How Does Income Affect Fertility?

An Analysis of *Oportunidades*, Mexico's Conditional Cash Transfer Program

By

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ABSTRACT

Although extensive economic theory and cross-sectional data associate greater income with smaller family sizes, there is little causal evidence of this relationship. Using regression discontinuity analysis, this study finds that Mexican women who received *Oportunidades* cash benefits long-term had more children from 1997-2007 (0.999 children; p value = 0.000) than those not receiving the payments. This robust finding casts doubt on the universality of the negative fertility-income relationship, and has significant implications for *Oportunidades*, which now reaches more than 20% of Mexicans. This unintended consequence may reflect pro-natalist incentives inherent in the program.

PART I: LITERATURE REVIEW

FERTILITY – TRENDS AND EFFECTS

I. GLOBAL FERTILITY TRENDS

In 1965, Dugald Baird proposed a fifth freedom to be added to Franklin Roosevelt's four: "freedom from the tyranny of excessive fertility." (Baird, 1965) At that time, contraception was limited, and women spent much of their adult life pregnant or breastfeeding. Since then, improved contraceptive technology, government family planning programs, and economic development have prompted a worldwide decline in total fertility rates.[1] In developing countries, family size has decreased by half, from about six to three children. (Cleland et al., 2006) Many regions with once high birth rates now have fertility levels at replacement level[2] or below.

The 2012 London Summit on Family Planning reaffirmed the need for investment in family planning; development leaders at that meeting committed to increase contraception information and access to 120 million additional women worldwide. (Hardee et al., 2014) Still, the unmet need for family planning[3] remains high in many developing countries, and there is significant variability in contraceptive use both between and within countries. Globally, about 40% of pregnancies are unintended, exemplifying the continued need for investment in reproductive health. (Singh et al., 2010)

II. FERTILITY IN MEXICO

Mexico is often promoted as a model for fertility transitions: its total fertility rate dropped from more than six children in 1974 to 2.02 children in 2011, below replacement levels. (Figure 1) (Romo Viramontes and Sánchez Castillo, 2009) This decline has largely been due to increased use of effective contraception. (Tuirán et al., 2009) Although the gap between rural and urban women has narrowed, rural women still have slightly higher fertility

[1] The average number of children per woman over her reproductive life if she experiences the average birth rates during every period of her lifetime.

[2] The replacement rate is the birth rate that maintains a static population size. The exact replacement level depends on mortality rates, but usually is between 2.1 and 2.5 children/woman.

[3] The number of people who state a desire to limit births but do not use contraception.

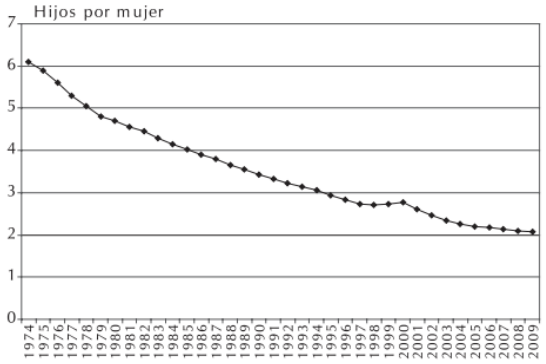

Figure 1: Total Fertility Rates in Mexico
(From Viramontes and Castillo 2009)

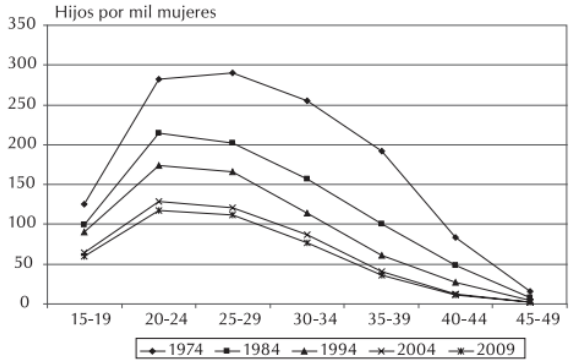

Figure 2: Fertility Rates by Age.
(From Viramontes and Castillo 2009)

levels than their city-dwelling counterparts. Likewise, fertility remains higher among indigenous women and among those with less education.

When the fertility trends are stratified by age, there is significant variation in the magnitude of decline. The youngest age group, ages 15-19, has experienced the smallest drop in fertility rates (Fig 2). This is surprising: when overall birth rates drop so drastically, the biggest decline is typically among the oldest and youngest cohorts, and the distribution of childbearing years narrows. Therefore, the high birthrate among Mexican teens is paradoxical given overall trends. Because of this uneven decline in birth rates, adolescents are now responsible for a larger proportion of births in Mexico than in previous decades.

More worrisome is newer data, which shows the teen birthrate actually increasing. In a large national health and behavioral survey, La Encuesta Nacional de Salud y Nutrición (ENSANUT), the crude fertility rate (births per 1000 women) rose 67% between 2005 and 2011, despite a previous drop (Fig. 3). A 2008 analysis of a UN survey, CELADE, noted a similar rise in adolescent pregnancy: the proportion of 15-19-year-olds in Mexico who had ever been pregnant jumped to 12.1% in 2000 from 10.4% in 1990. (Vignoli, 2008) Fertility rates plateaued or decreased in most other Latin American countries in this sample, although many countries saw the number of children per teen rise. (Ibid.) Clearly, adolescent pregnancy in this region has been resistant to family planning efforts.

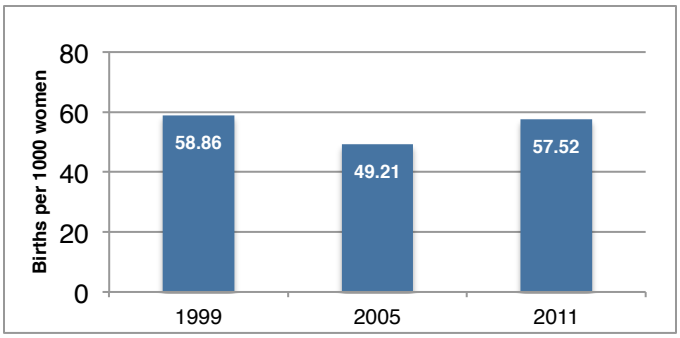

Figure 3: Crude Fertility Rate, 15-19 years old
(From ENSANUT 2000, 2006, and 2012)

The demographic composition of adolescent mothers in Mexico is weighted towards older teens. In the 2012 ENSANUT data, more than two thirds of those who had been pregnant were eighteen or nineteen, a pattern that was also noted in an earlier analysis of teen parents. (Instituto Nacional de Salud Pública, 2012; Núñez-Urquiza et al., 2003) This age group is also more likely to be sexually active than younger teens. (González-Garza et al., 2005)

Teen mothers in Mexico are also likely to be married or cohabitating. (Arceo-Gomez and Campos-Vazquez, 2014) One small, cross-sectional study of teens in Guanajuato found that all the teen mothers were living with a partner; most described being married or in a civil union. (This is in contrast to the non-parenting control group, all of whom were single.) (Chávez-Hernández and Gutiérrez-Marín, 2007) In another sample of 220 adolescent mothers in the state of Morelos who were interviewed between 1992 and 1994, 91.8% of the survey respondents were married or in a couple. (Núñez-Urquiza et al., 2003) Additionally, adolescent parents were more likely than their non-parent peers to have a mother who was also a teen parent.

Mexican teens also report low levels of unwanted pregnancy. 90% of teen parents in the Guanajuato study stated that their first pregnancy was wanted, and 63.3% said that the pregnancy was planned, although 57.1% said that they wished they had waited longer before the arrival of their first child. This high reported level of desired pregnancy holds across samples: only 22.73% of the Morelos sample reported that their pregnancy was not wanted. (Ibid.) This may be due to the high proportion of births occurring in married or cohabitating teens, who are more likely than single adolescents to plan a family.

Additionally, women may positively reframe attitudes toward the pregnancy after conception.

In both the ENSA 2000, a prior iteration of the ENSANUT, and in the 2012 ENSANUT, risk factors associated with teen parenthood included low schooling, early sexual debut, and having an older partner. (González-Garza et al., 2005; Gutierrez, 2014) The ENSA 2000 also found that only 69.2% of teens had knowledge of contraception, a percentage that jumped to almost 90% 12 years later. (González-Garza et al., 2005; Instituto Nacional de Salud Pública, 2012) It is surprising that teens' knowledge of contraceptives has increased while births have increased or plateaued.

Although the reasons for the resurgence of teen births in Mexico have not been formally studied, the phenomenon is already the target of a new government program. On January 23, 2015, Mexican president Enrique Peña Nieto announced a national adolescent pregnancy prevention program. The stated objective of this "Estrategia Nacional de Prevención del Embarazo en Adolescentes" is "to reduce by 50% the adolescent fertility rate between 15-19 by the year 2030." (Presidencia de la República, 2015) The details of how these ends will be achieved have not yet been released.

III. Why Does Family Planning Matter?

The high level of global unmet need for contraception is concerning for both ethical and practical reasons. First, and most importantly, controlling one's own fertility is a fundamental right. This concept was first established by the United Nations in the 1968 International Conference on Human Rights in Tehran, which stated in its proclamation, "parents have a basic human right to decide freely and responsibly the number and spacing of their children." (United Nations, 1968) Subsequent global population conferences have restated this value, which international organizations and governments are obligated to uphold.

Beyond social justice, family planning is associated with improved health outcomes. As more women use contraception, fertility rates drop. Fewer pregnancies and births reduce maternal mortality, including mortality from unsafe abortions. In 2000, 20% of global obstetric-related deaths and 90% of global abortion-related mortality and morbidity could have been avoided if women with a stated desire to avoid pregnancy had used contraception. (Collumbien et al., 2004) In addition, increasing birth spacing improves outcomes for

children: babies conceived within 18 months of an older sibling are at a higher risk of fetal demise, low birthweight, and premature birth. (Conde-Agudelo et al., 2006) This negative health impact may be due to depletion of maternal nutrients, such as folic acid, by the initial pregnancy. (Ibid.)

Further, fertility reduction has important economic effects. A smaller population lessens strain on a society's finite natural resources, reducing poverty on a community-wide scale. (Das Gupta et al., 2011) Additionally, reducing the proportion of the populace under working age – the youth dependency ratio – increases per-capita productivity. Many of these productive gains come from women, who, with fewer children, are able to obtain more schooling and work outside the home. As more women work, economic productivity will increase, which promotes development and improves living conditions. (Ibid.)

The benefits of family planning are especially impactful in younger women, who have the highest levels of unmet need for family planning: in 2014, USAID estimated that 33 million women ages 15-24 wished to use contraceptives but did not. (MacQuarrie, 2014) These teens may be especially at a risk for negative outcomes after a birth. Several studies in the 1980s found that teen mothers are more likely to be both educationally and economically disadvantaged after the birth of their children. (Davis, 1989; Hayes, 1987)

While often taken for granted, the deleterious effects of teen motherhood have more recently been called into question. Socioeconomic status and education have been highlighted as potential mediators in the relationship between early childbearing and economic, health, and social outcomes. (Coley and Chase-Lansdale, 1998) To investigate this link, researchers developed innovative analytic approaches, such as comparing cousins (Geronimus et al., 1994), sisters (Corcoran and Kunz, 1997; Holmlund, 2005), and using miscarriages as an instrumental variable. (Hotz et al., 2005) All four of these studies found that many of the negative outcomes previously associated with teen pregnancy lessen when preexisting demographic characteristics (such as poverty or education) are held constant. This confounding can overstate the adverse effects of teen parenthood.

Still, some researchers dispute these new analysis techniques. Ashcraft and Lang (2006) argue that using miscarriages as an instrumental variable underestimates the effects of teen motherhood; their analysis found that American teen mothers experience modestly adverse outcomes. Fletcher and Wolfe (2008) likewise contend that miscarriages are not a

valid instrument for birth. They found that teenage childbearing both reduces the probability of receiving a high school diploma and reduces annual income.

Although there is scant research on the outcomes of adolescent mothers in Mexico, one study found that, on average, teenage pregnancy causes a loss of 1-1.2 years of schooling, as well as lower household per capita income. (Arceo-Gomez and Campos-Vazquez, 2014) This study used propensity-score matching to control for socioeconomic status, so researchers could only balance the treatment and control groups on observable factors. The negative outcomes may thus be overstated.

Research on the children of teen parents has likewise struggled to adjust adequately for confounding. (Coley and Chase-Lansdale, 1998) A long-term study of children in Baltimore found that being born to a teenaged mother was not associated with negative educational outcomes after controlling for education and poverty. (Hardy et al., 1997) Next, analysis of a large Canadian cohort found that children born to teens had higher death rates, hospital use, academic failure, and adverse social outcomes than those born to older parents. (Jutte et al., 2010) Although this study controlled for socioeconomic factors, these were measured after the birth of the child. Without controlling for pre-pregnancy covariates, researchers did not adequately take into account selection into teen pregnancy. Finally, adolescent mothers in Mexico were more likely to be malnourished, have lower cognitive development scores, and worse behavior than their peers born to older parents, although no controlling for background factors was performed. (Buvinic, 1998) This research also found that Mexican adolescent mothers were more likely to be living in poverty, and had lower educational expectations for their children.

In sum, although there is no definitive causal link between early childbearing and poor outcomes, there does appear to be a strong cross-sectional association between the two. Any mitigation of this relationship is due to confounding by low socioeconomic status, a fact that highlights the high burden of early fertility on poor women.

IV. Determinants of Fertility

In designing interventions or policies to reduce fertility rates, it is essential to take into account the behaviors and beliefs that underlie reproduction. The classic demography approach considers fertility to be influenced by certain "proximate determinants," as established by John Bongaarts in 1978.

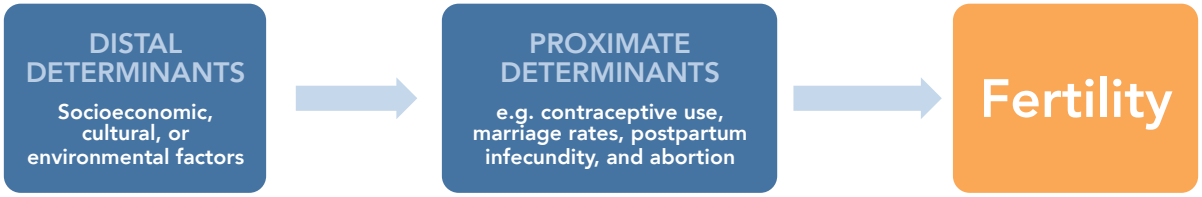

Figure 4: Distal and Proximate Determinants of Fertility – Demographic Model

To control births, Bongaarts's theory postulates, couples can modify their behavior in regards to the eight proximate determinants, which are 1) marriage rates, 2) contraceptive use and effectiveness, 3) induced abortion, 4) postpartum infecundity through lactational amenorrhea, 5) frequency of intercourse, 6) sterility, 7) spontaneous abortion (miscarriage), and 8) duration of fertile period of the menstrual cycle. (Bongaarts, 1978, 1982) The first four proximate determinants (marriage rates, contraception, induced abortion, and postpartum amenorrhea) explain 96% of the variation in fertility rates between countries. (Bongaarts, 1982) The way a couple approaches these proximate determinants will depend on social, economic, and political factors such as religion, access to contraception, poverty, and education. These macro-level elements are often referred to as distal determinants of fertility. (Figure 4)

V. Income and Fertility

To understand the effects of market forces as distal determinants of fertility, economists frame fertility as a resource-constrained choice. (Becker, 1960; Schultz, 2005) In this scheme, couples have a certain natural reproductive capability, *i.e.* "supply." By changing behavior in regard to proximate determinants such as contraception or abortion, a couple can achieve their desired number of births, or "demand" for children. (Schultz, 2005)

Using these principles, fertility decisions can be modeled in a utility-maximization framework. One basic model considers that parents will maximize their utility given a budget constraint, a time constraint, and a production function for children. (Jones et al., 2008) This production function allows biological influences on fertility to be linked formally

to the costs of having children based on observable factors like prices and wages, as well as on unobservable preferences for family size. This model can thus predict how fluctuations in an economic environment will impact fertility behavior. An in-depth analysis of current economic theories of fertility can be found in Jones et al., 2008.

If market constraints can dictate fertility behavior, how might potential parents respond to increased income? First, economic models developed in the 1960s and 1970s predict that, with more money, parents will invest more in child "quality" – allocating more resources to existing children – rather than conceiving additional ones (child "quantity"). (Becker and Lewis, 1973) Studying the substitution of quality and quantity empirically has been difficult, because the relationship between family size and measures of quality (e.g. educational attainment) are often confounded by socioeconomic factors.

Economic research has tested whether quality and quantity indeed function as substitutes – whether parents with exogenous increases in fertility (like having twins) invest less in each child. Rosenzweig and Wolpin used the birth of twins to approximate a random increase in fertility, and found that, with larger family sizes, educational investment (i.e. quality) decreased. (Rosenzweig and Wolpin, 1980) A more recent study using U.S. census data concluded that, in larger families, there was less private school attendance but no effect on whether a child was held back, a proxy for achievement. (Conley, 2004)

This decline in educational investment was most pronounced for children later in the birth order, a trend that has been affirmed in subsequent studies. Among families in Norway, for example, birth order was a much more powerful predictor of school and labor outcomes than overall number of siblings. Both later-born men and women had lower earnings, and later-born women were also less likely to work full time and more likely to become a teen parent than their older siblings. (Black et al., 2005) The significance of birth order in these analyses shows that finite resources are not always evenly distributed among children. In economic terms, parents are adjusting investment in child quality on the margins, i.e., on more recent children. This relationship is not universal, however. Researchers in China found a negative relationship between quality and quantity of children, even when controlling for birth order. (Li et al., 2008)

Notably, all the above studies focused on the nature of the quality-quantity relationship. There is no empiric literature on how income mediates the association between

child quality and quantity. Thus, the causal relationship between income and number of children remains theoretical.

Another way income could lower birthrate is by increasing the value of women's time. If increased household resources come through improved education and employment of women, the opportunity cost of pregnancy and child rearing will increase. This will incentivize family sizes to shrink. (Schultz, 2005; Subbarao and Raney, 1995) Additionally, on a larger scale, increased societal income can prompt development of public programs such as pensions and healthcare, which replace caregiving responsibilities traditionally performed by children. The need for adult children to care for elder parents diminishes, and families will have fewer children. (Ibid.)

It is important to note, however, that not all increases in income have the potential to lower fertility rates. Increases in physical capital, like land, may increase the value of child labor and, thus, the birthrate. This finding underscores the need to link increased wealth to its source to understand its potential effect on the relative "price" of children in a society. This determines its impact on fertility rates.

I. INTRODUCTION TO CCT PROGRAMS

Because families could respond to increased income by having fewer children, antipoverty programs may be an effective way to reduce unwanted or mistimed pregnancies. Conditional cash transfer (CCT) programs, an increasingly popular welfare program worldwide, distribute cash benefits to poor households contingent on their investment in human capital, such as providing their children with health screenings or education. (Fiszbein et al., 2009) Increased parental resources, improved child health, and investments in education all decrease the risk of passing poverty on to the next generation. (Harper et al., 2003) So, by combining short-term financial benefits with investments in social development, CCT programs aim to reduce both present and future poverty. Early CCT programs, such as those in Mexico and Brazil, showed significant gains in utilization of targeted services, as well as improvements in certain economic and health outcomes. Today, nearly every county in Latin America has a CCT program.

Conditional cash transfers have two economic goals. (de Janvry and Sadoulet, 2004) First, CCTs increase a family's immediate wealth by providing cash payments. In economic theory, this is known as the income effect: with more money, people will consume more of most goods. In the case of CCTs, people who receive money can afford to spend more time on education and on health. For the purpose of improving immediate financial well-being, cash transfers need not have a condition attached.

The conditional quality of CCTs addresses their second aim: redirecting suboptimal behavior caused by externalities, such as misinformation and erroneous beliefs. These factors can lead to skewed individual decision-making, which subverts a rational market. (Fiszbein et al., 2009) By tying a cash benefit to a behavior, CCTs reduce the perceived cost of that action. This prompts the recipient to consume more of the targeted service: the distorting factor is counteracted. This is the same strategy behind subsidizing universal education or providing low-cost health care.

The classic example of a conditional cash transfer beneficiary is a low-income family that undervalues the benefits of increased schooling, and so instead chooses to have their children work. A conditional cash transfer lowers the opportunity cost of studying compared to working, and the family will send the child to school. Not only does this

benefit the individual child, but also has a social impact as the younger generation as a whole becomes better educated. A family receiving a no-strings cash benefit, on the other hand, would not necessarily be motivated to substitute education for child labor.

It is important to note that CCTs only work when a market-distorting externality is encouraging suboptimal behavior, as described above. If individuals are able to make efficient, fully informed, and rational decisions, they will optimize their choices, even if that means withholding education from their children. Therefore, when CCTs are introduced in a system without problematic behavior, they will incorrectly distort consumption. (Ibid.)

There is reason to believe, however, that CCT programs address real weaknesses in education and health markets. In one empirical study of Mexican parents and their children, for example, both groups consistently undervalued the returns to schooling. (Attanasio and Kaufmann, 2009) This distorted view subverts otherwise rational decisions. In a perfect market, parents and children would weigh the true importance of education against the benefits of dropout. In reality, these misguided beliefs, which were most pronounced among the poorest families, may prompt students to drop out of school.

There are other reasons why people may not make entirely rational decisions about investment in their children. One common factor that may distort parental decision-making is incomplete altruism, or principal-agent issues: situations in which parents make choices that are in conflict with a child's interests. Most models of education or health decisions assume that a family acts as a cohesive unit. In reality, children must defer decisions to their parents, who may discount the future more than they would. (Fiszbein et al., 2009) Unfortunately, this concept is harder to measure than are educational expectations, so there is little direct proof to support this claim. Still, the ample evidence that mothers invest more in their family than fathers do proves that each parent can place differential values on child utility. (Vyrastekova et al., 2014) Based on this data, it is plausible that fathers would not place as much emphasis on education as children would themselves.

Beside this economic rationale, there is a social justice motivation for conditional cash transfer programs. The poor are often relegated to low-productivity endeavors, since they lack access to the same credit or insurance markets as those who are better off. Redistributing capital through programs such as CCTs alleviates this inequality, and allows families to make long-term investments that would otherwise be out of reach. By directly

targeting low-income households, CCT programs may also circumvent the failure of traditional government programs to reach the poor. (Fiszbein et al., 2009)

It is important to note that CCT programs must depend on local infrastructure to provide the services required. (Lagarde et al., 2007) By incentivizing uptake of primary care or education, CCT programs assume that those services are available and effective. If a country does not have an adequate foundation of social services, CCT programs will not improve targeted outcomes. Also, it is essential that the transfers do not create a disincentive for adults to work. Current analysis of CCT programs in general has found that they have had, at most, a modest negative effect on employment. (Fiszbein et al., 2009) In particular, Mexico's *Oportunidades,* discussed in detail below, has not caused any decrease in adult labor force participation. (Skoufias and diMaro, 2006)

II. OPORTUNIDADES: HISTORY AND STRUCTURE

Mexico was one of the first countries to institute a CCT program. This initiative, initially called *Progresa* and later rebranded as *Oportunidades,*[4] began in 1997. Due to limited resources, the program was initially implemented as a randomized control trial, with some communities randomized to receive benefits eighteen months later than others. (Fernald et al., 2009) To begin, the Mexican government randomly selected 506 experimental communities. Eligible households in these areas were selected using a poverty score, composed of several measures of household resources and development from census data. Extensive evaluation was performed by the Instituto Nacional de Salud Pública on both early and late intervention groups, as well as on community members who were too wealthy to be eligible for the program. (These surveys are referred to as ENCEL; separate evaluations were performed on rural and urban communities.) To compensate for the loss of the control group in 1998 (when control households began receiving benefits), additional communities were added to the sample in the 2003 evaluation round. (Figure 5) These communities were chosen nonrandomly to match socioeconomic distribution of existing *Oportunidades* communities. (de la Torre García, 2005)

[4] *Oportunidades* was recently renamed *Prospera*. (See: http://diario.mx/Nacional/2014-09-02_cce9f936/programa-oportunidades-cambia-de-nombre-a-prospera/)

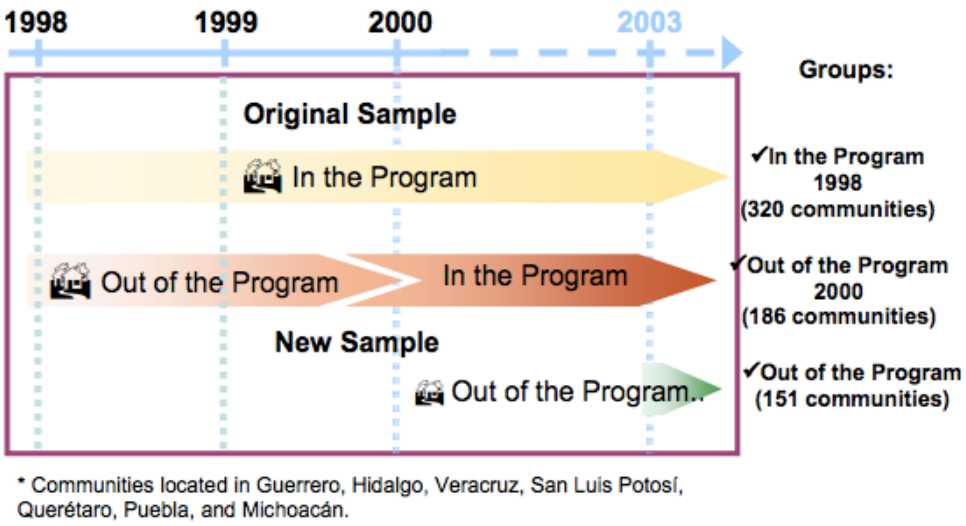

Figure 5: *Oportunidades* sampling structure
(From de la Torre Garcia 2005)

Oportunidades provided three distinct benefits at its inception in 1997. (Some program features have since changed.) First, every other month, beneficiaries collected cash transfers that increased household income by 20-30%. The *titulares*, or transfer recipients, were the female heads of the household. (This program design is based on evidence that mothers invest more in their families than fathers; see Vyrastekova et al. 2014.) Next, families received educational scholarships for children in third grade through high school, which increased in value for groups are most vulnerable to dropout (female and older children). Finally, pregnant and lactating women, children 6-24 months, and children 2-4 with low bodyweights (as identified in health care visits) received milk-based food supplements. Since its establishment, the program has been expanded nationally. As of the beginning of 2015, it served more than 6 million Mexican households. (Secretaría de Desarrollo Social, 2014)

In exchange for these benefits, program participants had to adhere to both educational and health conditions. All children were required to school, maintaining a minimum attendance of 85% monthly. Students also received a bonus for finishing high school. (Levy, 2006) All members of the family had to complete preventative health care visits, and those older than 15 had to attend *plácticas*, health education talks. Since the program's inception, only about 1% of households have been denied benefits because of noncompliance. (Fernald et al., 2009)

III. OPORTUNIDADES: EFFECTS

There is substantial research on *Oportunidades* from several time points – early analyses, which used data from the eighteen-month experimental window; medium-term effects, using the 2003 data; and long-term outcomes, taking into account all ten years of data collection (1997-2007). All studies cited below sourced their data from the ENCEL surveys collected as part of the official external evaluation unless otherwise specified.

A. EARLY EFFECTS: 1997-1999

In the eighteen months after initiation of the program, uptake of primary health care increased significantly. In the early treatment group, preventative care visits increased and hospitalizations dropped. (Gertler et al., 2001) There was an immediate improvement in child health: children born during the intervention period experienced a 25.3% lower rate of illness than the control group, as well as increased height and growth, less stunting, and less anemia. (Behrman and Hoddinott, 2005; Gertler, 2004; Rivera et al., 2004) This was likely due to an increase in primary care visits and food supplementation. Adults in treatment households also reported better health, including less difficulty with daily activities due to illness and fewer days incapacitated. (Gertler et al., 2001)

Oportunidades also showed immediate benefits in the domain of education. For children of both sexes, transfers increased school attendance across grade levels and reduced involvement in work activities. (Dubois et al., 2012; Skoufias and Parker, 2011) Increases in enrollment were greater for girls than for boys, which narrowed the gap between the two sexes. (Schultz, 2004) The study by Skoufias and Parker, however, found a positive effect on academic performance in primary, but not secondary, school. (Skoufias and Parker, 2011) This pattern of increased service utilization without a boost in outcome measures is common in CCT programs located in areas with weak infrastructure. If children attend a low-performing school, their academic performances may remain poor, even if attendance improves.

The program, which distributes the cash transfers to female heads of household (*titulares*), also had a measureable effect on female empowerment and bargaining power. With their new financial responsibilities, women in *Oportunidades* reported having more control over household spending. In qualitative interviews, women described increased

confidence after receiving the transfers: some felt more able to leave the house, and others more comfortable speaking out in groups. (Adato et al., 2000) Finally, women participating in *Oportunidades* had increased knowledge of contraception, perhaps from participating in *plácticas*. There was also a small but statistically significant increase in contraceptive use. (Huerta and Hernández, 2000)

B. MEDIUM-TERM EFFECTS: 2000 TO 2003

By this time, the oldest children had been receiving educational scholarships for 5.5 years. School enrollment increased up to 41.5% in rural areas, and probability of child labor concomitantly decreased. (de la Torre García, 2005) (Note that in the official external evaluation in 2003, new communities had been selected as controls. This selection process was based on nonrandom matching, and thus may be vulnerable to bias.) Treated children entered school earlier (1% reduction in age of entry), and accumulated more grades of schooling (8-9% more) than their peers. (Behrman et al., 2009) Again, despite improved school attendance, achievement tests did not show any increase in reading, writing, or mathematics. (Ibid.) There was no impact on cognitive development, despite the improvements in nutrition. Still, children in the program experienced improvements in motor skills (boys and girls) and improved development of socio-emotional skills (girls). (Gertler and Fernald, 2004)

The utilization of public health services in rural areas increased by 35% and by 17% in urban areas. (de la Torre García, 2005) This increased use of preventative primary care had a positive effect on the health status of beneficiaries: sick days decreased for both children and adults, and levels of hypertension and obesity dropped in adults. (Gutiérrez et al., 2005; de la Torre García, 2005) Consistent consumption of food supplements led *Oportunidades* children to have less anemia and more growth than controls. (Ibid.) In the group receiving program benefits, knowledge and use of family planning methods increased. (Hernández-Prado et al., 2005)

C. LONG-TERM EFFECTS: 2003 TO 2007

From an economic standpoint, *Oportunidades* increased overall consumption, reduced measurable poverty, and boosted agricultural and business investment. (Gertler et al., 2012) These effects result from the ability of the cash benefit to aid recipients in overcoming credit

constraints or making riskier investments that they may have previously avoided. Thus, the cash transfer has achieved its aim of reducing financial hardship.

There was a strong positive effect on education: young adults in the program completed more grades (an increase of 0.9 years for 19-20 year olds and 0.6 years for ages 17-18) than those who had never been in the program; this impact on schooling was slightly larger for girls than for boys. (This could be due to differential migration, and thus loss to follow up, by gender.) (Parker and Behrman, 2008) Both secondary school completion and the rates of students entering high school increased compared to those who never received benefits. There was no effect, however, on college enrollment, which continued to be low (about 2%). (Ibid.) Additionally, in the ten-year analysis, male long-term beneficiaries who received support through primary or secondary school earned a 12% to 14% higher salary than those not in the program, depending on their level of education. (Rodríguez-Oreggia and Freije, 2008) Women did not see similar effects, perhaps due to lack of rural employment opportunities.

Like in earlier points of analysis, *Oportunidades* beneficiaries used health or community clinics more than those not in the program. (Bautista-Arredondo et al., 2008) However, none of the impacts on health observed in *Oportunidades* participants in earlier analyses, including reduced diabetes, obesity, and sick days, persisted in the ten-year analysis. (Ibid.) There are several potential causes of this finding:

> This result can be interpreted in different ways. It could be a result of two opposing effects: family members with more time spent in the Program tend to fall ill less frequently, but this effect is nullified by the tendency of these persons to be more aware of health issues and have more information, which leads them to report illnesses more frequently. […] An alternate interpretation is that the households that were incorporated into the Program earlier were different than those incorporated later; in particular, they were in worse shape and had a greater probability of having a poorer state of health in the absence of the Program. If the latter situation is true, the fact that no differences were found in the state of health in 2007 could be evidence that the Program has leveled the beneficiaries to an equal state of health, regardless of the status of their initial condition at the implementation of the Program. A third alternative is that some of the effects of the Program are largely achieved in the short term, and that it is difficult to observe further improvement once these changes are achieved. (Ibid.)

Despite this finding, *Oportunidades* did improve vaccination rates in older adults, an effect attributable to the increased preventative primary care visits. (Salinas-Rodríguez and Manrique-Espinoza, 2013)

Additionally, *Oportunidades* did lead to long-term improvements in child health and behavior. In one analysis by Fernald, et al., children who were in the early treatment group showed significantly increased height, improved cognition, improved vocabulary, and fewer behavioral problems than those who received the benefits later. (Fernald et al., 2009) Children in treatment households also reported fewer health problems than their peers whose families did not receive benefits. (Bautista-Arredondo et al., 2008)

IV. CCTs and Fertility: Background

Given the link between income and fertility, how could CCT programs affect birth rates? This question is especially important as welfare programs inevitably raise questions about whether they indirectly motivate families to have more children to maximize benefits. *Oportunidades* has several specific features to prevent this incentive. Families receive a constant lump sum for the cash benefit that does not increase during their time in the program, even if more children are born. Instead of providing more income, additional children impose further conditions, as they, too, are subject to the health and education requirements. Additionally, educational scholarships, which do vary based on number of children, are not implemented until third grade: families must wait nine years after a birth to see their subsidy level increase.

Further, several pieces of evidence suggest that CCT programs actually *lower* fertility rates. First, because the programs increase income, there would likely be a shift of resources from child quantity to child quality, as discussed in the previous section. The conditional incentives also lower the price of investing in the health and education of current children, further pushing the quantity-quality substitution. Finally, all members of the family older than 15 may be better informed about ways to control fertility after attending reproductive health-focused *plácticas*.

For adolescents, there is an additional causal pathway between CCT programs and lower birthrates: through incentivized school attendance. The association between female education and less risky sexual behavior has been well-studied using cross-sectional methods. (Zuilkowski and Jukes, 2012) In an analysis of six Latin American countries (not including

Mexico), higher education was associated with later sexual debut. (Flórez and Núñez, 2001) Additionally, the connection between school attendance and teen birthrate is evidenced by historical data: the introduction of compulsory schooling in the US and in Norway directly reduced the incidence of teen births. (Black et al., 2008)

To understand why education and early fertility are related, it is helpful to parse the elements behind reproductive decision-making. In the utility maximization model, an individual must balance a desire to have a baby with the long-term economic costs of childbearing. If a woman feels that she has limited future opportunity, she is more likely to choose to have a baby early in life over continued education. Staying in school is difficult after a pregnancy; without extensive educational support, many teen parents, especially in Latin America, drop out. (Näslund-Hadley and Binstock, 2011) Although education and parenthood are not necessarily mutually exclusive, perceived low future prospects may also spur the undervaluing of education's benefits. In a WHO review, "Risk and Protective Factors Affecting Adolescent Reproductive Health in Developing Countries," low future aspiration was a risk factor for pregnancy occurrence in both studies that investigated the connection. (Blum and Mmari, 2005)

Another quantitative study, The Miseducation of Latin American Girls: Poor Schooling Makes Pregnancy a Rational Choice," found that young women in Paraguay and Peru who had low educational expectations were more likely to become teen mothers than their peers with higher motivation. The authors explain: "…the results of this study do not support the conventional assumption that the problem of school dropouts begins with the pregnancy, but can also result from early formal unions, the low quality of education offered, and generally low expectations of life." (Näslund-Hadley and Binstock, 2011) Girls who are uncertain about future prospects tend to have similar ambivalence about becoming pregnant, which exacerbates a disconnection from the formal education system. Further, in the United States, teen mothers lower their educational goals further after the birth of their first child, making it even more difficult to catch up to their peers. (Beutel, 2000)

Even though many pregnancies are not consciously planned, these economic rationalizations also affect proximal determinants of fertility, such as contraceptive use. If a person does not perceive a future of economic opportunity, for example, she may be unmotivated to use birth control, and may become pregnant sooner than her more hopeful peers. In one 1991 sample of Mexican adolescents, for example, teens who did not use

contraception were less future-oriented than those who did. (Pick de Weiss et al., 1991) Another large cross-sectional study of American teens found that those ambivalent about pregnancy were less likely to use contraception. (Bruckner et al., 2004) These girls, already less hopeful about the future, further drop behind their peers by unwittingly choosing childbirth over education.

The connection between future expectations and teen motherhood is evidenced on a larger scale by the variation in teen pregnancy in the United States, which is best explained not by differential sex-education policies but by state income inequality. (Kearney and Levine, 2011) This evidence highlights perceived lack of economic mobility as an antecedent of high teen fertility in unequal states.

If a woman undervalues the true benefits of delayed childbearing, a CCT program that encourages her to stay in school would appropriately correct this market failure. Through its educational benefits, a CCT scheme reduces the price of school compared to pregnancy. This creates short-term incentives against dropping out that may counter the perceived futility of delaying childbearing. Not only does paying young people to stay in school force those who are ambivalent about parenthood to start using contraception so that they do not drop out, but better-educated women have more control over their reproductive choices – a mutually reinforcing cycle. An economic model developed by Cortés et al. confirms that educational CCT programs can successfully reduce teen pregnancy if benefits are tied to performance requirements that preclude pregnancy, such as enrollment in the next grade. (The authors assume that a pregnant teen cannot finish a school year and must repeat the grade.) (Cortes et al., 2011) For young adults who had been in the program ten years, *Oportunidades* successfully increased both overall grades completed and rates of secondary school graduation, effects that were stronger in girls than in boys. (Parker and Behrman, 2008) *Oportunidades* may have an effect on adolescent fertility through its success in improving girls' educational attainment.

V. CCTs and Fertility: Evidence

There has been no targeted CCT program to reduce teen pregnancy, despite theory that it could be efficacious. The randomized control trials that do use CCT programs to improve sexual health in young women have all been focused on reducing HIV rates in sub-

Saharan Africa.[5] Even though these policies did not focus primarily on teen pregnancy, they successfully addressed risky sexual behaviors. The CCT program implemented by Hallfors *et al.* in Zimbabwe, for example, reduced school dropout by 82% and marriage by 63% in orphan adolescent girls. (Hallfors et al., 2011) In Malawi, another CCT intervention found that, for participating girls who were out of school at baseline, the probability of getting married and becoming pregnant declined by more than 40% and 30%, respectively. (Baird et al., 2010)

The same team also tested an unconditional cash transfer (UCT) program against a CCT in 2011. They found that pregnancy rates declined even further in the group that received no-strings-attached cash, perhaps due to effects in girls who had already dropped out of school. Researchers in South Africa have similarly found beneficial results from unconditional government cash transfer programs on transactional sex, age-disparate sex, unprotected sex, multiple partners, and sex while intoxicated or after taking drugs. (Cluver et al., 2013) These are striking findings from both UCT's and CCTs, yet very few randomized control trials have linked education-focused antipoverty programs to sexual behavior outcomes in teen girls.

Even though increased family planning was not a stated goal of Latin America's large-scale CCT programs, researchers have analyzed several for fertility effects. Todd et al. found that the Red de Protección Social in Nicaragua decreased the hazard of a birth for its beneficiaries. (Todd et al., 2012) In 2011, researchers found that Brazil's Bolsa Familia, another well-know conditional cash transfer program, did not significantly affect fertility rates. Most relevant for this paper, Stecklov and colleagues analyzed CCT programs in Honduras, Nicaragua, and Mexico. (Stecklov et al., 2007) They found that Honduras's program unwittingly incentivized births by increasing benefits for additional children (after the program had already started). Unsurprisingly, the birth rate among beneficiaries in Honduras increased when the participants were offered this incentive. Conversely, *Oportunidades* and the *Red de Protección Social* in Nicaragua, neither of which allowed families to earn additional benefits by having additional children, had no effect on fertility rates.

[5] A study in 2012 by Walque *et al.* directly paid participants in Tanzania to maintain negative STI tests. This and similar programs, which do not incentivize investments in human capital, should not be considered a traditional CCT program and will not be discussed here. (Walque et al., 2012)

It is important to note that, in studying *Oportunidades*, Stecklov *et al.* only used data from the eighteen months of the formal randomized control trial. Depending on the mechanism behind the impact on fertility, this may not be enough time to observe changes in fertility rates. An increase in family planning use mediated by the information shared at *plácticas* could immediately lower fertility rates. Changes in reproductive behavior from an income effect, however, would take longer to establish, since a year and a half of benefits might not have dramatically changed household wealth. Finally, a long-term reduction in teen pregnancy through increased female education would not have occurred, as the difference in education in the two study groups was less than a grade in the ten-year analysis. (Parker and Behrman, 2008) Thus, although the paper by Stecklov *et al.* may identify true changes in fertility patterns in the first eighteen months of the program, long-term research is needed to identify all fertility effects.

While there are a few other analyses on the impact of *Oportunidades* on births, none examined fertility long-term. Juan Pablo Gutierrez conducted an informal analysis in a report for the Instituto Nacional de Salud Pública, which showed that the program reduced adolescent fertility from 2005-2011, but had no effect on overall fertility. (Gutierrez, 2012) Another study, whose aim was to evaluate alternative program designs, used a dynamic behavioral model to predict that *Oportunidades* would not affect lifetime fertility rates. (Todd and Wolpin, 2006) Finally, there seemed to be no programmatic effect on birth spacing in either the 2000 or 2003 ENCEL data. (Feldman et al., 2009)

The impact of *Oportunidades* on births to teens in particular has also been inconclusive. Darney *et al.*, using a cross-sectional national survey from 2006, did not find an association between the program and either pregnancy or use of contraception in young women. (Darney et al., 2013) It is important to note that using cross-sectional data may introduce bias, since the treatment and control groups can only be matched on observable characteristics. By 2006, the point of analysis in the study by Darney *et al., Oportunidades* covered five million poor households. (Secretaría de Desarrollo Social, 2007) Any controls matched to participants on income would, therefore, be likely to differ on unmeasured characteristics that also prevented program participation, like living in remote location or refusing to enroll.

In contrast, Andalón used a regression discontinuity design with the 2003 ENCEL data to show a decrease in pregnancy among treated youth at the eligibility cutoff. (Andalón,

2011) This relationship was echoed in a 2009 dissertation, which found that, in urban ENCEL data through 2004, *Oportunidades* significantly delayed the onset of premarital sex and the birth of first children among adolescents. (Gulemetova-Swan, 2009)

In predicting how *Oportunidades* could affect fertility rates, it is essential to track how the program has influenced the proximate determinants of fertility, such as marriage rates and contraception. Using the eighteen-month randomized control data, one study found that the program led to modest increases in marital turnover and a significant rise in new unions among young, less-educated women. (Bobonis, 2011) (Families could be splitting and forming new households to increase benefits.) Using the full ten years of program data, on the other hand, Juan Pablo Gutierrez found that Oportunidades reduced risky sexual behavior in the adolescents who had been in the program longest. Fewer girls ages 14-18 reported having sex in the early treatment group than in the later treatment group, and there was an increased age of sexual debut. (Gutierrez, 2008, 2012) His analysis also detected a decrease in marriage rates among teen beneficiaries, as well as a delay in birth of the first child. (Bertozzi and Gutiérrez, 2013; Gutierrez, 2012; Gutierrez and Bertozzi, 2007) Although *Oportunidades* seems to have initially increased marriage rates, it decreased them in the long term.

Analysis of contraceptive use was included in the paper by Stecklov *et al*., who concluded that, in the eighteen month randomized period, contraceptive use among women in the early treated group went up 1.8%. (Stecklov et al., 2007) Feldman *et al.* found that women were more likely to use contraception in the 2000 ENCEL but not the 2003 survey. (Feldman et al., 2009) This result is contrary to the official external evaluation, which did find a significant increase in contraceptive use in both the 2000 and the 2003 ENCEL sample. (Hernández-Prado et al., 2005) Another study found heterogeneous effects of the program on contraceptive use – there was a large rise in family planning use by the poorest women in the treatment sample, but a small impact on those near the income threshold. (Lamadrid-Figueroa et al., 2010)

Given the strong theoretical and empiric evidence supporting conditional cash transfers as interventions that could increase family planning and reduce fertility, it is surprising that overall beneficiary fertility over the ten years of *Oportunidades* data has not been studied. Both the birth rates of *titulares* and their adolescent children may be affected, although existing research suggests that adolescents may experience the strongest decline in

pregnancy. My Master's thesis will address this gap in the literature by studying how *Oportunidades* has influenced fertility in these two groups.

REGRESSION DISCONTINUITY DESIGN

The short period of randomization presents a challenge in studying *Oportunidades*. More than ten years after the program was initiated, the original treatment and control groups differ only by eighteen months of benefits. In the absence of a long-term randomized control group, one impact evaluation method that can closely approximate a randomized design is regression discontinuity design, or RDD. (Gertler et al., 2011) This approach takes advantage of a built-in cutoff, or discontinuity, in benefit eligibility determined by some "forcing" or "running" variable. This forcing variable may be income, test scores, or some other measurable characteristic tied to program participation, like the *puntaje* score in *Oportunidades*. Using regression analysis, those who barely qualify for the program are compared to their peers who fall on the other side of the cutoff, under the assumption that the only significant difference between those two groups is program participation. Take, for example, a scholarship that is given to students with SAT scores over 1300. It is reasonable to assume that the students who score 1299 and those who score 1301 will not measurably differ, except for receipt of the scholarship. This method thus is a good estimator of the counterfactual for participants at the program cutoff.

Compared to other nonexperimental analysis methods, like instrumental variables or differences-in-differences, regression discontinuity has mild assumptions and thus provides more reliable estimates. (Lee and Lemieux, 2010) This quality is attributable to the random distribution of participants at the cutoff. As long as participants cannot control program assignment, baseline variables will be truly randomized in the region immediately to either side of the cutoff. For this reason, covariates should not alter outcomes if the baseline is indeed balanced.

There are two main types of RD designs: sharp and fuzzy. (Hahn et al., 2001) In a sharp design, the treatment variable is directly related to the discontinuity: participants are divided based on the cutoff. In a fuzzy design, the discontinuity does not necessarily designate who is treated. In the previous example, a fuzzy design would be used if certain students who fell below the cutoff were still given the scholarship – test scores (the discontinuity) do not directly determine treatment (the scholarship), although the probability of treatment jumps at the score cutoff.

Because participation in *Oportunidades* is tied to income, and data was collected on ineligible households living in treatment areas, a sharp RD design may be used. In this analysis, participants whose income score fell just under the cutoff are compared to those who were narrowly ineligible. This allows evaluation of the program's impact on a selected outcome. Buddelmeyer and Skoufias used the window of the randomized trial to test the performance of RDD against the *Oportunidades* data, ENCEL. In their calculation, RDD produced similar outcomes to the randomized control analysis (both cross-sectional difference and double difference) for several educational outcomes, emphasizing its usefulness in studying this program. (Buddelmeyer and Skoufias, 2003) For my Master's thesis, I will use regression discontinuity to study the changes in fertility among *Oportunidades* recipients in the ENCEL datasets.

CONCLUSION

Increased availability of family planning has resulted in a worldwide drop in fertility over the last forty years. This decline in birthrates is associated with improved maternal and child health outcomes, increased women's education, and economic development. Still, unmet need for family planning remains high globally. Mexico provides an informative case study: in the last decade, total fertility rate has dropped below replacement level, but teen pregnancy has increased. Innovative strategies are necessary to address this paradoxical trend.

If childbearing is modeled in an economic framework, decisions to become pregnant are based on a couple's demand for additional births. This demand is affected by perceived costs and benefits of delaying fertility, including, for adolescents, attitudes towards education and beliefs about future opportunity. Teens who perceive they have limited economic prospects may become parents earlier than their peers with higher future expectations. Additionally, research shows that, when income increases, parents often respond by having fewer children.

The relationship between increased income and lower fertility is well-established in theory, but few interventions target this link. One antipoverty strategy that could reduce family size is conditional cash transfer (CCT) programs. These policies, which provide cash benefits in exchange for family participation in health and education, reduce both immediate and future poverty. Through raising household income and promoting adolescent schooling, CCT programs are well-structured to influence fertility rates.

Mexico's CCT program, *Oportunidades*, was one of the first of its kind. Although there has been some research on short-term effects of the program on pregnancy rates, this research has either analyzed too short a time period to see true fertility changes, or has used cross-sectional analysis in national surveys without a strong comparison group. To date, no study has investigated the effect of *Oportunidades* on fertility, either in all beneficiaries or in adolescents, through the end of the ten-year evaluation period. My Master's research will fill this gap by analyzing birth rates in both direct program beneficiaries and their teen daughters.

Due to the narrow window of the *Oportunidades* sampling, it is not possible to use the randomized early and late control groups to detect all fertility effects. Instead, I will take

advantage of the data collected on the just-ineligibles, as well as the discrete program cutoff, to perform a regression discontinuity design. This scheme will compare program participants who barely qualify with those making just enough more to disqualify. In this way, I will have a robust counterfactual for analysis.

PART II: HOW DOES INCOME AFFECT FERTILITY? AN ANALYSIS OF OPORTUNIDADES, MEXICO'S CONDITIONAL CASH TRANSFER PROGRAM

BACKGROUND

Over the last fifty years, improved contraceptive technology, government family planning programs, and economic development have prompted a worldwide decline in total fertility rates.[6] Today, forty-six percent of the world's population lives in a country with below-replacement fertility levels. (United Nations Department of Economic and Social Affairs, Population Division, 2015) An observed negative relationship between income and fertility has persisted throughout this decline. In the context of cross-sectional evidence, the direction of the causal relationship between wealth and family size is unclear. (Schultz, 2005) Does increased personal income lead families to have fewer children, or, conversely, do smaller family sizes spur economic development?

To answer this question, it is essential to consider the different factors that influence fertility. The classic demographic approach considers fertility to be influenced by eight "proximate determinants," as John Bongaarts established in 1978. (Bongaarts, 1978, 1982) Six of the eight proximate determinants are modifiable through behavior change: marriage rates, contraceptive use and effectiveness, induced abortion, postpartum infecundity through lactational amenorrhea, and frequency of intercourse. The last three, sterility, spontaneous abortion (miscarriage), and duration of fertile period of the menstrual cycle, are biologically determined. The way a couple approaches these proximate determinants will depend on distal socioeconomic factors, such as religion, access to healthcare, and education. (Figure 1)

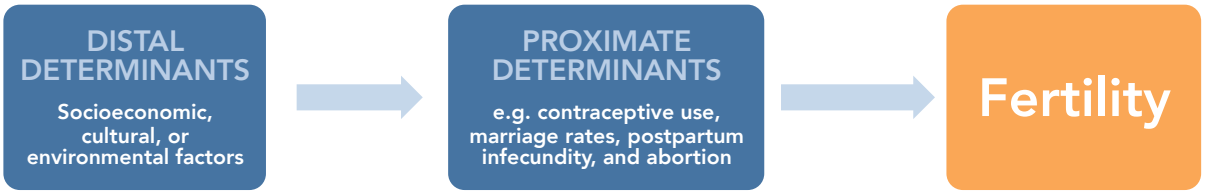

Figure 1: Distal and Proximate Determinants of Fertility – Demographic Model

[6] The average number of children per woman over her reproductive life if she experiences the average birth rates over every year of her lifetime.

To understand the effects of market forces as distal determinants of fertility, economists frame fertility as a resource-constrained choice. (Becker, 1960; Schultz, 2005) In this scheme, couples have a certain natural reproductive capability, *i.e.* "supply." By changing behavior in regard to proximate determinants such as contraception or abortion, a couple can achieve their desired number of births, or "demand" for children. (Schultz, 2005)

Using these principles, fertility decisions can be modeled in a utility-maximization framework. One basic model considers that parents will maximize their utility given a budget constraint, a time constraint, and a production function for children. (Jones et al., 2008) This production function allows biological influences on fertility to be linked formally to the costs of having children based on observable factors like prices and wages, as well as on unobservable preferences for family size. This model can thus predict how fluctuations in an economic environment will impact fertility behavior. An in-depth analysis of current economic theories of fertility can be found in Jones et al., 2008.

How would increased income affect decisions regarding childbearing? Beginning in the 1960s and 1970s, researchers developed economic models to explain empiric evidence of a negative relationship between family size and income. From an economic standpoint, this association is counterintuitive: if children are a normal good, richer people should have larger families. One proposed theory predicts that, with more money, parents will invest more in child "quality" — allocating more resources to existing children — over an increase in child "quantity." (Becker, 1960; Becker and Lewis, 1973) Studying the substitution of child quality for quantity empirically has been difficult, because socioeconomic factors often confound the relationship between family size and measures of quality, like educational attainment.

Several levels of support are needed to prove that reduced birth rates among wealthier families are attributable to increases in child investment. First, evidence must show that child quality and quantity can truly function as substitutes for one another. Several studies across populations in the United States, Europe, and Asia have demonstrated that parents do treat the two as substitutes. That is, parents with exogenous increases in fertility (like having twins) invest less in each child. (Rosenzweig and Wolpin, 1980; Conley, 2004; Black et al., 2005; Li et al., 2008)

Still, these studies do not show how quantity and quality vary by income. To this point, extensive evidence establishes a population-level association between wealth and smaller family sizes across time periods and geographies. (Jones et al., 2008) This existing research, however, is generally limited to cross-sectional, population-level data. Thus, the direction of the causal link between wealth and a shift from child quantity to child quality remains theoretical.

Income can also affect family size by changing the opportunity cost of childbearing. If improved education and employment opportunities for women create additional household resources, it will become relatively costly to raise children instead of participating in labor markets. (Schultz, 2005) Based on the utility-maximization function described above, we would expect this rise in time cost to exert a substitution effect away from childbearing. The evidence supports this phenomenon; empirically, the economic factors with the greatest influence on fertility decline are a rise in the value of women's time and a drop in contraceptive costs. (Gertler and Molyneaux, 1994)

Antipoverty programs that increase household wealth offer an opportunity for researchers to establish a causal relationship between income and birth rates. Unfortunately, these large-scale government programs are rarely implemented in a way that adequately controls for other demographic factors that could affect family size, such as education and access to services. To avoid such confounding, this paper takes advantage of income eligibility cutoffs in *Oportunidades*, Mexico's conditional cash transfer program, to approximate a randomized experiment through regression discontinuity analysis. This design isolates an income effect on fertility by comparing program participants on either side of an eligibility cutoff.

I. OPORTUNIDADES AND CCT PROGRAMS

Oportunidades, originally called *Progresa* (and recently rebranded as *Prospera*), was one of the first large scale conditional cash transfer (CCT) programs in the world. These welfare measures are designed to halt the cycle of intergenerational transfer of poverty. To achieve this goal, CCT programs distribute cash benefits to poor households contingent on investment in human capital, such as child health screenings or education. (Fiszbein et al., 2009) This demand-side intervention relies on pre-existing local infrastructure such as health clinics and schools. By combining short-term financial benefits with investments in social

development, CCT programs aim to reduce both present and future poverty. Today, countries on nearly every continent have adopted CCT initiatives.

Due to limited resources, *Oportunidades* was initially implemented as a randomized control trial. Program administrators selected 506 communities for early participation. These communities were randomized to either an early-intervention group or a late-intervention group. To identify qualifying households within these localities, the Mexican government surveyed poor households on a variety of characteristics. These results were weighted and compiled into a single score, or *puntaje*. This measure of poverty takes into account dwelling characteristics, like dirt floor, and household characteristics, like number of young children. Notably, 80% of the households in these early communities qualified for benefits. (Coady and Parker, 2009) The few whose *puntaje* were too high to qualify were also included in all survey rounds.

Once enrolled, households received three benefits.[7] First, every other month, participants collected cash transfers, the amounts of which were designed to increase household income by 20-30%, regardless of the number of children in the family. The *titulares*, or transfer recipients, were the female heads of the households. Next, families received educational scholarships for children in third grade through high school, which increased in value for the groups most vulnerable to dropout (female and older children). Finally, pregnant and lactating women, children 6-24 months, and children 2-4 with low bodyweights (as identified in health care visits) received milk-based food supplements.

In exchange for these benefits, program participants had to adhere to both educational and health requirements. All children had to attend school and maintain a minimum monthly attendance of 85%. Students also received a bonus for finishing high school. (Levy, 2006) All members of the family were required to complete preventative health care visits, and those older than 15 had to attend *plácticas*, health education talks. Since the program's inception, only about 1% of households have been denied benefits because of noncompliance. (Fernald et al., 2009)

As of the beginning of 2015, more than 6 million Mexican households – about 26 million individuals – were *Oportunidades* beneficiaries. (Secretaría de Desarrollo Social, 2014)

[7] Benefits are described as they were at the program's inception in 1997; some elements have been since modified.

After ten years, *Oportunidades* showed promising results in the domains of economics, education, and child health and development. (Gertler et al., 2012)

II. Fertility Impacts of Oportunidades

Welfare programs inevitably raise questions about whether they indirectly motivate families to maximize benefits by having more children. *Oportunidades* is no different. *Oportunidades* was specifically designed to minimize pro-natalist incentives; families received a fixed stream of cash that did not increase when children were born. Instead of providing more income, additional children imposed further conditions, as they, too, were subject to the health and education requirements. Finally, education grants, which did vary based on number of children, did not go into effect until third grade: families had to wait nine years after a birth to see their education subsidy level increase. (Stecklov et al., 2007)

If these incentives are properly controlled, economic theory predicts that the program will decrease birth rates. First, the program may exert an income effect on fertility, as discussed in the previous section. Also, because the education and health conditions are tied to receipt of benefits, they reduce the price of investing in current children. This may exert a substitution effect on birth rates, as parents shift resources from child quantity to quality. Finally, family planning is one of the topics of the *plácticas*. As participants become better-informed about family planning, fertility may decrease.

Current research, however, shows that *Oportunidades* has had little impact on birth rates. Using data from the first eighteen months of the program, Stecklov et al. found no effect on fertility: the probability of being currently pregnant or having a child was 0.3% lower in the treatment communities than in the controls at the end of the initial sampling period (p=0.852). (Stecklov et al., 2007) Schultz analyzed this early data again in 2004, with similar results: *Oportunidades* was not associated with changes in fertility among beneficiary adults or adolescents. (Schultz, 2004) Next, there was no programmatic effect on birth spacing in either the 2000 or 2003 evaluation data. (In 2003, birth spacing was 28.9 months in beneficiaries and 28.3 months in controls.) (Feldman et al., 2009) Finally, another study, intended to evaluate alternative program designs, used a dynamic behavioral model to predict that *Oportunidades* would not affect lifetime fertility rates. (Todd and Wolpin, 2006) No published study, however, tracks the impact of *Oportunidades* on fertility over the ten-year evaluation period, 1997-2007.

The impact of *Oportunidades* on births to teens in particular has also been inconclusive. Darney et al., using a cross-sectional national survey from 2006, did not find an association between the program and either pregnancy or use of contraception in young women. (Darney et al., 2013) It is important to note that using cross-sectional data may introduce bias, since the treatment and control groups can be matched only on observable characteristics. By 2006, the point of analysis in the study by Darney et al., *Oportunidades* covered five million poor households. (Secretaría de Desarrollo Social, 2007) Any controls matched to participants on income would, therefore, likely differ on unmeasured characteristics that also prevented program participation, such as living in a remote location or refusing to enroll.

In predicting how *Oportunidades* could affect fertility rates, it is essential to track how the program has influenced the proximate determinants of fertility. Currently, there is evidence of *Oportunidades's* impact on marriage rates on adolescent and young adult beneficiaries only. Using the eighteen-month randomized control data, for example, one study found that the program led to modest increases in marital turnover and a significant rise in new unions among young, less-educated women. (Bobonis, 2011) This rise in new unions may be attributable to families splitting and forming new households obtain new benefits. However, after ten years of implementation, *Oportunidades* is associated with a decrease in marriage rates among teens 14-19 (5% in most-exposed group vs. 14.9% in never-exposed). (Gutierrez, 2012)

There is mixed evidence of the impact of *Oportunidades* on another proximate determinant of fertility, contraceptive use. The analysis by Stecklov *et al.* concluded that, in the eighteen month randomized period, contraceptive use among women in the early treated group increased 1.8%. (Stecklov et al., 2007) Feldman et al. found that women were more likely to use contraception in the 2000 survey (change in log odds of using modern contraception = 0.16; $p < 0.05$), but not the 2003 survey (change in log odds =-0.16; $p > 0.05$) . (Feldman et al., 2009) This result is contrary to the official external evaluation, which did find an increase in modern contraceptive use in the treatment group in the 2003 sample (17.23% increased use; $p < 0.05$). (Hernández-Prado et al., 2005) Another study found heterogeneous effects of the program on contraceptive use – there was a large rise in family planning use by the poorest women in the treatment sample, but a small impact on those near the income threshold. (Lamadrid-Figueroa et al., 2010)

In sum, the evidence for the impact of *Oportunidades* on fertility rates is contradictory and incomplete. Although there has been some research on the short-term effects of the program on pregnancy rates, this research has either analyzed too short a time period to see true fertility changes, or has used cross-sectional analysis in national surveys without a strong comparison group. To date, no study has investigated the effect of *Oportunidades* on fertility through the end of the ten-year evaluation period. These data represent a rare opportunity to evaluate how a decade of rising income directly affects family size.

DATA AND METHODS

I. DATA COLLECTION

The Instituto Nacional de Salud Pública performed extensive evaluations on both early and late intervention groups, as well as on the few households that were too wealthy to be eligible for the program. (These surveys are referred to as ENCEL; separate evaluations were performed on rural and urban communities.) To compensate for the loss of the control group in 1998 (when control households began receiving benefits), additional communities were added to the sample in the 2003 evaluation round. These communities were chosen to match socioeconomic distribution of existing *Oportunidades* communities. Although the evaluation team administered the last ENCEL survey in 2009, other evaluation of *Oportunidades* continues.

II. SAMPLE

The study sample was limited to those households from the original rural 1997 communities (ENCASEH 1997) that were surveyed in 2007, including both treatment and control communities. Neither nonrandom controls added after 2003 nor urban participants were included in the analysis, as their program implementation significantly differed from the original sample. Similarly, those whose poverty status had changed between 1997 and 2007 were excluded from analysis. This sampling frame limits results to long term effects and minimizes eligibility and benefit heterogeneity.

The sample was further limited to participants who completed the reproductive health survey module in 2007. This reproductive health module was part of a longer ENCEL survey offered to certain groups, and contained questions on birth history, contraceptive use, and family planning resources. All participants from the original sample who lived in locations with more than 20 households were offered the longer ENCEL survey with reproductive health questions.[8] Once offered the survey, only women between the ages of 18 and 49 who were either the head of household or the spouse of the head of household completed the reproductive health module. (Figure 2)

[8] Participants incorporated later had a <1 probability of receiving a long survey; those in the original sample had a probability of 1.

In analysis, those who had received benefits for eight to ten years were compared to those who were ineligible all ten years.[9] Poverty status was used as proxy for program participation, because all original participants whose *puntaje* qualified them as poor should have received benefits by 1999. The following analysis, therefore, gives intent-to-treat estimates, since not all poor households may have actually received all benefits. These two comparison groups will be referred to as "poor" (puntaje score qualifying for benefits in 1997 and 2007) or "nonpoor" (not qualified for benefits in 1997 or 2007). Finally, baseline variables on family size in 1997 were constructed using data gathered in 2007.

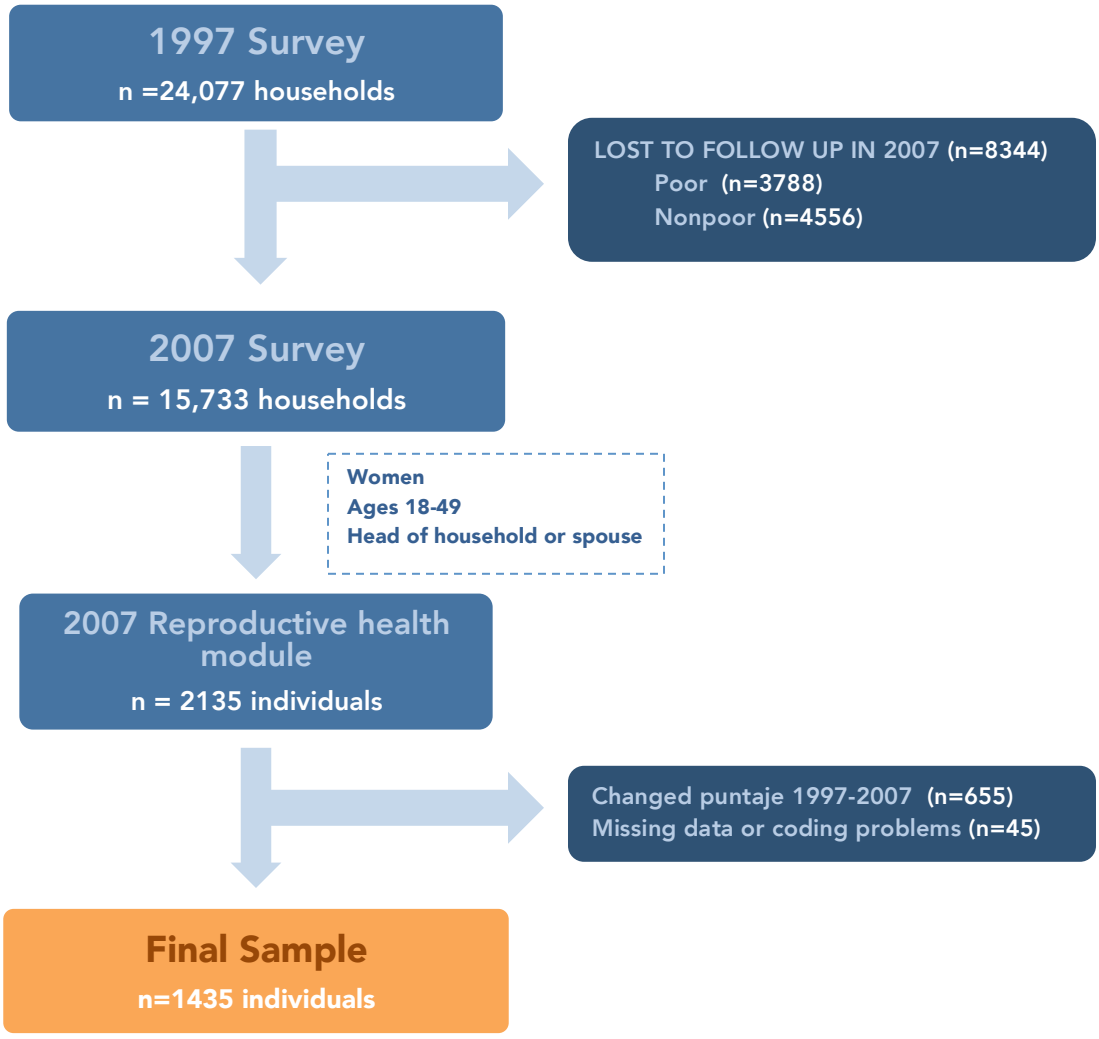

Figure 2: Sample selection

[9] This two-year differential in program participation stems from the delayed participation of the original control group.

III. Regression Discontinuity Design

The short period of randomization presents a challenge in studying *Oportunidades*. More than ten years after the program was initiated, the original treatment and control groups differ only by eighteen months of benefits. In the absence of a long-term randomized control group, one impact evaluation method that can closely approximate a randomized design is regression discontinuity design, or RDD. (Gertler et al., 2011) This approach takes advantage of a built-in cutoff, or discontinuity, in benefit eligibility determined by some "forcing" or "running" variable. This forcing variable may be income, test scores, or some other measurable characteristic tied to program participation, like the *puntaje* score in *Oportunidades*. Using regression analysis, those who barely qualify for the program are compared to their peers who fall on the other side of the cutoff.

Compared to other nonexperimental analysis methods, like instrumental variables or differences-in-differences, regression discontinuity has mild assumptions and thus provides more reliable estimates. (Lee and Lemieux, 2010) This quality is attributable to the random distribution of participants at the cutoff. As long as participants cannot control program assignment, baseline variables will be truly randomized in the region immediately to either side of the cutoff. For this reason, covariates should not alter outcomes if the baseline is indeed balanced.

IV. Estimation

Regression discontinuity estimates can be considered a weighted average treatment effect, with weights being the probability the individual will be near the cutoff. (Lee and Lemieux, 2010) A flexible parametric model allows use of all data, although it is customary also to perform a local linear regression within a limited bandwidth of data to verify robustness of results. This model takes the following form:

$$Y_i = \alpha + \beta D_i + \sum_{d=1}^{n} \gamma \, (X_l)^n + \sum_{d=1}^{n} \delta \, (X_r)^n + \rho F_i + \sigma F_i * D_i + \varepsilon_i$$

Where D_i is a dummy variable for program participation, X is the poverty score (the running variable) either to the left (X_l) or to the right (X_r) of the cutoff, F_i is family size at baseline,

and β is the program effect. An interaction term for number of baseline children and program participation was also included, since larger families in the program earned more benefits through educational grants. Results were clustered by village, the unit of randomization in the original sample.

RESULTS

First, the number of births from 1997 to 2007 was graphed in binned local averages along *puntaje*.[10] A 4th order global polynomial was fitted on either side of the eligibility cutoff (*puntaje* = 0). This graph was constructed with the Stata command rdplot, as described in Calonico, Cattaneo, and Titiunik (2015). (Figure 3)

Demographic variables in 2007 for ten-year poor and non-poor groups are in Table 1. Women who qualified for benefits were less educated: only 8.62% in the poor group completed secondary school or above, compared to 27.91% of the nonpoor group. A larger proportion identified as indigenous, compared to those too wealthy to receive benefits (25.45% vs. 7.83%). Poor women had slightly fewer abortions than non-poor ones, although the difference was minimal. Age, marital status, and contraceptive use were similar between the two groups. The poor group had, on average, more children than the non-poor group (5.51 children vs. 3.12). This trend was echoed in desired number of children.

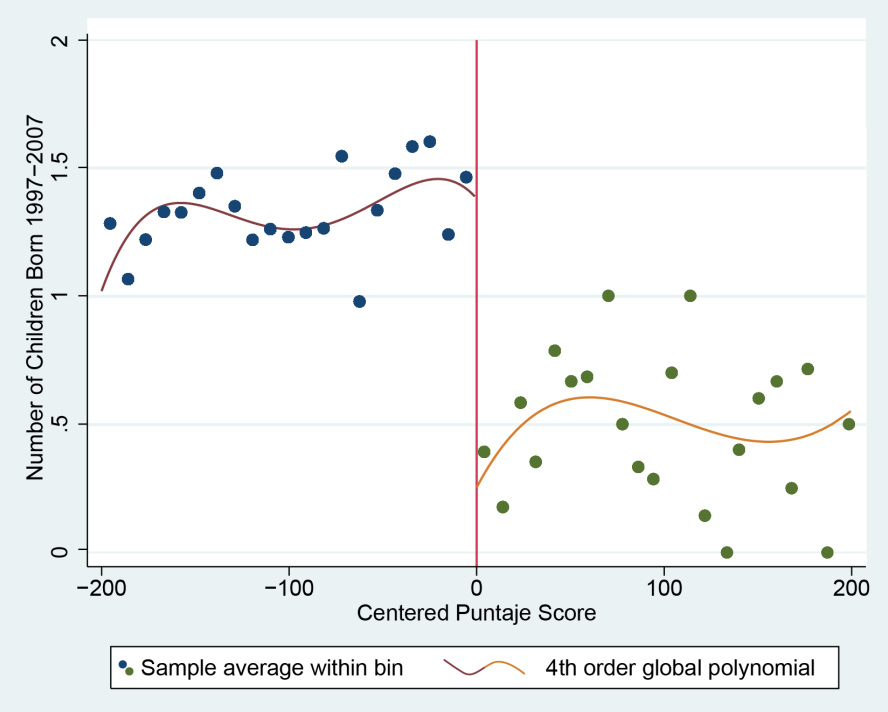

Figure 3: Binned Averages of 10 Year Fertility by Poverty Score (*puntaje)*

[10] Bin size was chosen to mimic underlying variance of the data as described in Calonico, Cattaneo, and Titiunik (2015). Final bin size was 24 households to the left of the cutoff and 22 to the right.

Table 1. Demographic Variables in 2007

		Nonpoor	Poor
Age	16-19	0% (0)	0.08% (1)
	20-29	5.07% (11)	5.67% (69)
	30-39	41.01% (89)	44.42% (541)
	40-49	53.92% (117)	49.84% (607)
Education	None	6.05% (13)	19.59% (236)
	Preschool	0.47% (1)	0.08% (1)
	Primary	65.58% (141)	71.70% (864)
	Secondary	21.40% (46)	8.46% (102)
	Preparatory	1.86% (4)	0.08% (1)
	Higher	4.65% (10)	0.08% (1)
Indigenous Status	Yes	7.83% (17)	25.45% (310)
	No	92.17% (200)	74.55% (908)
Marital Status	Married	84.33% (183)	84.32% (1027)
	Civil Union	12.44% (27)	12.40% (151)
	Widow	0.92% (2)	1.72% (21)
	Separated	0.46% (1)	0.99% (12)
	Divorced	0% (0)	0.16% (2)
	Single	1.85% (4)	0.41% (5)
Abortions	0	75.94% (161)	80.40% (972)
	1	18.87% (40)	13.98% (169)
	2+	5.19% (11)	5.62% (68)
Contraceptive Use	Yes	37.40% (49)	37.81% (304)
	No	62.60% (82)	62.19% (500)
Number of Children	*Mean*	3.12	5.51
	0	3.23% (7)	0.74% (9)
	1	11.52% (25)	1.07% (13)
	2	27.19% (59)	5.26% (64)
	3	24.42% (53)	12.49% (152)
	4	13.82% (30)	16.52% (201)
	5+	19.82% (43)	63.93% (778)
Number of Desired Children	*Mean*	3.64	5.27
	0	6.34% (13)	5.58% (62)
	1	1.95% (4)	0.27% (3)
	2	17.56% (36)	4.41% (49)
	3	26.83% (55)	12.15% (135)
	4	22.93% (47)	19.98 (222)
	5+	24.39% (50)	57.61% (640)
	Total*	217	1218

*cells may not sum to total due to missing data

Table 2. Program Effects on Fertility (1997-2007)

Model Specification		Birth since 1997 - Dummy				Number of children born since 1997			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Full	0.234	3.23	0.001	0.091 – 0.376	0.999†	6.71	0.000	0.705 – 1.293
2	Full	0.221	2.71	0.007	0.060 – 0.382	0.926	5.94	0.000	0.618 – 1.233
3	Full	0.391†	4.58	0.000	0.226 – 0.560	1.225	7.98	0.000	0.922 – 1.528
preferred	Limited	0.506	4.59	0.000	0.289 – 0.724	1.049	6.75	0.000	0.743 – 1.356

Regressions clustered at village
† Preferred AIC specifications

To assess differential attrition, the percent of each comparison group lost to follow up was tested with a two-sample test of proportions. 0.394 of the nonpoor group was lost to follow up (95% CI: 0.385 – 0.403), and 0.303 of the poor group was lost to follow up (95% CI: 0.295 – 0.311). The difference in attrition was significant ($p = 0.000$).

Coefficients for linear regressions of program participation on the number of children born from 1997-2007 and on a dummy variable for births are in Table 2. Each outcome was modeled with varying polynomial orders.[11] Each polynomial model was tested using the Akaike information criterion (AIC); preferred polynomial specifications are marked. These preferred polynomial specifications were then tested with limited bandwidth. For the dummy variable for birth 1997-2007, the coefficient of the preferred model (third-order) was 0.391 ($p = 0.000$); for number of children since 1997, the preferred coefficient (first-order) was 0.999 ($p = 0.000$). Results were similar for limited bandwidth (0.506 and 1.049, respectively).

Similar analyses were conducted for selected proximate determinant variables (current contraceptive use, current marriage, and ever had an abortion) in Table 3. Again, preferred models are marked. No variable was widely different between the two groups in 2007. (Note that it is not possible to track changes in proximate determinants from baseline in 1997, since individual-level identifications were changed from the 1997 to 2007 data.)

[11] This bandwidth limited puantaje between -200 and 200.

Table 3. Program Effects on Proximate Determinants of Fertility (1997-2007)

Model Specification		**Current Contraceptive Use**			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Full	0.019†	0.21	0.837	-0.163 – 0.201
2	Full	-.0739	-0.64	0.523	-0.302 – 0.154
3	Full	-0.091	-0.61	0.541	-0.385 – 0.202
1	Limited	-0.104	-0.93	0.354	-0.324 – 0.117
Model Specification		**Currently Married**			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Full	0.007	0.18	0.853	-0.063 – 0.076
2	Full	0.012	0.33	0.740	-0.058 – 0.082
3	Full	0.000†	-0.00	0.997	-0.070 – 0.070
3	Limited	-0.058	-1.72	0.087	-0.125 – -0.009
Model Specification		**Ever Had Abortion**			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Full	-0.048†	-0.62	0.538	-0.203 – 0.106
2	Full	-0.047	-0.52	0.441	-0.023 – 0.053
3	Full	-0.007	-0.06	0.955	-0.240 – 0.227
1	Limited	-0.098	-1.06	0.292	-0.282 – 0.085

Regressions clustered at village
† Preferred AIC specifications

In regression discontinuity, covariates should be adequately distributed around the cutoff, and they are not typically included in regression models. To check that covariates were indeed adequately controlled, they were added to the preferred, limited model from Table 2. (This method is discussed at length in Lee and Lemieux, 2010.) The coefficient of the poor-nonpoor dummy variable was slightly reduced (0.846; $p = 0.000$), although no covariates were statistically significant in the model. (Table 4) Shaded variables indicate covariates that were not included in earlier models.

Table 4. Children Since 1997 - With Demographic Controls

Variable	*Coefficient*	*t-test*	*p-value*	*95% CI*
Poverty Dummy	0.846	5.91	0.000	0.564 – 1.129
Total Children in 1997	0.033	1.34	0.183	-0.016 – 0.081
Kids in 97*Program	-0.027	-1.01	0.315	-0.813 – 0.026
Age	-0.123	-1.70	0.090	-0.267 – 0.020
Age Squared	0.000	0.50	0.617	-0.001 – 0.002
Education Level	-0.064	-1.33	0.184	-0.139 – 0.027
Indigenous Status	0.158	1.24	0.216	-0.075 – 0.330

Regressions clustered at village
† Preferred AIC specifications
Shading indicates new variables

The desired number of children, as reported in 2007, was higher among poor women in the preferred, full-data model (0.937; $p = 0.001$). This result was slightly less significant in models that included higher-order polynomials or with limited bandwidth. The difference between desired and actual number of children was also significantly different between the two groups in the preferred model (-0.675; $p = 0.022$), although this result was less robust to model selection.

Table 5. Number of Desired Children (2007)

Model Specification		**Number of Desired Children**			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Full	0.937†	3.48	0.001	0.407 – 1.468
2	Full	0.699	2.28	0.024	0.094 – 1.305
3	Full	0.668	1.72	0.087	-0.098 – 1.434
1	Limited	0.755	2.55	0.012	0.171 – 1.340
Model Specification		**Difference Between Desired and Actual Children**			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Full	-0.675†	-2.31	0.022	-1.250 – -0.098
2	Full	-0.764	-2.10	0.037	-1.482 – -0.046
3	Full	-0.561	-1.24	0.217	-1.456 – 0.333
1	Limited	-0.679	-1.97	0.051	-1.362 – 0.003

Regressions clustered at village
† Preferred AIC specifications

Table 6. Baseline Balance

Model Specification		**Number of children in 1997**			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Limited	0.280	1.03	0.306	-0.258 – 0.818
Model Specification		**Had children in 1997**			
Polynomial Order	*Bandwidth*	*Coefficient*	*t-test*	*p-value*	*95% CI*
1	Limited	-0.045	-1.64	0.103	-0.100 – 0.009

Regressions clustered at village
† Preferred AIC specifications

For regression discontinuity estimates to be internally valid, baseline covariates must truly be randomly distributed around the cutoff. To test the baseline balance of family size, linear regressions were performed on number of children in 1997 and a dummy for having children at baseline. These tests were performed with a limited bandwidth and a first-order polynomial. (Table 6) Neither variable was significantly different between the two groups in 1997. A graph of this finding can be found in Figure 4.

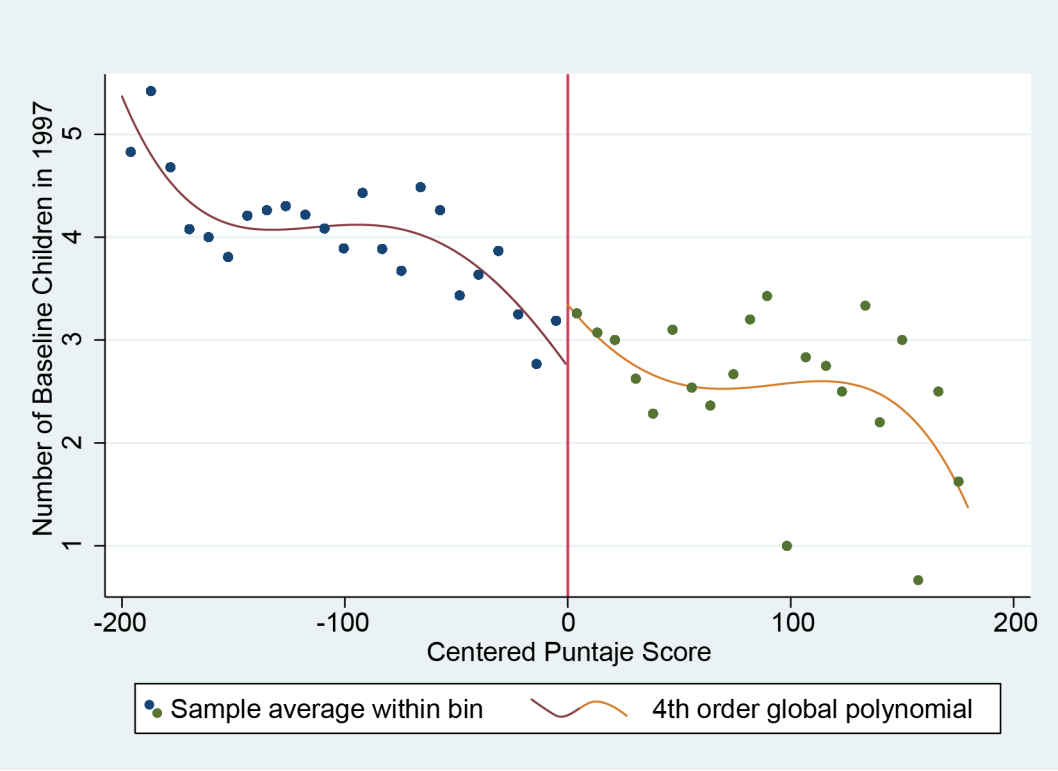

Figure 4: Binned Averages of Baseline Fertility by Poverty Score (*puntaje)*

DISCUSSION

Women who received *Oportunidades* benefits for eight to ten years had significantly more children in that period of time than those who did not receive benefits. These results are robust to model specification and inclusion of covariates. This surprising result confirms the preliminary research of Arenas, Parker, Rubalcava, and Teruel presented at the 2015 Population Association of America Meeting. (Arenas et al., 2015) Their estimates, which used difference-in-difference analysis in a cross-sectional sample (the Mexican Family Life Survey), found that the group that received *Oportunidades* the earliest had 0.15-0.2 more children from baseline than those whose villages had been incorporated into the program after 2004. This similar finding in a different sample confirms the robustness of the positive relationship between *Oportunidades* and fertility.

Although proximate determinants such as contraceptive use, marriage, and abortions were not significantly different between the two groups, the available data give only a snapshot of behavior in one moment in time, 2007. Data limitations make it impossible to compare change in individual behavior over the sampling period. (In contrast, the 2007 dataset contains the date of every birth, allowing for inferred baseline information on birth variables.) Contraceptive use, for example, is especially close between the two groups (37.4% in the nonpoor group and 37.8% in the poor; see Table 1). It is possible that *Oportunidades* changed participants' contraceptive use from baseline, leveling rates between the two groups in 2007.

What distal factors could have prompted the behavior change that underlies increased fertility? First, despite the built-in programmatic safeguards against incentivizing larger family sizes, the program could have indirectly promoted fertility. If so, this paper suggests that neither the delay between birth and benefits nor the burden of additional requirements for subsequent children were sufficient to counter the financial reward of a larger family. The only cash benefit in *Oportunidades* that increased with additional children was educational subsidies; making all benefits invariable with family size may remove this fertility incentive. Additionally, *Oportunidades* may have incentivized the splitting of existing households to establish a new benefit stream. Splitting from an original household may have increased reproductive opportunities or desired family size for younger adults formerly living with family.

Alternatively, the *Oportunidades* cash benefits may have allowed parents to achieve a higher desired number of children. Poverty may have reduced family size below beneficiaries' ideal. When household income increased under a CCT program, parents could, for the first time, afford to complete their families. The desired number of children was significantly different between the 10-year *Oportunidades* group and the ineligibles around the cutoff, indicating that the program itself may have inflated participants' ideal family size. However, on average, the poor group had more children than their desired number compared to the nonpoor group (see table 5). It is possible that the program exerted an initial income effect on fertility, leading to an early increase in family size, but decreased the desired number of children long term. Thus, by 2007, family size and desired number of children were no longer congruent.

The above theories are all based on alterations in parental demand for children. *Oportunidades*, however, could have impacted the natural supply of fecundity through improved health of beneficiaries. Although the program aimed to improve nutrition in children, *Oportunidades* also had significant health effects for adults. Participants experienced decreased obesity and hypertension, and improved self-reported health. (Andalón, 2011; Fernald et al., 2008) The program may have conferred these health benefits through several mechanisms. The cash transfers may have improved the ability of households to afford more nutritious foods, or participants may have learned about healthy behaviors through attending *plácticas*. Maternal health, including BMI, is known to impact both maternal and fetal outcomes. (Scott-Pillai et al., 2013) Further, obese women are more likely to have anovulatory cycles, and, even when regularly ovulating, they have lower levels of spontaneous pregnancy than their thinner peers. (van der Steeg et al., 2008) Thus, as *Oportunidades* improved beneficiary health, it may have made its beneficiaries more fertile.

Finally, it is important to note that some economists have posited that the apparent negative association between fertility and income is driven by women's wages. (Jones et al., 2008) An increase in the monetary value of a woman's time makes childbearing a comparatively costly endeavor. In contrast, limited data suggest that the relationship between paternal earnings and family size may actually be positive. (Jones et al., 2008) Even though *Oportunidades* provided cash benefits to the female head of household, it may not have affected women's labor market wages. The positive relationship between cash transfers

and family size may therefore reflect that the benefit behaves more like men's wages in its impact on reproductive behavior.

There are several limitations to this analysis. Most importantly, the sample excluded those who were lost of follow-up in 2007. The poor and nonpoor group had significantly different levels of attrition, with more poor participants lost from the sample. This has serious implications for the internal validity of these results. This analysis could overstate impact estimates if those who dropped out of the poor group were different than those who stayed. Households in which migration of a male partner caused loss to follow up, for example, may have a lower fertility rate than those with a male living at home. Further research is needed on the characteristics of those lost to follow up to ensure that the sample used in this analysis is not biased.

Next, the sample was restricted to those who had completed the reproductive health module of the survey. Not every household had a participant complete the reproductive health module. This information was gathered only if the surveyed person happened to fulfill certain criteria (female, 18-49, head of household or spouse). This is not a systematic method of sample selection; the households in which a woman did not complete the survey may have been different than those who did. Additionally, this intent-to-treat analysis excludes spillover effects. It is possible that the introduction of *Oportunidades* in a community improved socioeconomic status or changed behavior in all residents, regardless of program participation. If this is the case, the results presented in this paper represent an underestimation of the true effect on fertility. Regardless of limitations, however, the Arenas *et al.* study confirms the upward fertility trend among participants in *Oportunidades*.

CONCLUSION

Mexico is often promoted as a model for fertility transitions: its total fertility rate dropped from more than 5 children in 1975-1980 to 2.29 children in 2010-2015, and total fertility is projected to drop below 2 children per woman by 2030. (United Nations Department of Economic and Social Affairs, Population Division, 2015) This trend in fertility rate, however, may be stalling among certain populations; newer data suggest that birthrates in adolescents may actually be rising. (Instituto Nacional de Salud Pública, 2012) If *Oportunidades*, a program that serves almost 20% of the Mexican people, truly incentivizes births, Mexico may have to modify the program or confront a rising birthrate among the poorest.

The benefits of smaller families on poverty alleviation, maternal and child mortality, and female empowerment are well documented. (Cleland et al., 2006) Poorer families influenced by pro-natalist incentives in CCT programs would be excluded from these effects. Additionally, an increase in births among beneficiaries may strain the very public institutions on which CCT programs rely to break the intergenerational cycle of poverty. As CCT programs expand worldwide, the benefits of a global declining birthrate may become a privilege enjoyed by the wealthy. Paradoxically, the fertility effects of conditional cash transfer programs may jeopardize the ability of participating families to escape poverty.

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