental data from three separate countries to explore the impact of poverty programs on fertility. Our findings provide strong evidence that program design does matter. Our data show that fertility is increased by PRAF – apparently because the program created additional incentives for childbearing by allowing parents to join or to obtain increased benefits from the program by bearing children after the program had already begun. In contrast, we found no impact of either RPS or PROGRESA on fertility. In both of these programs individuals were unable to join the program or to increase their benefits after the initial roster of eligible households had been set. It should be noted that PRAF administrators have already recognized this potential problem and have altered the eligibility criteria and payment scheme to mirror those of RPS and PROGRESA. The expectation is that in its current form the incentives to increase fertility will be lessened. Thus, one central message to take from this analysis is that the design of a program matters and can create unintended and undesirable consequences if administrators are not careful. In particular, careful consideration has to be given to how the roster is formed and whether it remains opened or closed.

While our first goal was focused on determining the consequences of the programs on fertility, we then turned to investigate the mechanisms through which these effects (or non-effects) were translated into fertility change. In the one country where we did find an effect on fertility, Honduras, we identified a substantial and significant increase in marriage associated with treatment. The data thus might suggest that PRAF led to an increase in marriage and increased marriage is certainly a potential contributor to higher fertility.

Another mechanism through which the fertility rise may have occurred is through reduced migration. Individuals may be prompted to stay home to take advantage of the possibility of entering PRAF and enjoying its benefits. While the data is inconclusive on migration in Honduras, this explanation would be consistent with findings from Mexico that showed that PROGRESA reduced migration [[omitted to avoid self-identification (2005)]]

We also investigated the impact of the programs on contraceptive use. Changes in contraceptive use would be a natural pathway through which changes in the demand for children resulting from the programs might lead to increased fertility. While this data was not available for Honduras, we did find signs that contraceptive use rose in Nicaragua and Mexico. Increased contraceptive use would be an unsurprising outcome given that most programs had at least some supply-side and reproductive health components. It may be that the increase in contraceptive use by women was partially, if not entirely, to counteract the increased exposure to pregnancy due to less migration and hence spousal separation. Without easy and affordable access to contraceptives, such a decline in migration may have led to a rise in fertility.

What is undoubtedly clear from this study is that programs can have unintentional consequences. Few program planners would likely be pleased to learn that these projects may have increased fertility, particularly among poor, rural women. In fact, we see that poverty programs do not necessarily have an impact on childbearing except when special incentives are introduced – unintentionally – that provide good reason for potential parents to advance their family building plans. Future studies will have to determine whether these advances lead to a long-term rise in fertility or whether parents have simply advanced the timing of their childbearing but will not have ultimately a greater number of children.

Figure 1: PRAF Data on Age-Specific Fertility Rates in 2000 and 2002 by Group

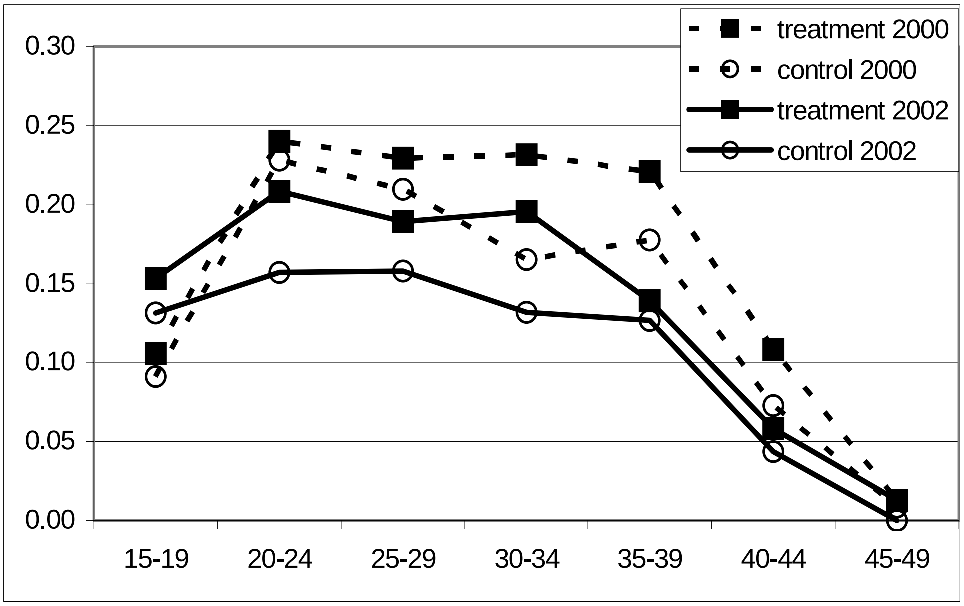

Figure 2: RPS Data on Age-Specific Fertility Rates in 2000 and 2002 by Group

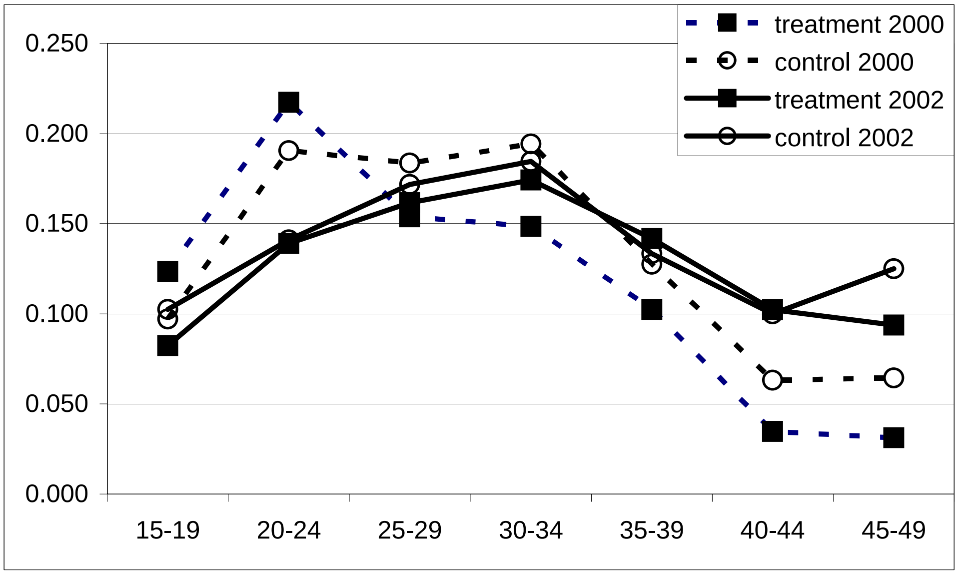

Figure 3: PROGRESA Data on Age-Specific Fertility Rates in 1997 and 1999 by Group

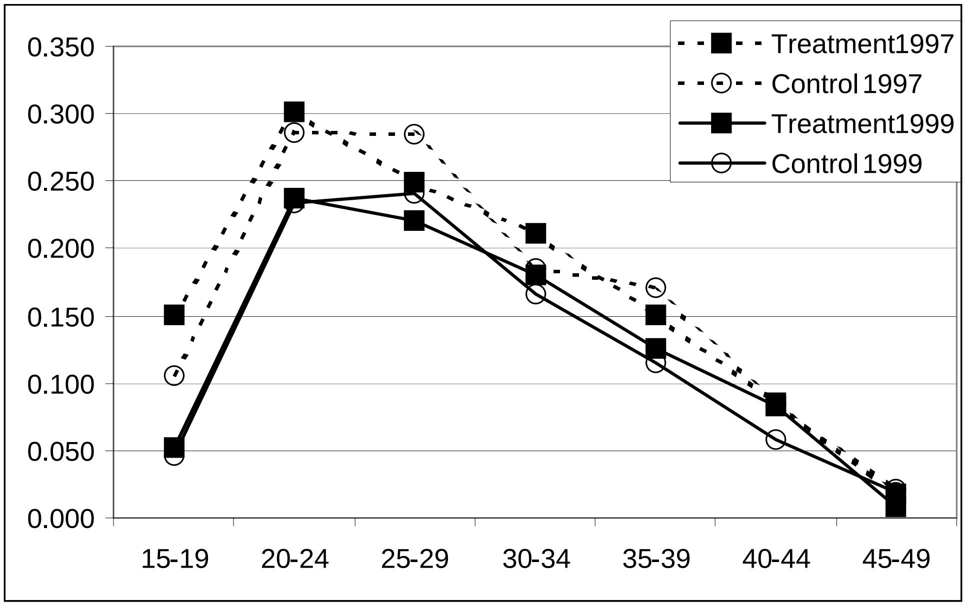

Table 1: Comparison of Treatment Group and Control Group Means including Test of Differences with Control for clustering

	PRAF			RPS			PROGRESA		
	Group Means		Difference Test	Group Means		Difference Test	Group Means		Difference Test
	Treatment	Control	P(Z)_	Treatment	Control	P(Z)_	Treatment	Control	P(Z)_
No Education	0.219	0.223	0.836	0.355	0.365	0.805	0.246	0.257	0.700
Some Primary Schooling	0.529	0.522	0.690	0.491	0.457	0.108	0.407	0.385	0.237
Completed Primary Schooling	0.195	0.193	0.920	0.105	0.104	0.975	0.296	0.299	0.896
Some Secondary Schooling*	0.035	0.040	0.639	0.049	0.074	0.16	0.041	0.052	0.086
Completed Secondary Schooling or more	0.021	0.023	0.873	-	-	-	0.010	0.008	0.356
Age Group 12-14	0.151	0.164	0.195	0.151	0.158	0.636	0.073	0.090	0.013
Age Group 15-19	0.176	0.171	0.638	0.268	0.276	0.66	0.122	0.129	0.428
Age Group 20-24	0.146	0.139	0.516	0.169	0.172	0.823	0.149	0.137	0.193
Age Group 25-29	0.154	0.142	0.321	0.125	0.121	0.832	0.186	0.190	0.735
Age Group 30-34	0.121	0.123	0.818	0.103	0.107	0.797	0.170	0.172	0.775
Age Group 35-39	0.105	0.121	0.025	0.090	0.078	0.162	0.157	0.141	0.046
Age Group 40-44	0.103	0.099	0.679	0.069	0.063	0.571	0.101	0.101	0.936
Age Group 45-49	0.046	0.040	0.197	0.025	0.025	0.964	0.041	0.041	0.923
Children Aged 1-15 in baseline	1.866	1.825	0.509	1.331	1.276	0.505	3.240	3.201	0.542
Logarithm of Per Capita Adult Expenditures in baseline	8.773	8.681	0.089	8.158	8.136	0.779	-	-	-
Probability of Birth Pre-treatment	0.172	0.153	0.130	0.108	0.112	0.773	0.179	0.173	0.538
Probability of Birth or Current Pregnancy Pre-Treatment	0.243	0.229	0.382	0.145	0.162	0.384	0.276	0.270	0.623
Number of Cases	3269	2354		1127	1237		5421	3396	

* Category is actually "some secondary schooling or more" for Nicaragua.

Table 2: First-Difference Model of Transfer Effects on Fertility

	Honduras PRAF		Nicaragua RPS		Mexico PROGRESA	
	Model 1 P(B)	Model 2 P(BCP)	Model 1 P(B)	Model 2 P(BCP)	Model 1 P(B)	Model 2 P(BCP)
Treatment	0.031	0.048	-0.004	-0.004	0.006	0.003
	[0.000]	[0.000]	[0.819]	[0.836]	[0.603]	[0.842]
Observations	6456	6456	2417	2417	8817	8817

Model estimated using Probit regression
Coefficients presented in terms of marginal change in the probability of the outcome.
Robust p values in brackets

Table 3: Difference-in-Difference Model of Transfer Effects on Fertility

	Honduras PRAF		Nicaragua RPS		Mexico PROGRESA	
	Model 1 P(B)	Model 2 P(BCP)	Model 1 P(B)	Model 2 P(BCP)	Model 1 P(B)	Model 2 P(BCP)
Treatment	0.013	0.01	-0.005	-0.016	0.006	0.006
	[0.240]	[0.499]	[0.751]	[0.405]	[0.538]	[0.623]
Post	-0.046	-0.067	0.002	-0.016	-0.038	-0.073
	[0.000]	[0.000]	[0.907]	[0.280]	[0.000]	[0.000]
Treatment x Post	0.021	0.043	0.002	0.013	0.001	-0.003
	[0.053]	[0.001]	[0.925]	[0.479]	[0.939]	[0.851]
Observations	12679	12679	4885	4885	17634	17634

Model estimated using Probit regression
Coefficients presented in terms of marginal change in the probability of the outcome.
Robust p values in brackets

Table 4: Difference-in-Difference Model of Transfer Effects on Marriage

	Honduras PRAF	Nicaragua RPS	Mexico PROGRESA
	P(Marriage)	P(Marriage)	P(Marriage)
Treatment	0.016	0.023	0.016
	[0.296]	[0.252]	[0.186]
Post	0.011	0.042	0.003
	[0.090]	[0.005]	[0.442]
Treatment x Post	0.021	0.012	-0.005
	[0.007]	[0.485]	[0.333]
Observations	12679	4885	16937

Model estimated using Probit regression
Coefficients presented in terms of marginal change in the probability of the outcome.
Robust p values in brackets

Table 5: Transfer Program Effects on Contraceptive Use in Nicaragua for 2002 using a FD Model and in Mexico for 1997-1999 using a DD Model

	Nicaragua RPS	**Mexico PROGRESA**
	P(ConUse)	**P(ConUse)**
Treatment	0.064	-0.003
	[0.071]	[0.875]
Post		0.066
		[0.000]
Treatment x Post		0.02
		[0.010]
Observations	3820	17634

Model estimated using Probit regression
Coefficients presented in terms of marginal change in the probability of the outcome.
Robust p values in brackets

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[i] This observation was made by researchers familiar with all data sets who noted that the consumption levels found in Honduras seemed high relative to Mexico and Nicaragua suggesting that the poor in Honduras were better off than the poor in the other countries which does not seem to make sense.

[ii] Details of the programs can be found in [omitted to avoid self-identification]

[iii] This assumption assumes that parents treat all children the same and runs counter to empirical evidence that birth order effects matter (see, for example Behrman and Taubman 1986). However, the assumption should be viewed as a restriction that allows for highlighting the trade-off between quality and quantity.

[iv] The household level attrition rate for PRAF, RPS and PROGRESA is 8%, 9% and 15% respectively. Analysis of the data suggests that attrition was similar for control and treatment groups in all cases and we expect no bias to be created as a result of attrition.

[v] Note that PRAF has been redesigned to avoid this problem.

[vi] The available abortion data do not distinguish between miscarriages and induced abortion.