Evaluating the Impact of the Poverty-Reduction Programs on Fertility:

The Case of the *Red de Protección Social* in Nicaragua[*]

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Abstract. Evaluating the impact of poverty-reduction programs on fertility is limited by the fact any shift in incentives for having children take time to be incorporated into decision making and the observation period for evaluation is usually quite brief. The purpose of this paper is to explore the use of birth spacing as a short-run indicator of the impact of poverty-reduction programs on fertility patterns. Using data from a Nicaraguan conditional cash transfer program that offers incentives for poor households to invest in the health, nutrition and education of children, we estimate the program impact on birth spacing using a stratified Cox proportional hazard model. The results indicate that the program decreased the hazard of a birth, indicating a subtle decrease in fertility.

JEL codes: J13, C41, H53, J11

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I. Introduction

Do poverty-reduction programs influence fertility? This question has been raised repeatedly since Malthus asked whether England's poor laws "create the poor which they maintain" (Malthus, 1890, pp 342). The relevance of asking whether poverty-reduction programs affect fertility in developing countries has intensified recently as assistance for the poor is increasingly provided through conditional cash transfer (CCT) programs (Stecklov *et al*. 2007). CCT programs most often provide funds directly to mothers, or the principal female, and benefits are tied to household compliance with a range of activities related to investments in the human capital of household members, especially that of children. These types of programs have the potential to alter fertility through a number of channels. Impact evaluations have shown that CCT programs lead to improvements in health and education outcomes among children (Rawlings and Rubio 2003). Such improvements in child human capital, along with the increased income provided by the program, may ultimately lead to a reduction in the desired (and actual) number of children within poor households as the economic model of fertility suggests (Becker and Lewis 1973). In addition, CCT programs aim to influence other proximate determinates of fertility, such as breastfeeding practices and use of contraceptives. Moreover, by transferring cash to women, CCT programs may increase the decision-making power of women in the household, leading to changes in fertility when the preferences of women differ from their spouses'.

By conditioning transfer payments on household investment in the human capital of children, CCT programs may influence the demand for children and inadvertently alter total fertility. Data on program impacts, especially experimental data, tend to be collected only during the life of the program and are thus often over the short term. Trying to identify the short-term impact of a poverty-reduction programs on fertility by looking at the total number of births is not likely to lead to clear results since changes in incentives for having children take time to be incorporated into decision making and only quite rapid or dramatic shifts in behavior are likely to be uncovered in a brief observation period using traditional measures. Even if long-term data collection is possible, by the time fertility impacts are determined it is likely to be too late to adjust the program to alter incentives. Thus, identifying the impact of fertility of poverty-reduction programs requires considering alternative measures to total number of births; namely a measure that can identify short-run fertility impacts.

One possible indicator of fertility decision making is birth spacing. Changes in birth spacing can indicate overall changes in fertility, or at the very least, a response to changes in factors affecting fertility decisions (Ward and Butz 1980; Ewbank 1989). Fertility timing has long been an important gauge of reproductive shifts as well as a major point of contention in understanding the fertility transition in both the historical European context (Anderton and Bean 1985; Knodel 1987) as well as the experience of developing countries (McDonald 1984). However, there is little evidence yet from either developed or developing countries regarding the effect of poverty-reduction programs, and in particular transfer programs, on fertility timing.

The objective of this paper is to explore the use of birth spacing as a short-run indicator of the impact of poverty-reduction programs on long-term fertility patterns. As a practical application of this approach, we examine the Nicaraguan *Red de Protección Social* (RPS) poverty-reduction program. *RPS* is a CCT program in Nicaragua, which, like many CCT programs in developing countries offers cash transfers to poor families through both a nutrition/health component and an education component conditional upon household members complying with a set of requirements related to the use of health and education services and participation in other program activities. The pilot phase of the program began in 2000 and included an experimentally designed evaluation that collected panel household data over a period of four years. The experimental design provides an opportunity to estimate the impact of the program on fertility as confounding factors are minimized. Maluccio and Flores (2005) find that the program significantly increased per capita consumption, child nutritional status and school enrollment. Despite these impacts on consumption and human capital, Stecklov *et al*. (2007) found no impact on fertility—measured by births and pregnancies—among women of reproductive age after two years of program operation.

Recently, however, an additional round of the evaluation survey has become available which expands the observation period from two to four years. We employ a hazard model using data on the timing of births, which allows us to account for the censoring of outcomes that arises from the fact that the women are still of reproductive age at the end of the observation period. The hazard approach can be modified to capture differences in birth hazards that are specific to a woman's parity (the number of live births a woman has had), likely arising from stopping behavior at higher parities. This approach enables us to identify the impact of RPS on the timing of births and, through examining the heterogeneity of impact on different women, shed light on the complexity of the program's impact on childbearing.

The next section briefly reviews the literature on modeling fertility decisions and outlines a framework for investigating changes in fertility through examining birth spacing. Section III summarizes current fertility and related indicators in Nicaragua and describes the RPS program. Section IV describes the sample, construction of the data and the empirical specification. Section V presents the results and implications are discussed in the final section.

II. Fertility and Birth Spacing

We begin by considering the basic economic model of fertility and the insights it provides into how a CCT program might influence fertility. This is followed by a discussion of how birth spacing fits into fertility decision making and how this can be considered in modeling the fertility decision. Finally, this allows for an assessment of how birth spacing can be used to determine the impact of poverty-reduction programs on fertility.

A. The Economics of Fertility and CCT Programs

In the basic economic model of fertility first developed by Becker (1960) and expanded by Becker and Lewis (1973), couples maximize utility from "child services" by choosing both the number and quality of children. While the details of the model are beyond the scope of this paper, the model provides a few key insights into the role of income and prices in fertility decisions and thus how CCT programs may influence these decisions.

If quantity and quality are normal goods, an increase in income would appear, in theory, to lead to an increase in both the quantity and the quality of children. However, this ignores the relationship between quantity and quality in determining the marginal costs of each component of child services and fails to recognize the overall parental objective of maximizing child services. If quality is more elastic with respect to income, as suggested by Becker and Lewis, an increase in quality resulting from an increase in income would make quantity more expensive relative to quality since each additional child would be of higher quality and thus cost more.[1] This would therefore lead to a subsequent reduction in the number of children suggesting a quality-quantity tradeoff as income increases. The effect of a change in the cost (price) of either the quantity or quality of children follows a similar logic. An increase in the cost of quantity leads to a decrease in quantity which lowers the marginal cost of quality, which in turn leads to

[1] A key assumption in this model is that all children are of similar quality.

an increase in quality. An increase in quality would then reinforce the decrease in quantity by increasing the marginal cost of quantity. As with income, the effect of a change in the price of quantity or quality must be understood by considering the relationship between the two in producing child services.

Thus, according to the Becker and Lewis type model, poverty-reduction programs that provide cash for health and education can influence the fertility decision. The extent and direction of that influence depends on parental preferences for quantity and quality children, the size of transfers and how the program influences the absolute and relative costs of child quantity and quality (Stecklov et al. 2007).

B. Incorporating Birth Spacing

While useful for considering broader shifts in fertility that have occurred within demographic transitions, this basic economic model is less useful when interventions occur in contexts in which household members are of reproductive age and are still building their families. As Namboodiri (1972) and others point out, fertility decisions are inherently sequential. Couples choose not only the number of children to have, but also *approximately* when to have each child. Thus, observed differences in the number of births among couples in which the woman is still of reproductive age captures not only the decision about how many children to have but also when to have them (Newman and McCulloch 1984). In addition, because fertility decisions are made sequentially, couples can adapt to changes in their economic or social situation and can update their goals for the quantity and quality of children they have. However, once a child is born, it cannot be unborn. Therefore, the number of children cannot be decreased in the short run, only held constant or increased.

Spacing also plays a role in child quality. A longer period of time between births can improve the health of the newborn child, the previous child (Pebley and Millman 1986; Huttly *et al*. 1992) as well as that of the mother (Winikoff 1983; Merchant and Matorell 1988; Rutstein *et al*. 2004). Greater spacing can ultimately lead to fewer total births, even if that was not the explicit objective, as the time in which births can occur is exhausted before the delayed births occur (Rafalimanana and Westoff 2000). The use of spacing is not a modern phenomenon; historically, populations have limited the number of children indirectly through practices such adherence to norms about post-partum abstinence and the length of breastfeeding (Hionidou 1998), often with the goal of improving the health of the mother and child rather than specifically to reduce total fertility (Knodel 1977).

In addition to affecting child and maternal health, spacing decisions can also have economic consequences for the family. For a given desired family size, spreading out births and lengthening the overall time spent bearing and rearing children can be costly, especially when the mother's opportunity costs are high or there are economies of scale in raising children (Schultz 1997). On the other hand, greater spacing can be optimal in the case of rising income over the life cycle and the absence of credit markets to smooth consumption intertemporally (Heckman and Willis 1975).

Fertility outcomes are also constrained by a couple's potential biological supply (Easterlin 1975), and by the inherent uncertainty under which they occur (Namboodiri 1972). Some couples are capable of having more children than others, and therefore, face higher costs to limit births than do less fecund couples. Given inherent fecundity, uncertainty arises from the fact that the timing of conception cannot be planned precisely. A woman's fecundity cannot be perfectly observed and contraceptive methods (with the exception of complete sterilization or abstinence) are not without failure.

To assess the impact of CCT programs on fertility in the short-run, an ideal model would incorporate both the sequential nature of decision making and the uncertainty with which outcomes are realized. Arroyo and Zhang (1997) review the many dynamic fertility models proposed since the Becker and Lewis (1973) static model including Heckman and Willis (1975), Wolpin (1984), Hotz and Miller (1988), Rosenzweig and Schultz (1985), Newman (1988) and Leung (1991). In general, in these models couples maximize the expected value of lifetime utility by choosing the level of consumption of non-child goods and services and the level/efficiency of fertility control in each period subject to budget and reproductive constraints. It is commonly assumed that the use of contraceptives reduces the period-specific utility and sometimes incurs a monetary cost as well. In each period, the probability of a birth occurring is a function of the couple's natural fecundity and the level of use of contraceptives.[2] Contraceptives are used when the expected benefit of preventing (or reducing the probability of) a birth exceeds the cost incurred from their use. Thus, the total number of children born at any given period and the space between births is the result of the series of period-specific decisions on the use of contraceptives.

Although birth spacing is not an explicit choice in these models, it is the outcome of the sequence of choices about contraceptive use over time and the resulting births that occur.

[2] It is recognized that contraceptives directly affect conception and that gestational length also affects the timing of a birth. Given that conception and the factors affecting conception (intercourse frequency, ovulation, etc.) are unobserved and complicated, the focus is placed on the observed outcome, a birth.

Assume child quality is determined by the space between births and the level of quality in the next period is therefore a function of the current probability of a birth occurring given the current quality and other factors. Furthermore, assume that reductions in the period specific birth probability (through the use of more effective contraception) increase quality, but at a diminishing rate. Thus, there is a limit to how much quality can be increased by more effective contraceptive use (in effect, spacing).

Couples therefore choose the level of efficiency of contraception in each period such that the expected marginal benefit to the remainder of lifetime utility equals the expected marginal cost of use. The benefit of greater contraceptive efficiency is the gain in lifetime utility associated with a reduced risk of unwanted children, while the costs are the disutility of use and the loss of lifetime utility from the delayed entry of desired children into the household. Since contraceptive use affects the probability of birth and quality, the benefits of greater efficiency of contraception include the expected increase in quality from greater spacing. Thus, couples face a trade-off between creating higher quality children and reaching the optimal number of children sooner. Preference for quality over quantity is expected to result in greater spacing of births, all else equal. Furthermore, more efficient contraception is expected once the desired family size has been reached, as the addition of unwanted children would reduce the couple's utility.

C. Evaluating the Impact of Poverty-Reduction Programs on Fertility

The model, which is based on evidence on the role of birth spacing in fertility outcomes, suggests that the impact of poverty-reduction programs on fertility decisions could be measured through its impact on observed birth spacing. Couples in the early stages of family building can respond to altered incentives by increasing (decreasing) contraceptive effectiveness to increase (decrease) the space between births and/or to stop (continue) family building at a lower (towards a higher) parity. The practical advantage of using this insight is that it allows for the use of data on the timing of births which is more sensitive than data on the number of births. For example, comparisons of changes in the number of births over a two year period between treatment and control groups may not lead to significant results but subtler shifts in the timing of those births may indicate changes. [3]

The question is whether this indicator is sufficient to get an assessment of program impacts on long-term fertility. Clearly, poverty-reduction programs are rarely aimed at increasing the fertility of the poor. Evidence to suggest that birth spacing remains unchanged or even increases as a result of a program clearly implies that fertility will decline. The only way it might indicate an increase in fertility is if the program somehow creates an incentive to delay having children while simultaneously produces a long-run incentive to increase the total number of children. This seems unlikely and no change or an increase in spacing appears to be a reasonable indication that a poverty-reduction program is not increasing long-term fertility. If, on the other hand, evidence suggests a reduction in birth spacing there are two possible explanations. One possibility is that the program increased the incentive to have children in the long run. Alternatively, the desired number of children has not changed, but the program has created an incentive to have children sooner. It is not possible to distinguish these too effects.

[3] Simulations were run to examine whether real changes in long-term fertility can be captured in the short run using spacing versus measures of birth. The results indicated that significant results are found for spacing and not number of births, particularly when the sample is small and when the time period is short. The results also indicated that *increases* in fertility are easier to identify as measured by births than are decreases in fertility. Thus, birth spacing measures are more valuable for these types of changes.

However, this issue is true of any short-term indicator of fertility effects including the use of total births. Birth spacing, however, would allow evaluators to detect more subtle shifts in fertility and the direction of possible long-term change. Thus, it does appear to be a good approximation of short-term fertility effects and likely provides a reasonable indicator of the impact of poverty-reduction programs on fertility.

The RPS program specifically aimed to increase child quality. In fact, impact evaluations have determined that the program has had positive impacts on aspects of child quality, such as nutritional status and education (Maluccio and Flores 2005). An (exogenous) increase in the quality of children should increase the future marginal cost of quantity, assuming children are generally treated in a similar manner by their parents. Couples that are still in the family building process are capable of adapting to these changing circumstances by re-evaluating their fertility goals in terms of ultimate quantity and quality. Specifically, in the case of RPS it is expected that the increase in quality, and the resulting increase in the marginal cost of quantity, will reduce the optimal quantity and increase the optimal quality of children. In addition to the increase in quality, couples could also experience a decrease in the cost of contraceptive use from the program, either the monetary or utility cost, reinforcing the decrease in the price of quality relative to quantity and the substitution of quality for quantity. Alternatively, it might be the case that RPS increases the incentive to have children by altering the marginal cost of each additional child. If this were the case, the expectation is that couples would decrease the space between children, possibly increasing the number of children they have overall.

Finally, note that prior research on fertility in Latin America has highlighted use of contraception for stopping future births, rather than for spacing, as is common in sub-Saharan Africa (Rodríguez 1996; Westoff and Bankole 2000). Thus, we might expect that the main impact of the program could be to increase stopping at lower parities rather than increasing spacing. This can be explored by looking at program impacts at different levels of parity.

III. The Nicaraguan context and RPS

Data from the 2001 Demographic and Health Survey (DHS) for Nicaragua (INEC *et al*. 2002) clearly indicate that fertility in rural areas is well above that found in developed countries and that many proximate determinants (Bongaarts 1978) are consistent with the high observed fertility. Women aged 45 to 49 have had an average 6.9 live births. The rural infant mortality rate is also high (43 per 1,000 live births) and nationally is strongly associated with birth intervals and prenatal care.[4] The median age at first marriage (including informal unions) in rural areas among women 20–49 is 17.3 years, although there is a three-year difference between women with no education (16.4) and those with secondary education (19.4). The median age at first birth (among women 25–49) is 18.7, with more than 80 percent of women aged 25-49 having had their first birth before age 25. Nationally, knowledge of at least one form of modern contraceptive method is basically universal; however, the proportion of women that have ever used (86.2 percent) and that are currently using (69 percent) is much lower. Moreover, contraceptive use in rural areas is about 10 percentage points lower than in urban areas. Most importantly, use of modern contraceptives is strongly associated with greater completed education (52 percent use among women with no education vs. 73 percent among women with secondary education) and by women's status in the household.

The median birth interval in rural areas is 30.7 months, which contrasts to that in urban areas (42.7 months). Nationwide, greater education and lower parity is associated with longer

[4] The mortality rate for children born less two years after another child is 60 per 1,000 live births, while that for children born 2 to 3 years after another child is 28 per 1,000 live births.

intervals between births, with a difference of almost 12 months between women with no education and those with secondary education, and around 6 months between women with two or three children and those with seven or more. Although recommended for the health of mother and child as well as for extending the period of postpartum amenorrhea, levels of exclusive breastfeeding for the first six months of a child's life are low. Less than 50 percent of children are breastfeed exclusively for at least two months and the rate drops to 12 percent for children between four and five months of age. Correspondingly, the median duration of postpartum amenorrhea is less than five months.

RPS is a cash transfer poverty-reduction program that conditions the receipt of two types of transfers, nutrition/health and education, on compliance with a set of health and education-related activities, respectively (Maluccio and Flores 2005).[5] The cash transfers are delivered bimonthly to the mother (or primary female in the household) under the expectation that women will be more effective at utilizing the additional resources for child human capital investment. All households are eligible for the lump sum health and nutrition transfer of approximately $224 per year (about 13 percent of total household expenditures), but the conditions for the receipt of the transfer depend on the composition of the household. Specifically, all household recipients must attend bimonthly health and nutrition information lectures. In addition, households must take children under age five to regular preventative care checks at the health clinic. The number and frequency of visits is determined by the child's age (see Maluccio and Flores 2005).

[5] In addition to the cash transfers for households, the program also included increased resources for schools and the contracting of a basic health care service package for program areas through NGOs.

Only households with children between seven and 13 years old are eligible for the education transfer, which is also a lump sum amount of approximately $112 per year (about 8% of total household expenditures). Receipt of the education transfer is conditional upon the enrollment and regular attendance of all eligible children that have not completed the fourth grade (see Maluccio and Flores 2005 for more details about the education component of the program). Thus, households with at least one child eligible for the education transfer could receive a boost of approximately 21 percent of annual expenditures through the program.

Since both types of transfers are per-household and not per household member, there is no direct cash incentive to add household members. In fact, adding to the family results in reduced average per-capita benefits for all other members. Perhaps more importantly, the addition of a new child in the household imposes additional requirements for the household to meet in order to receive the health/nutrition transfer without any compensating increase in benefits.

The pilot phase of the program began in September 2000 in a set of 42 rural *comarcas* located in the departments of Madriz and Matagalpa. An experimental evaluation was incorporated at the start whereby half of the *comarcas* designated to participate in the pilot phase were randomly selected to serve as the control group while the other half was selected to serve as the treatment group and began receiving benefits immediately. The original treatment group received transfers through December 2003.[6] Beginning in May 2003, the control group began to receive cash transfers thus ending the pure experimental design.

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[6] It is not exactly clear whether beneficiaries were explicitly told that the benefits would only last three years, however they were informed that the program was not permanent

Specific components of the program could have directly affected use and effectiveness of modern contraception methods, including the lactational amenorrhea method. As detailed above, although knowledge of modern methods is near universal in the country, use is much lower. The bimonthly health lectures included discussions about contraceptive methods, which could lead to increased use, as well as improved effectiveness, by shifting women to more effective methods and by providing more knowledge about correct usage. Moreover, the counseling that women received at their children's growth monitoring and preventative care check ups in the health centers included information about best practices for breastfeeding which could also result in lengthened post-partum amenorrhea and decreased risk of pregnancy.

IV. Data and Empirical Specification

Formulating fertility decisions within the framework of decisions affecting the probability of birth over time is easily translated into a hazard rate approach (Newman 1983), where we estimate the period-specific conditional probability of a birth for those at risk of having a birth (i.e. not pregnant and still of reproductive age). The hazard model captures the changes in period-specific birth probabilities that occur when a couple wants to delay the next birth or when demand for children changes in response to changes in income, prices or information. In addition, the hazard model accommodates censored data that arise when women are still of reproductive age. Changes in the probability of a birth translate into changes in birth spacing, and can be observed through the estimation of the associated survivor function. Although the program began operating on a specific date, its beginning in terms of fertility decisions actually varied for each woman, depending on when the last birth occurred. Thus, the hazard model approach utilizes all available information to estimate the impact of the program on fertility, which is particularly useful given the short period in which the program operated.

Newman and McCulloch (1984), Lehrer (1984) Hotz and Miller (1988) and Heckman and Walker (1991) use the hazard rate approach to study the timing of births in Costa Rica, Malaysia, the United States and Sweden, respectively.

A. Data

The data were collected as part of the experimental evaluation of the RPS pilot phase. A sample of households was observed repeatedly between 2000 and 2004 in both treatment and control communities. A baseline (pre-program) census and household survey were collected in 2000 and three additional survey rounds were collected in 2001, 2002 and 2004. Out of the original 1,581 households surveyed in 2000, 1,259 were interviewed in all four survey rounds (Maluccio *et al*. 2006). The final sample is composed 881 of women, between 14 – 45 years old in 2000, who are the head or the spouse of the household head at baseline and were observed in 2004.

To estimate the impact of the program on birth spacing, we need to construct a complete account of each woman's birth history using data from the household roster since birth histories were not obtained directly in the surveys. The baseline census and household surveys collected the date of birth, gender and identity of both father and mother (if also living in the household) for all individuals. In addition, in each survey round, women of reproductive age (12 – 49) are asked whether or not they have ever had a live birth and if so, how many. Furthermore, in 2002 and 2004, each woman who has given birth in the last five years is asked to report on the survival and date of birth of her last born child. Thus, we use data from the full roster of individuals that is observed across the surveys, along with the child mortality information, to reconstruct the birth history of each woman in the sample. This approach assumes that all children are present or were observed at least once over the four year survey period. Children that have since died will not be observed unless they died after the first survey was conducted, or they were the most recent birth and were born after 1996. However, the construction of the data for the treatment and control groups is the same, so this should not introduce bias in the estimate of program impact if child mortality was similar in both groups prior to the start of the program—an assumption we later test.

The fact that the birth histories are constructed indirectly from the information available in the data rather than directly through a birth history module poses other challenges. Since only those children that are observed in the household, or are reported through the mortality questions, are captured, it is highly likely that some births are not observed, especially for the older women or those that began having children very young. Children born long ago are likely to have already left the household. While this problem cannot be completely overcome, a decision rule is used to determine when the history should be adjusted for these unobserved births.[7] As long as

[7] When a woman's reported number of live births equals the number of children observed (including those having died), it is assumed that all births are observed. When the reported number of live births is greater than the number of children observed, a rule is used to determine whether the missing birth(s) is likely to be the first birth or an intermediate birth. First, it is assumed that women age 35 or younger are more likely to have their oldest child still living with them, and therefore, a missing birth is most likely to be an intermediate birth and not the first birth. Noting that a large majority of women in the sample under age 35 had their first birth before age 24 and recalling that the median age at first birth in rural areas is approximately 18, it is assumed that for women over age 35, the first birth is not observed in the data when the earliest observed birth occurred when the women was 24 years old or older. For the 54 women

adjustments are made equally across the treatment and control groups, they should not introduce bias into the estimate of the program impact.

Birth spacing is calculated as the length of time the woman was at risk of having a birth until a birth occurred, or was censored by the end of the observation period. A risk period is the time in which a woman is susceptible to having a birth. Time within a risk period refers to the duration of time in which the woman remains at risk. Women enter their first risk period at the start of menses, which is assumed to be age 12.[8] The first risk period ends on the date of the woman's first birth. Each subsequent risk period begins seven months after the birth of a child and ends when another birth occurs or when the observation period ends. Since spacing measures the time until a birth occurs, not conception, starting the next risk period seven months after a birth allows for the possibility that a women conceives immediately after a birth and delivers prematurely. Although few children were born less than nine months after the previous birth, to restrict the start of the risk period to nine months after a birth would eliminate the observations of very quick conceptions and/or premature births (a total of nine births are observed where spacing is less than nine months). The start of the next risk period seven months after a birth is

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for whom it was determined that the first birth was missing, the birth order of children is adjusted by assuming that the children that are observed are the most recent births.

[8] Although it is not possible to measure the start of the first risk period precisely, Pawloski et al (2004) report the average age at menarche among a sample of girls in Managua to be 12 and these data show that some women have their first birth as young age 12. In fact, a small number of women report that a child was born before age 12 and these women are not included in the sample.

also consistent with spacing reported for the Nicaragua DHS of 2001, where birth intervals are tabulated beginning at seven months (INEC *et al*. 2002).[9]

To preserve the pure experimental nature of the data, the overall observation period is assumed to end on the date in which the earliest birth that could have been affected by the program in the control group could have occurred. Since transfers began in the control group in May of 2003 and a minimum of seven months is assumed for a birth to occur, the earliest time at which a woman in the control group could become at risk of a birth and have been influenced by the program is January 31, 2004. Thus, outcomes for risk periods in which no birth occurs prior to the end of the observation period (January 31, 2004) are considered to be censored, even if a birth is actually observed beyond that date.

Given that women are likely to employ stopping methods after their desired family size has been achieved, we control for parity at the start of the risk period. Parity is determined from the total number of births observed up to the start of the risk period (when all births are observed in the data) and for the 54 women whose actual first birth is not observed in the data, parity is determined by the adjusted birth order, the construction of which was explained above.

Women are dropped from the sample when the age at the start of a risk period, or at least one spacing value calculated from birth dates, appears to be highly unlikely or extremely unusual. Eleven women are dropped because they report having had a live birth, but no children are observed. A total of 29 women are dropped either because their age at first birth is less than 12 or at least one birth is calculated to have occurred less than seven months after another birth.

[9] As a robustness check, estimates are found to be insensitive to whether we use an alternative minimum of eight or nine months between a birth and the start of the subsequent risk period.

An additional six women are dropped because of other inconsistencies between the number of births and the woman's age. After cleaning, the resulting sample has 835 women.

Table 1 reports descriptive statistics and tests of the equality of means across the treatment and control groups. In 2000, the women in both groups are on average just over 31 years old and have had an average of just under four children (including births of pregnancies observed in 2000). Most are married or in a civil union in 2000 and the average schooling completed is just over two years. In addition, completed parity at the end of the observation period in 2004 is just over four, and there is no difference between the two groups. These descriptive statistics are consistent with an experimentally designed sample.

B. Empirical Specification

The hazard model estimates the probability that a birth occurs for individual *i* in each time period *t* of the risk period, conditional upon the event not occurring at a previous time (i.e. the individual is still at risk), given observed covariates (X):

$$h_i(t) = \Pr(T_i = t \mid T_i \geq t, X_{it}), \tag{1}$$

where T_i is the time at which the event occurs or is censored by the end of the observation period. Covariates can be time invariant or time varying, thus both the *i* and *t* indexes are included in (1) for generality. As Schultz (1997) points out, the hazard model is not free of issues such as endogenous regressors and bias due to unobservable characteristics, however, the availability of experimental data greatly minimizes these issues in identifying the program impact.

In the RPS data, the time between births can be measured in days, because birth dates of children and women are available, which allows the use of a continuous-time, rather than discrete hazard model. Since the interest here is in estimating the impact of RPS on the hazard of birth, not on estimating the hazard function *per se*, the Cox model is used. The Cox model is a continuous time model that is semi-parametric, as the baseline hazard function is not parameterized, while the effects of other covariates are parameterized. We choose to center the covariates to facilitate interpretation of the estimated parameters, except in cases such as education and proportion male where "0" values are present and meaningful. The estimated model is a proportional hazard model, where the effect of each explanatory variable on the hazard of a birth is proportionally constant in every time period. The proportionality assumption can be tested and relaxed by allowing interactions of the independent variables with time (or more generally, a function of time). Parameter estimates that are asymptotically normal, efficient and consistent are obtained by maximizing the partial log likelihood associated with estimated equation (see Box-Steffensmeier and Jones 2004).

The reconstruction of the entire birth history for each woman means almost all women are at risk of experiencing a birth more than once. This means that risk periods and outcomes for each woman are likely to be correlated over time. Both stratified and random effects models are suggested solutions (Box-Steffensmeier and Jones 2004). We prefer the stratified model approach to the random effects approach since the latter requires making difficult assumptions about the underlying distribution for the estimated random effects and can lead to biased and inconsistent parameter estimates when the random effects are not independent of the explanatory variables (Blossfeld and Rohwer 1995; Hausman 1978). The stratified model allows for the baseline hazard to differ across strata (i.e. non-proportionality for the stratifying variable) and ensures that each woman is in each stratum only once. This eliminates the autocorrelation of women for the estimation of the baseline hazard functions for each stratum, but does restrict the effects of independent variables to be constant across strata. In addition, standard errors for the estimated coefficients still need to be adjusted for the correlation across observations for each woman.

The stratification approach offers the additional advantage in that stratification on parity at the start of each risk period restricts the model to compare outcomes among women of equal parity. Thus, to correct for correlation across birth intervals for the same woman, a stratified Cox model is estimated in which the strata *s* are defined by parity at the start of the risk period for parities zero (risk of first birth) through nine (risk of tenth birth, after which the sample size becomes too small):

$$h_{is}(t) = h_{0s}(t)\exp(\boldsymbol{\beta}\mathbf{X} + \beta_5 P_t + \boldsymbol{\gamma}\mathbf{R} + \boldsymbol{\alpha}\mathbf{M}) \qquad \text{for } s = 0,\ldots,9 \tag{2}$$

where the vector **X** includes the age in which the woman entered the risk period (age at-risk), the proportion of past births that were male (zero for all women when parity equals zero), the woman's completed education (as measured in 2000) and the year the woman was born. Age at-risk and the proportion male are invariant within the risk period, but vary across risk periods, while education and year born are invariant both across and within risk periods. The matrix **R** is constructed of dummies indicating the 5-year period in which the risk period started and **M** is a matrix of dummies indicating the woman's municipality of residence. The dummy variable, P_t, indicates whether the program is in operation at time *t* and varies over time within a risk period and across risk periods.

The proportion of past births that are male is included to control for the possibility that gender preferences in the composition of children affect the demand for children. Yamaguchi and Ferguson (1995) find that in the United States, women prefer a balanced gender composition, so that women with two children of opposite sex are less likely to have another child than women with two children of the same sex. In addition, Rahman and DaVanzo (1993) find that gender composition is an important factor in the timing of a subsequent birth in Bangladesh. We hypothesize that the lower the proportion of children that are male, the sooner the next child will be born. Age at-risk controls for the fact that a woman's fecundity declines with age and education controls for both preferences and income. Year of the woman's birth is included to control for the possibility of cohort effects and dummies indicating the five-year period in which the risk period started are included to control for factors that may change over calendar time but are otherwise unobservable in the data. Municipality dummies control for the fact that many factors affecting fertility decisions, such as family planning programs, health care and norms about breastfeeding or contraceptive use are locally influenced.

The proportionality of age at-risk is tested by modifying equation (2) to include an interaction of age at-risk with time. As indicated previously, couple specific-fecundity is an important factor in determining the probability of a birth occurring in each period. Heckman and Walker (1991) point out that spacing of prior births (for parities greater than 1) may be an indicator of a woman's natural fertility. However, lower spacing of prior births may also be a general indication of a preference for quantity over quality of children. In either event, both couple-specific fecundity and preferences for children are unobserved in the data. Thus, to test whether the estimate of the program impact is sensitive to the inclusion of a proxy measure of fecundity or preferences, the average spacing of past births is included for parities higher than one (i.e. periods in which the woman is at risk for her third birth or more) in an additional set of estimates.

V. Results

As an initial assessment of the impact of RPS on fertility and a point of reference for the results on birth spacing, Table 2 presents results of an analysis of program impact on the probability of having a birth and on total parity during the 30 month period for which data is available. If the program increased the incentive to have children the expectation is that the program would positively influence these variables while if it decreased the incentive to have children a negative effect would be found. To examine this impact a standard double difference specification is used which includes controls for general changes over time, initial time-invariant differences between control and treatment and an interaction of these two terms. The interaction measures the program impact since it indicates changes in the treatment group controlling for general trends and initial difference between the groups.

The results for the interaction of treatment and time indicate that the program seems to have caused a relatively lower probability of having a birth although the coefficient is not significant. Correspondingly, the results on parity suggest the program lead to lower total parity relative to the control group, although again the results are not significant. The results at least suggest the program did not increase fertility but it is not possible to draw strong conclusions about whether the program reduced fertility. For this, we move to looking at the results for birth spacing.

Table 3 presents the estimated coefficients for equation (2) along with several additional specifications. For all estimations, age at risk is centered on 30 and year born is centered on 1965. Table 3 shows coefficients but they are easily convertible to the hazard ratio for a unit change in a covariate, which is equal to the exponentiated value of the coefficient. The first column of results reports the estimated coefficients, which excludes the interaction of time with age and the measure of the average spacing of previous births. The estimated coefficient on the program indicator is -0.3886, which translates into a hazard ratio of approximately 0.68 and suggests that the hazard of a birth in each period among women benefiting from the program is only 68 percent that of women without the program. In other words, the program reduces the hazard of a birth occurring in each time period by approximately 32 percent. These results are not sensitive to the choice of number of months until the start of the next risk period or the inclusion of the children only observed through the infant mortality questions (results available upon request).

The second through sixth columns of Table 3 report the estimated coefficients for alternative specifications of the model to test the robustness of the results and to explore the role of parity on the program impact. Even though the data is experimental and baseline characteristics are similar, there might be some concern that the specification of the program indicator—which varies by time and risk period—may be capturing pre-existing difference in the treatment and control groups. The second model that is run includes a time and risk-period invariant dummy indicating whether the woman lives in a treatment community in addition to the time and risk-period varying program indicator. This variable is not significant and its inclusion does not substantially reduce the estimated impact of the program, reducing the hazard ratio only from 68 percent to 66 percent.[10]

The next specification includes an interaction between parity (centered at four) and the program indicator. Recall that the model is stratified by parity which in the hazard model is similar to having a fixed effect for parity since the underlying hazard is allowed to vary by parity (for reasons discussed earlier). The interaction then captures whether program impact varies by

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[10] A further test for pre-program differences in the hazard was conducted by limiting the observation period to only pre-program time and including a treatment group dummy. The coefficient on the treatment indicator was not significant, indicating that there were no differences in the hazard of birth between the two groups prior to the start of the program.

parity. The results for the interaction are negative and significant, indicating that the program impact is greater for women at higher parities. The hazard rates shown at the bottom of the table show that for women at risk of a second birth, the program reduced the hazard by 15 percent, while for a women at risk of a fifth birth the reduction was 39 percent, and for a seventh birth, 51 percent. This pattern may suggest that the program is leading to an increase in stopping behavior.

Since the risk of first birth (parity=0) may be very different from subsequent births, there is some concern that inclusion of parity zero in the model could be partially driving the results. The model is then rerun for on the sample restricted to risk periods for the second or higher birth. The results are consistent with the previous specification, indicating that the results are not driven by the inclusion of the observations of first births.

Stratification by parity and correcting the standard errors by clustering on each woman helps to deal with the correlation due to repeated observations. As an additional control for unobserved heterogeneity, we include a measure of the average spacing of all closed birth intervals following the first birth. In this case, our sample size is reduced since this variable is available only for risk periods starting at parity two or higher. To provide a proper comparison, the model is first estimated using the previous specification but for the subsample of risk periods starting at parity two through nine and then the additional variable is included. The results using the previous specification indicate the same increasing trend in the impact of the program with greater parity. The inclusion of the average spacing of previous births does not affect the estimated program impact, as the coefficient decreases by only 0.002, resulting in a decrease in the estimated hazard ratio for the program of only 0.001.

VI. Implications for Future Evaluations and Policy

Earlier findings using the probability of birth as an indicator of fertility suggested that RPS had no significant impact on childbearing (Stecklov *et al*. 2007). The results here suggest that this early impression was mistaken because of the measure of childbearing used in the analysis. The difference in results could partially be driven by the shorter time period studied previously. However, employing the same methods here for measuring the impact of RPS on fertility suggests an impact would not be found using that method even over a longer period. Determining short-run fertility impacts requires the use of methodology that is capable of capturing the more subtle changes to fertility that can be observed in this limited time period. Examining birth spacing using a stratified Cox hazard model provides an appropriate method. Using this approach, the RPS program is estimated to have reduced the odds of a birth occurring by about 32%. The results also indicate that the impact is greater among women with greater parity which may indicate that program mainly encourages earlier stopping rather than just increased spacing.

Although the results do not unequivocally indicate that a CCT program will lead to reduced total fertility, they do suggest that such programs may have subtle impacts on household fertility decisions that are consistent with the accepted development goal of reducing fertility and population growth. A program that targets poor families (with children), encourages investment in human capital and provides information and access to contraception by transferring cash to women can have an indirect and unintended effect of increasing the time between births. This suggests that if the increase in spacing continues, additional long-term impacts on human capital and poverty could occur through a reduction in total fertility rates. However, the program was in operation for just over three years and was never intended to be permanent. The real question is how much of an impact such a program could have on total fertility over this short period and if a greater impact would occur if benefits were extended. Alternatively, it is also possible that the shift in child spacing is what demographers term a "tempo effect," driven by a shift in the timing of births, and will not lead over the long run to any meaningful shift in the "quantum" or level of fertility (Bongaarts and Feeney 1998). Future research can investigate how long of an intervention is necessary to achieve long-term impacts on the quantum or level of fertility to significantly speed the transition from low to high fertility in poor rural areas.

The results have important implications for the collection of data for evaluating the fertility impact of poverty-reduction programs. The use of birth spacing as an indicator of the impact on fertility requires collecting data on the birth histories of women for each round of data collection. Only with this data can such an analysis be conducted. While we have identified an impact of the RPS program on fertility behavior, understanding the exact mechanisms driving the shift in child spacing cannot be precisely identified using the available data. Thus, we can not distinguish whether the effect is mainly due to changes in proximate determinants, such as contraceptive use or breastfeeding practices, or whether the impact is due to couples reducing their demand for children following the increase in the human capital of their children. Achieving an understanding of how the proximate determinants are affected by poverty-reduction programs would be a useful target for future research as it will aid in the design of programs that are more effective at reducing total fertility.

A final note regards the role of women's status in determining fertility outcomes. Since the transfers are delivered to the primary woman in the family, the program has the potential to improve the status of women and/or her decision-making power. An increase in women's status could translate into changes in fertility if women's preferences for family size or birth spacing are different from that of their spouses and they are more able to bargain for the level of use of contraception necessary to achieve the desired family size. In a qualitative study of the program, Adato and Roopnaraine (2004) find that women report an increase in their decision making power, especially in regards to food and expenditures related to the home, and self-esteem. In addition, "about half of the respondents—beneficiaries, their spouses and sometimes other household members—said that intra-household relations had improved since the introduction of the program" (p. 77). Although changes in female decision-making power cannot be empirically tested with the available data, women's status is likely to have a strong influence on the pace of the fertility transition, especially in rural areas of Latin America.

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Table 1. Baseline Statistics, Female Heads and Spouses, 14 - 45 Years Old

	Control	**Treatment**	**Test (p-value)**
Age in 2000 (yrs)	31.28	31.24	0.946
Parity in 2000	3.84	3.89	0.801
Parity in 2004	4.15	4.14	0.940
Years of education	2.07	2.28	0.289
In any union in 2000	0.90	0.93	0.168
Married in 2000	0.40	0.36	0.291
Per capita exp 2000 (C$)	3890	4004	0.603
Head of household in 2000	0.10	0.08	0.358
Age of Household head in 2000	36.35	36.34	0.981
Household head's Education (yrs)	2.07	2.13	0.727
observations	**406**	**429**	

Notes: weighted means; parity in 2000 also includes births of pregnancies that began prior to the program.

Table 2. Impact of *RPS* on birth rates and parity

	birth past 30 months full sample	parity full sample
year = 2004	-0.2122***	0.3099***
	(0.000)	(0.000)
treatment community	-0.0136	0.0245
	(0.658)	(0.829)
treatment * 2004	-0.0201	-0.06004
	(0.681)	(0.178)
age in 2000 (years)	-0.0173***	0.1372***
	(0.000)	(0.000)
years of completed education	-0.0049	0.0442*
	(0.396)	(0.086)
years of education of household head	-0.0100	-0.0701**
	(0.120)	(0.032)
age of household head	-0.0044**	0.0266**
	(0.032)	(0.019)
annual per capita expenditures in 2000	-0.0000***	-0.0002***
	(0.000)	(0.000)
municipality dummies included	yes	yes
Observations	**1670**	**1670**

Notes : marginal effects for probits reported; * significant at 90% confidence; ** significant at 95% confidence; *** significant at 99% confidence; pvalues in parentheses; survey weights applied in estimation; standard errors corrected for clustering at the community level

Table 3. Impact of RPS on the Hazard of Birth using a Cox Model Stratified by Parity

	parity 0 - 9	parity 0 - 9	parity 0 - 9	parity 1 - 9	parity 2 - 9	parity 2 - 9
program	-0.3886***	-0.4142***	-0.4943***	-0.4600***	-0.3562**	-0.3539**
	(0.002)	(0.002)	(0.000)	(0.000)	(0.011)	(0.011)
program*(parity - 4)			-0.1109**	-0.1054*	-0.1611**	-0.1622**
			(0.043)	(0.053)	(0.018)	(0.017)
age at start of risk period	-0.0199	-0.0200	-0.0193	-0.0395**	-0.0644***	-0.0500**
	(0.256)	(0.260)	(0.270)	(0.043)	(0.005)	(0.034)
age*time at risk	-0.0010***	-0.0010***	-0.0009***	-0.0010***	-0.0008**	-0.0008**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.022)	(0.024)
yrs education	-0.0193**	-0.0202**	-0.0193**	-0.0139	-0.0127	-0.0148
	(0.038)	(0.030)	(0.038)	(0.25)	(0.370)	(0.280)
proportion previous births male	-0.1992*	-0.2060*	-0.2011*	-0.2115**	-0.2216*	-0.2225*
	(0.058)	(0.052)	(0.055)	(0.044)	(0.078)	(0.075)
year born	-0.0045	-0.0045	-0.0046	-0.0278*	-0.0481***	-0.0478**
	(0.755)	(0.750)	(0.750)	(0.088)	(0.010)	(0.010)
average spacing (mos) of previous births						-0.0067***
						(0.001)
Treatment group		0.0441				
		(0.31)				
Municipality dummies included?	yes	yes	yes	yes	yes	yes
Dummies for 5-yr period of start of risk period?	yes	yes	yes	yes	yes	yes
Observations	**4119**	**4119**	**4119**	**3336**	**2549**	**2549**
Estimated Hazard Ratios						
parity = 0	0.68	0.66	0.95			
parity = 1	0.68	0.66	0.85	0.87		
parity = 2	0.68	0.66	0.76	0.78	0.97	0.97
parity = 3	0.68	0.66	0.68	0.70	0.82	0.83
parity = 4	0.68	0.66	0.61	0.63	0.70	0.70
parity = 5	0.68	0.66	0.55	0.57	0.60	0.60
parity = 6	0.68	0.66	0.49	0.51	0.51	0.51

Notes: coefficients reported; * significant at 90% confidence; ** significant at 95% confidence; *** significant at 99% confidence
pvalues in parentheses; survey weights applied in estimation; standard errors corrected for clustering at the individual level