Empowering Women: How Mexico's Conditional Cash Transfer Program Raised Prenatal Care Quality and Birth Weight

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Abstract: We use data from a controlled randomized trial to estimate the effect of Mexico's Conditional Cash Transfer Program, OPORTUNIDADES, on birth outcomes, and examine the pathways by which it works. We estimate that the birth weight of beneficiaries are on average 127.3-grams higher than non-beneficiaries and that the incidence of low birth weight is 44.5 percent lower among beneficiaries. We also find that the improvement in birth outcomes is entirely explained by better quality of prenatal care. *OPORTUNIDADES* affected quality through empowering women to insist on better care by informing them of what content to expect, and by giving them skills and social support to negotiate better care from healthcare providers. The broader policy implication is that efforts to empower the less-well off are necessary for public services to fully benefit the poor.

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I. Introduction

Low birth weight is a major problem, especially in poor populations. Among the 20 million low birth-weight infants born annually, over 95 percent occur in less developed nations (WHO 2004). Reducing low birth weight is a global health priority because of its consequences on neonatal and infant mortality (McCormick 1985), morbidity and mortality during childhood and adolescence (Ashworth 1998, and Moore et al 1999), adult chronic conditions (Prentice et al 2005), and long-term economic productivity (Alderman and Berhman 2006). Recommended interventions to reduce low birth weight include increasing prenatal care utilization, improving the quality of prenatal care, and addressing maternal nutritional deficiencies (Institute of Medicine 1985, Alexander and Korenbrot 1985, Merialdi et al 2003, Zulfiqar et al 2005).

One program with huge potential to improve birth weight is Conditional Cash Transfers (CCT). Many governments have turned to conditional cash transfer programs (CCT) as a means of improving the health and schooling of children born into poor families (Lagarde et al 2007). In general, CCT programs use money as an incentive for parents to invest in their children's human capital enabling their children to have the capabilities to escape poverty when they reach adulthood. With respect to maternal and child health, CCTs typically condition the cash transfer to families on obtaining prenatal care, and on participating in classes that educate mothers about prenatal care and proper nutrition as well as encourage them to demand proper prenatal care.

We use data from a controlled randomized trial to evaluate the impact of Mexico's CCT program, OPORTUNIDADES, on the birth weight of children from poor rural families and the pathways by which the improvements occurred. We find that the

program increased birth-weight by 127.3-grams and reduced the incidence of low birth weight by 4.6 percentage points, which represents a 44.5 percent reduction in low birth weight. We then examine three possible pathways for the program's impact on birth weight: increased utilization of prenatal care, improved quality of prenatal care, and maternal nutrition. We find that the improvements in birth weight are entirely attributable to the program's impact on quality.

Our analysis supports the hypothesis that *OPORTUNIDADES* affected quality through empowering women to insist on better care by informing them of what content to expect, and by giving them skills and social support to negotiate better care from healthcare providers. These results are further supported by qualitative research that reports increased self-confidence, and positive attitudinal changes with regard to healthcare, prevention and self-care, and patient participation (Adato, de la Brière et al 2000). Indeed, medical doctors reported that "beneficiaries are the ones who request the most from us" and they are "very demanding" (Adato, Coady, and Ruel 2000).

Our results are consistent with the theory of economics and identity (Akerlof and Kranton 2000 and 2002). This theory argues that one's sense of self can affect payoffs and economic outcomes. In the case of poverty and social exclusion, if poor and minority families view themselves as undeserving and those that provide them services hold similar views, then the less well off will not fully benefit from public services such as health and education. The explicit intervention to educate mothers to insist on their rights is in effect to change their identity in their own eyes and in the eyes of the medical care providers.

The policy implication of our results is that efforts to empower the less well off and change their 'identity' are necessary for public services to fully benefit the poor. Indeed, a major problem in access to health care and other public services is that poor, uneducated and minority beneficiaries may not know their health care rights or believe that they are entitled those rights from healthcare providers. These results support the goal of the specific recommendations put forward in the World Bank's 2004 World Development Report to make public services, such as health care, more accountable to clients, especially the less-well off (World Bank 2004).

Our results also contribute to the growing body of evidence that CCTs have greatly improved child health and nutritional outcomes. Across diverse settings, CCTs have been successful in reducing child mortality, anemia, diarrhea, acute respiratory infections, and stunting (Lagarde et al 2007, Morris et al 2004, Maluccio and Flores 2004). The CCT in rural Mexico has resulted in reductions in child morbidity, mortality, and anemia, and in improvements in child height for age and physical functioning (Gertler 2004, Rivera et al 2004, Barham 2005, and Fernald, Gertler, and Nuefeld in press). However, none of these studies evaluates the program's impact on birth weight or tries to sort out the specific pathways by which CCT programs are effective. Our paper is the first to document the impact on birth weight, and to examine women's empowerment and quality of care as mechanisms.

Finally, this paper contributes to the surprisingly small literature on the effect of the quality of prenatal care on birth weight. Indeed, while increasing improving quality of prenatal care is frequently promoted, the evidence base for this is weak (Carroli et al 2001). Several observational cross-sectional studies report positive associations between

the clinical content of prenatal care and birth outcomes. US women that did not receive all components of health advice recommended by the Expert Panel on the Content of Prenatal Care were more likely to have a low-birth-weight infant compared with women who reported receiving the optimal level of advice (Kogan et al 1994). In the second, having access to a more complete prenatal examination was associated with higher birth weight in Jamaica (Peabody, Gertler and Leibowitz 1998). The third study found that failure to comply with clinical standards was a strong predictor of perinatal mortality in Mexico (Cruz-Anguiano et al 2004). One study used panel data from Indonesia to try to sort out causality and found that improvements in quality as measured by adherence to prenatal and childcare clinical practice guidelines were associated with significant improvements in child height (Barber and Gertler, 2007).

This paper is organized in several sections. We first describe the *Oportunidades* program, the benefit structure, and health and nutrition requirements. We then discuss the epidemiology of birth weight in low-income settings and the pathways by which *Oportunidades* could improve birth weight. We then examine the magnitude of the reduced-form program impact on birth weight and the pathways by which the program could have worked. The paper concludes with a discussion of the findings that are relevant for policy.

II. Oportunidades

In 1997 Mexico established *OPORTUNIDADES* (originally called PROGRESA), a program designed to address short- and long-term poverty.[a] *OPORTUNIDADES* provides

[a] See SEDESOL-a, SEDESOL-b and SEDESOL 2003 for the operational rules and operational performance of *Oportunidades*.

cash transfers conditional on family members, especially pregnant women and young children, obtaining preventive medical care from public clinics, on attending *pláticas* (educational talks) about health-related topics, and on school-aged children attending school. While the income transfer is meant to address immediate needs such housing, food security and medical care needs, conditioning the transfer on health and education is designed to invest in the children's human capital. As a result, when the children reach adulthood, they will have the capabilities to take advantage of labor market opportunities and pull themselves out of poverty. In this sense, *OPORTUNDADES* was designed to break the intergenerational transmission of poverty. *OPORTUNIDADES* is one of the largest programs of its kind. In 2004, it distributed approximately US$ 3 billion to more than 5 million households – including approximately one-third of all rural families.

Cash Transfer Structure

Participating households receive cash transfers conditional on preventive health activities and children attending school (SEDESOL-a and SEDESOL-b). The monthly health stipend is conditional on each family member obtaining regular preventive medical care consultations and *pláticas,* or health education sessions. The principal beneficiary, usually the mother in the household, is also required to attend monthly program meetings. The health transfer is fixed at approximately US$15 per household per month regardless of the number of household members or their health requirements. The education transfer paid per child and the amount varies based on school grade and child sex. The transfer starts in the third grade and is conditional on 85% attendance and on not repeating a grade more than twice. The stipend rises substantially after completion of primary school and is higher for girls during junior high and high school. The maximum total monthly transfer was

capped at approximately US$ 90 and 160 for families with primary and high school children, respectively. Total transfers for health and education averaged 17 to 20 percent of pre-program rural per capita household consumption (Gertler et al 2007).

Oportunidades requires that households prove compliance via certification at public clinics and schools (Adato, Coady and Ruel 2000). Within the health facility, a beneficiary is provided an appointment book detailing the health requirements for all family members. One part of the form is kept at the clinic to record attendance and another part is returned to the beneficiary as proof of registration and attendance. An estimated 1 percent of households were denied the cash transfer due to non-compliance.

A unique feature of the program is the deliberate decision to give the cash transfers directly to the female head of household. This decision was based on the expectation that resources given to women would more likely be spent on improvements in health and nutrition within the family.

Healthcare requirements and nutritional supplements

The *OPORTUNIDADES* health requirements are extensive (Appendix Table A). They identify not only the number of visits but also the content of this care by age groups. Specifically for pregnant women, five prenatal visits are required, with an emphasis on monitoring the pregnancy's progression; and the prevention, detection, and control of obstetric and perinatal risk factors. Two additional post-partum visits correspond with the newborn check-ups at 7 and 28 days, and include family planning and maternal nutritional advice.

In addition to obtaining healthcare, nutritional supplements are given to pregnant and lactating women, all children between 4 months and two years, and malnourished

children between the ages of two and five years. The program developed two types of supplements to meet the separate nutritional needs of pregnant women and children. The main ingredients include whole dry milk, sugar, maltodextrin, vitamins, minerals, and artificial flavors and colors. For women, the 52-gram daily ration was intended to be consumed as a beverage. It provides 250 kilocalories of energy, 12-15 grams of protein, and includes iron, zinc, vitamin B12, Vitamin C, Vitamin E, folic acid and iodine. The specific content per ration for the maternal and child supplements are detailed elsewhere (Rosado et al 2000). Beneficiaries collect a one-month's supply of supplements at the health clinics for each targeted family member.

Empowerment

Oportunidades explicitly tried to empower women to take control of their lives to improve health outcomes through a series of activities. The first activity was a set of educational meetings to inform them about how to improve health, about the public health services available to them, and their rights to those services. The second was help in making and keeping appointments with health care providers as well as providing them with the skills necessary to get the most out of those appointments. The third was the social support to demand their rights from providers.

The program mandates attendance at monthly educational and programmatic meetings. Participating adults are required to attend monthly *pláticas,* or health education meetings (Adato, Coady and Ruel 2000). Up to 25 themes are discussed covering a broad range of topic from infectious diseases to cleaning latrines. Many of the *pláticas* emphasize prevention and reduction of health risks, including immunizations, sanitation and hygiene, and appropriate home care during illnesses. *Platicas* are mainly

directed at mothers as primary caregivers, although other family members and non-beneficiaries can also attend. Relevant to this study, pregnant women are required to attend *pláticas* in which information is provided about what to expect from prenatal care consultations, the clinical content of this care, maternal nutrition, and other reproductive health information.

Monthly meetings also occur between beneficiary women and *promotoras* (Adato, Coady and Ruel 2000). *Promotoras* are beneficiary women elected by other beneficiaries to act as a liaison between *Oportunidades* and the beneficiary communities. *Promotoras* receive training about how the program operates, convey new program information, answer questions, and complete monitoring forms. In health, they also carry out patient appointment reminders and help establish a communication link between the health centers and beneficiaries. The monthly meetings aim to ensure that the program's objectives and requirements are explicitly announced and understood, and to encourage women to ask for their right to basic health and educational services. These meetings are designed to provide beneficiaries with the skills and encourage them to obtain the full benefits of public services.

Faenas are voluntary communal work activities that involve community improvements, such as cleaning up schools, streets, or health clinics. While they were in place before the program, *promotoras* encourage *Oportunidades* beneficiaries to participate. *Promotoras* together with health workers make a link between program benefits and *faenas* as an incentive for beneficiaries to participate in activities that improve community hygiene and sanitation and promote social cohesion (Adato, Coady and Ruel 2000). The activities take place about once per month. Both the monthly

meetings with *promotoras* and the *faenas* provide an opportunity for women to discuss personal or community, and thereby provide social support to beneficiaries to actively pursue their newly empowered status.

In summary, the *platicas* provide information to improve health status and about their rights to public health care services, and the regular meetings with the *promotoras* make explicit the program benefits and entitlements. Both the monthly meetings and the *faenas* strengthen social support mechanisms for beneficiary women to take control of their lives to improve their living standards. These activities aim to empower women by increasing their capabilities to take action that positively affect the health and welfare of their families. Qualitative research suggests that the program did indeed succeed in empowering the beneficiaries as both the *Promotoras* and beneficiaries themselves reported increased self-confidence as well as freedom of movement and association (Adato, de la Brière et al 2000).

Beneficiary Enrollment and Duration of Benefits

The rural *OPORTUNIDADES* program established eligibility in two stages (Skoufias, Davis and Berhman 1999). First, the program identified underserved or marginalized communities and then identified low-income households within those communities. Poor communities were selected using a marginalization index constructed from 1990 and 1995 census data (*Conteo de Población y Vivienda*) measuring adult literacy; households with basic household infrastructure such as running water, drainage, electricity, and dirt floors; number of housing occupants; and the proportion of the labor force in agriculture.

Within poor communities, a socioeconomic survey was conducted (*Encuesta de Características Socioeconómicas de los Hogares* or *ENCASEH*) to identify eligible households using a proxy means test from data about household demographics and physical structure; individual socioeconomic characteristics, occupation, income, and disability; and access to health services. Households classified as poor were eligible for participation. The original classification scheme designated approximately 52% of households as eligible.

Over 90 percent of eligible households living in treatment localities enrolled in the program. Once enrolled, households received benefits for three years conditional on meeting program requirements. To prevent migration into treatment communities, new households were unable to enroll until the next certification period. Households in rural areas were recertified by proxy means tests after three years to determine future eligibility and continued receipt of program benefits. Ineligible households were still guaranteed three more years of support followed by three years of transitional support. Thus, households could expect a minimum of nine years of program benefits upon enrollment.

III. Pathways to improved birth weight

In this section, we provide an overview of the pathways by which *Oportunidades* is hypothesized to affect birth weight. The main contributors to low birth weight are preterm birth (<37 weeks gestation) and intrauterine growth restriction (IUGR) defined as less than 2500 grams at full gestational age. Whereas preterm birth accounts for the vast majority of low birth weight infants among high-income populations (Blondel et al 2002, and Villar and Belizan 1982), IUGR accounts for low birth weight in low-income settings such as the *Oportunidades'* beneficiary populations (Kramer 2000, and de Onis et al

1998). IUGR is attributable to low maternal nutritional intake, low pre-pregnancy body mass index, hypertensive disorders, and other untreated illnesses and infections (Bergstrom 2003, and Kramer 1987 and 2003). The main interventions to improve birth weight in this setting, therefore, include mechanisms to improve maternal nutrition, and the prevention and treatment of conditions during pregnancy.

Nutrition

The first pathway by which Oportunidades could have improved birth weight is through maternal nutrition. Improving dietary intake is promoted as an immediate intervention to address weight gain before and during pregnancy. Indeed, balanced protein-energy supplements have been demonstrated in randomized controlled trials to reduce the risk of low-birth weight by 30 percent (Merialdi et al 2003). Specific micronutrients including magnesium, calcium, and Vitamin A also promote higher birth weight ((Merialdi et al 2003) and Zulfiqar et al 2005).

Nutritional improvements are a key *Oportunidades* program component. In addition to the healthcare requirements, pregnant and lactating women are given nutritional supplements as part of program participation. However, operational problems may have reduced the impact of the supplements. A qualitative study of the acceptability of the nutritional supplements for the *Oportunidades* program reported that participants initially experienced nausea, diarrhea, and vomiting, which may have affected compliance (Zarco et al 2006). Efforts to minimize such side effects by diluting the supplement may also have reduced its nutrient density. Other reports suggest substantial leakage due to a culture of sharing food. In addition, the operational distribution of the

supplements was problematic and shortages in the availability of supplement were reported at health centers (Adatal, Coady and Ruel 2003).

Another pathway by the program could have affected maternal nutrition is through the cash transfers. Indeed, there is evidence that families spent a good portion of the *Oportunidades* cash transfer in purchasing food (Hoddinott and Skoufias 2003 and 2004). They not only purchased more calories, but those calories were of higher quality in terms of fruits, vegetable and protein.

Prenatal care utilization

The second possible pathway to improving birth weight is through use of prenatal care. The *Oportunidades'* requirements include five prenatal visits, which emphasize monitoring the pregnancy's progression, health education, and prevention, detection, and control of obstetric and perinatal risk factors. While an increased number of prenatal care visits has been promoted as a means to improve birth outcomes (Alexander and Korenbrot 1995), the evidence supporting this recommendation is weak (Carroli et al 2001).

In their review of randomized controlled trials evaluating a standard number vs. reduced number of prenatal visits, Carroli *et al* identify two published studies conducted in developing countries. The largest is a multi-site evaluation in urban clinics in Argentina, Cuba, Saudi Arabia, and Thailand (Villar et al 2001). This trial compared standard prenatal care averaging eight visits with 4 to 6 goal-oriented visits. They report no significant differences in low birth weight or urinary tract infection, and slightly higher rates of preeclampsia in the shorter, goal-oriented model. The second trial was conducted over a two-year period in Harare (Munjanja et al 1996). It randomized women

into a shorter goal-oriented program (median 4 vs. 6 visits), and found no significant differences in low birth weight, or perinatal and maternal morbidity and mortality.

Healthcare quality

While the number of prenatal care visits may not matter, the clinical content or quality is likely to be important in improving birth weight. Evidence-based clinical practice guidelines for prenatal care procedures are well established (Institute of Medicine 1985). The importance of such basic procedures can be illustrated with the example of anemia. Although anemia results from different factors including nutritional deficiencies and infectious diseases, the provision of iron folate is considered beneficial. Iron supplements during pregnancy have been demonstrated effective in reducing maternal anemia, and in increasing mean birth weight and reducing the incidence of low birth weight (Villar et al 2003). Measuring hemoglobin levels can detect moderate to severe anemia that requires additional attention beyond routine supplementation. However, the proportion of women in our sample who reported having a blood sample taken during prenatal visits averages 49 percent – ranging from 26 to 62 percent by clinical setting. Iron deficiency anemia at full gestational age has been reported at 40 percent and higher in Mexico (Shamah-Levy et al 2003). This suggests that increasing the proportion of healthcare providers conducting basic procedures could have a positive health impact.

Oportunidades could have resulted in higher quality of care because of increased awareness and expectations encouraged by the *pláticas* and meetings with *promotoras*. During pregnancy, women were required to attend monthly *pláticas*, which aimed to improve knowledge of the importance of pre- and postnatal healthcare among other

topics. In addition, the function of the monthly meeting with *promotoras* is to make the specific health requirements explicit and ensure that participants demanded their benefits. Therefore, the program could have increased the quality of health care received by promoting more informed and active healthcare consumers.

In fact, in qualitative interviews, medical doctors reported positive changes in beneficiary attitudes about healthcare, prevention and self-care, and patient participation. One doctor commented that "beneficiaries are the ones who request the most from us;" and a large proportion of health providers reported that beneficiary patients are "very demanding."

In fact there is substantial room for improvement in the quality of prenatal care in Mexico, as previous studies have documented substantial variation in health provider practice. A study of prenatal care providers in urban Mexico reported that adherence to a basic set of prenatal procedures that private practitioners provided 33.2 percent of the recommended clinical practice guidelines procedures compared with 88.2 percent among practitioners at social security clinics (Barber 2006). A similar study in rural areas found that, during history-taking, 57.0 percent of private health care providers asked about bleeding during the pregnancy (a serious danger sign) compared with 82.2 percent of providers in the social security system (Barber, Bertozzi and Gertler 2007). In Mexico City Hospitals, 26.7 percent of providers conducted three or less prenatal care procedures out of six essential activities to be offered to all women during prenatal visits (Coria-Soto, Bobadilla and Notzon 1996).

IV. Experimental Design and Data Sources

An important advantage of the rural *Oportunidades* program is that a controlled randomized evaluation was carried out early in its implementation to determine program impact. The evaluation was based on a sample of 506 communities in seven states, which were among the first to receive program benefits (Berhman and Todd 1999). The 506 treatment communities ranging from 500 to 2500 residents were randomly selected using probabilities proportionate to the size of communities from a total of 6,390 from seven states that were scheduled to be incorporated into the program in the next two phases of program implementation. Of these 506 communities, 320 were randomly assigned to the first phase and 180 to the second. Information about the timeline for implementation of benefits was not made public. Eligible households in treatment communities were scheduled to receive benefits in May 1998, and control communities started to receive benefits in December of 1999. Attanasio and Szekely (2007) find no evidence of an anticipation effect.

Our primary source of data was collected in 2003 in the *Encuesta de Evaluación* (ENCEL), a survey commissioned by the Mexican Government to evaluate the medium-term impact of rural *Oportunidades*. The analyses here employ data from modules collecting socioeconomic and fertility data from households. Complete fertility histories were collected as well as details of the last pregnancy. We supplement the 2003 information with socio-economic data collected from a 1997 baseline survey of the same households collected prior to the intervention, and with data on program incorporation and benefits distributed from OPORTUNIDADES administrative records.

While the socio-economic survey interviewed all households in the 506 communities, the fertility module was only applied to a sub-sample. The sub-sample size was based on sufficient power to detect small differences in reproductive outcomes between beneficiaries in communities incorporated into the program in May 1998 and those incorporated into the program in December 2000 assuming an 80 percent survey completion rate (CONAPO 2003). This survey used a two-stage stratified sampling design. Within each state, the target sample was based on the proportion of women of reproductive age across the three groups. In order to achieve the target sample size, 286 of 506 communities were randomly selected based on a probability sample proportionate to the number of women of reproductive age. Within each community, all households that had reproductive age women were surveyed, and a randomly selected women was surveyed from each household if the household had more than one eligible women were interviewed in selected households. The fertility survey achieved 84 percent fully completed interviews of the target sample. The most common reason cited for incompletion was not at home (5.1 percent); and 1.8 percent refused to be interviewed. In these analyses, we limit the sample to women who participated in the original randomized evaluation[b], experienced a live birth between 1997-2003, and reported valid birth weights. With these restrictions, the sample size is composed of 840 women (Figure 1).

[b] In the beginning of the evaluation approximately 52 percent of the households in the experimental communities were eligible for *Oportunidades* based on the original eligibility criteria. Over time, however, the government loosen the eligibility criteria to incorporate more households called densificados. We limit our analysis to the original group and exclude the densificados from the analysis.

IV. Did *Oportunidades* Have an Impact on Birth Weight?

The first set of analyses estimates the overall program impact on birth weight in grams and low birth weight (<2500-g). Birth weight in grams is derived from maternal reports, and we corroborated the reports with birth certificates and maternal health cards where available. Maternal recall of birth weight is considered accurate for extended periods, and evidence suggests that any recall bias is not correlated with socioeconomic factors (O'Sullivan et al 2000). Valid birth weight data are available for 82 percent of the sample; we dropped less than 1 percent of outlying or implausible values.

Empirical Methods

We regress birth weight and the probability of low birth weight on a dummy variable identifying whether the women was a beneficiary at any time before delivering her most recent live birth. Because of the randomization, this should provide us with a consistent estimate of program impact. However, we also include controls for individual, household, and community covariates that have been identified as predictors of birth weight in other studies in order to reduce idiosyncratic variation and improve the power of the estimates. Since the level of randomization was at the community, we estimate the model by random effects clustered at the community level. In addition, in order to test the robustness of the results, we also estimate the models using community fixed effects.

The dummy variable indicating whether the women was a beneficiary before the birth was constructed using was obtained using data detailing the timing of cash transfers received by beneficiaries based on household rosters and government administrative records. We compare eligible women that delivered before receiving their first cash transfer (non-beneficiary births) with eligible women that delivered a live birth at any

time after the household received their first cash transfer (beneficiary births). Figure 1 described how this sample overlays on the randomized control and treatment communities. Some 20.5 percent of eligible women delivered before receiving any cash transfers. On average, women in the sample participated in the program for 2.8 years before delivering.

The control variables in the analyses include maternal and child characteristics from the fertility module, baseline data from the 1997 census, and administrative reports. Maternal and infant characteristics include maternal age, marital status at the time of the birth, the number of pregnancies prior to the birth, miscarriage or abortion prior to the birth, infant sex, whether the mother smoked during pregnancy, days weighed after birth, and whether the child was still alive at the time of the interview. The 1997 census data provides socioeconomic characteristics prior to intervention in the communities. The information included in the analyses includes a series of household and community variables at baseline (1997): educational level and age in years of the household head, indigenous-speaking households, household size, proportion of child and adolescent household members by sex and age group (0 to 5 years and 6 to 17 years); household socioeconomic index (the sum of whether the household owned land, their house, a refrigerator, a gas water heater, and a television, and had internal water and had electricity), distance to urban area, male and female community wages, and altitude.

Results

The randomization succeeded in balancing the analysis sample based the comparison of household community, and maternal control characteristics between beneficiary and non-beneficiary births reported in panel A of Table 1, Panel A. Of the 22

variables considered, there is only one significant difference at the 5 percent level between the groups. The sample is composed of poor households living in rural areas. On average women were 29 years old at time of delivery. The women have had about six prior births, but few had miscarriages or reported smoking during pregnancy. Not surprisingly, the socioeconomic frequencies demonstrate that the sample is restricted to a very poor population. The sample is also about one-third indigenous, and household heads had less than four years of education on average.

We report the regression results predicting overall program impact in Panel A of Table 2. The first three models report the results for birth weight. The first model regresses birth weight against the treatment dummy variable "beneficiary" without any controls. The coefficient is the difference in the means of the two groups and indicates the beneficiary births were 82.0-grams larger than non-beneficiary birth, but the difference is not statistically significant. In model 2 we include the control variables described above. In this case, the estimates show a 127.3-gram program impact on birth weight, which is different from zero at the .05 level. Since mean birth weight in the control group is 3166.9 grams, the estimated impact represents about a 4.1 percent increase in mean birth weight. Finally, the community fixed effects results reported in model 3 is very similar to the random effects results. In fact, we cannot reject the random effects specification in favor of fixed effects based on Hausman test statistics reported in the last row with the model presenting the results including fixed effects.

In models 4-7 in Table 2, we report the results for models predicting low birth weight. Models 4 and 5 report the estimated log odds from random effects probits, and model 6 reports the results from linear probability models estimated with random and

fixed effects. Model 4 reports the results for a probit regression with no controls, suggesting a 19% reduction but that result is not significant. In model 5, where we add the controls, the beneficiary group had a 32% reduction in the odds of low birth weight, and that difference is significantly different from zero at the .05 level. We re-estimate the model using a linear probability specification (model 6) and find that beneficiaries had 4.6 percentage points lower low birth weight than the non-beneficiary group and that estimate is significant at the .05 level. Since mean low-birth weight in the non-beneficiary group was 10.3 percent, this specification suggests that the program reduced low-birth weight by 44.5 percent. Model 7 includes fixed effects and the estimates are similar. Again, we cannot reject the random effects specification in favor of fixed effects based on a Hausman test.

The coefficients on the control variables, not reported here but available upon request, are consistent with results from other studies (Kramer 1987, 2000 and 2003). Factors that predicted higher birth weight or a decrease in the odds of low birth weight include in maternal age and education for the mother and the head of household. The proportion of females aged 6 to17 years in the household is also a predictor of a decline in odds of low birth weight, independent of the number of prior births. Given traditional work and gender roles in rural Mexico, this could indicate assistance by adolescent children for household management that could reduce the mother's work burden. Consistent predictors of either lower mean birth weight and an increased probability of low birth weight include older maternal age, smoking during pregnancy, female infant sex, residing in an indigenous-speaking, distance to urban population centers, and female wages in the community. Higher female wages in the community could be related to

negative birth outcomes because of an increase in physical or emotional stress during pregnancy or possibly exposure to occupational reproductive health hazards.

The magnitude of these results on program impact compares well with other estimates of the impact of the program on child health. Evaluations of households participating in the randomized effectiveness evaluation report a 25.3 percent reduction in illness episodes, a similar decline in the probability of anemia among children, and an increase in age-adjusted height by 1.1 centimeters (Gertler 2004 and River et al 2004). Given that birth weight remains an important predictor of neonatal and infant mortality, this finding may also help explain the previous reports of an 11 percent decline in infant mortality among beneficiary households in rural areas (Barham 2005).

Falsification Test

One possible alternative explanation for our results is that there was some other change, such as an improvement in the quality of care or an economic boom, that occurred in treatment areas that did not occur in control areas. We test this hypothesis by examining whether pregnant women who were not eligible for OPORTUNIDADES in treatment areas had better birth outcomes than those in control areas. To do so, we define hypothetical beneficiaries as women not eligible for OPORTUNIDADES in the treatment areas that gave birth after the start of the program in April 1998, and non-eligible women in control areas that gave birth after the start of the program in control areas in November 2000. Similarly, we define hypothetical non-beneficiaries as non-eligible women in the treatment areas that gave birth before April 1998, and non-eligible women in control areas that gave birth before November 2000.

We regress birth weight and the probability of low birth weight on a dummy variable identifying whether the women was a hypothetical beneficiary at any time before delivering her most recent live birth. Again, because of the randomization, this should provide us with a consistent estimate of program impact. However, we also include the same controls for individual, household, and community covariates that have been identified above in order to reduce idiosyncratic variation and improve the power of the estimates. Since the level of randomization was at the community, we estimate the model by random effects clustered at the community level. In addition, in order to test the robustness of the results, we also estimate the models using community fixed effects.

The sample with valid birth weights includes 306 women, of which 85 were hypothetical non-beneficiaries. Comparing the control characteristics for the hypothetical beneficiaries and non-beneficiaries, we found all but two of the characteristics to be the same for the two groups. Hypothetical beneficiaries were slightly younger and they were living in households with slightly fewer children 0 to 5 years of age (Appendix Table C). These results suggest that the randomization was successful in balancing the characteristics of the ineligible population across control and treatment groups.

The results of the analysis are reported in Panel B (columns 8 through 11) of Table 2. We estimated both random and fixed effects versions of the birth weight and probability of low birth weight models with full sets of controls. The estimated program impact was small and not statistically different from zero in all cases. These results support the hypothesis that the differences in birth outcomes are a results of the program and not some other difference between the treatment and control communities.

V. Did *Oportunidades* Have an Impact on Utilization?

We now turn to the pathways by which *Oportunidades* improved birth weight. We first investigate whether the program had an impact on health care utilization as measured by the decision to seek prenatal care, the minimum number of consultations (five) required to receive *OPORTUNIDADES* benefits, and the total number of consultations. However, the descriptive statistics in the first row of Table 3 suggest that most women were already complying with the prenatal care requirements before they became beneficiaries. Specifically, 94.3 percent of the non-beneficiaries in this sample had obtained prenatal care, the average number of consultations was 6.4 compared with the required number of 5 visits, and nearly three-quarters had five visits or more. Hence, the program was unlikely to have much of an impact on utilization in this sample.

We examined program impact on utilization using the same methods as the analysis of birth weight. Specifically, we regressed the various measures of utilization on a dummy variable identifying whether the women was the beneficiary controlling for the individual, household, and community covariates included in the birth weight regressions. Because of the randomization, this should provide us with a consistent estimate of program impact on utilization. Again, since the level of randomization was at the community, we estimate the model by random effects.

The measures of utilization are whether the mother sought prenatal care, whether she obtained five or more visits, and the number of visits as a continuous variable. The estimation results are reported in Table 3. We find no impact of program participation in any of the models estimated, predicting prenatal care seeking, obtaining a minimum of five consultations, or the total number of consultations. This suggests that the program

impact on birth weight is not attributable to changes in utilization among beneficiaries, or compliance with the program's health utilization requirements.

VI. Did *Oportunidades* Beneficiaries Get Higher Quality?

While the number of prenatal care visits may not be a pathway, an improvement in the quality or clinical content of care may explain the effect of the program on birth weight. We posit that OPORTUNIDADES could have improved quality through three possible mechanisms. The first is that OPORTUNIDADES required beneficiaries to use public clinics and not provider providers. This would lead to an increase in quality if public providers supplied higher quality than private providers. Second, there may have been pressure on the Department of Health to improve the supply of prenatal services. Third, *Oportunidades* empowered women to demand better care through information, resources and a sense of entitlement.

Measurement

Quality is measured in terms of the clinical content of care or, in other words, the procedures that patients received that correspond with the Mexican clinical guidelines for best practice (Secreteria de Salud 1993). The prenatal procedures are those routinely conducted during history taking and diagnostics (obtaining blood and urine samples, asking about bleeding and discharge during pregnancy); the physical examination (measuring blood pressure, weight, and uterine height; and pelvic exam); and other preventive procedures (giving tetanus toxoid immunizations and iron supplements; advising about family planning and lactation; and recording health information). We

construct a composite index, which is the sum of positive responses as proportion of the total. We standardize the index to a mean of zero and standard deviation of one.

Panel A of Table 4 reports the proportion of women that received prenatal procedures by beneficiary status. We find that beneficiary women received on average more of the recommended procedures overall and for the average of each domain (history-taking, physical, and prevention). Specifically, higher proportions of beneficiary women had a urine sample and blood pressure taken, and had their uterine height measured, and their health information recorded.

Empirical Methods and Results

We examine program impact on quality using methods similar to the analysis of birth weight. Specifically, we regress the aggregate quality index on a dummy variable identifying beneficiary women controlling for the individual, household, and community covariates included in the birth weight regressions, with the exception of those related only to birth weight (i.e., infant sex, altitude). Because of the randomization, this should provide us with a consistent estimate of program impact on quality. Again, since the level of randomization was at the community, we estimate the model by random effects.

The results are reported in first two columns of Table 5. The first column reports the random effects estimates and the second reports the fixed effects estimates. We estimate that beneficiaries received 0.36 standard deviation units (SD) higher quality, and the magnitude of the results increases slightly with community fixed effects. Both of the estimates are significantly different from zero at the .01 level.

Why Did Quality Increase?

We consider three hypotheses regarding why beneficiaries received higher quality of care: (1) program requirements to obtain care from the public sector, (2) the government strenghthened the supply of health services, and (3) empowered beneficaries demanded better care. One of the programmatic goals was empowerment; on the demand side, women could have been empowered by the program to demand more and better care. We discuss briefly each of these possibilities in turn.

First, our previous research has documented that the quality in the public sector is significantly higher compared with private alternatives for the rural poor (Barber, Bertozzi and Gertler 2007). We report similar results for this sample in Panel B of Table 4. Given that beneficiary families were required to obtain care in public facilities, higher quality of care could have resulted from the substitution of private service providers with public service providers. However, we found no significant differences in the use of public sector for beneficiaries and non-beneficiaries in this sample (Table 4, panel C).

Second, there was some intention on the part of the government to increase supplies and human resources in anticipation of higher utilization among beneficiaries. However, there is no evidence that suggests that quality improvements were actually implemented in program areas (Adato, Coady and Ruel 2000). Improved technical quality could have resulted from better-trained healthcare providers with improved facilities and equipment practicing stricter adherence to clinical protocols. However, these supply-side interventions would have improved the quality of care for both *Oportunidades* beneficiaries and non-beneficiaries.

We test this hypothesis by examining whether pregnant women who were not eligible for OPORTUNIDADES in treatment areas received higher quality than those in control areas. Again, we define hypothetical beneficiaries as non-eligible women in the treatment areas that gave birth after the start of the program in April 1998, and non-eligible women in control areas that gave birth after November 2000. These women would have also benefited from supply-side improvements in health quality. Similarly, we define hypothetical non-beneficiaries as non-eligible women in the treatment areas that gave birth before the start of the program in April 1998, and non-eligible women in control areas that gave birth before November 2000. We used similar methods to the generating the beneficiary analyses, we identified women with valid birth weights and those that got care and reported about quality received.

We find no evidence that the hypothetical beneficiary group received higher quality than the hypothetical beneficiary group. There are no differences in the average quality scores for the hypothetical beneficiary and non-beneficiary groups (Panel D, Table 4). Similar to the previous analyses, we also estimate random and fixed effects models using as the dependent variable quality received. The results are reported in the last two columns in Table 5. After controlling for maternal and socioeconomic factors, there is no significant difference in quality received for hypothetical beneficiaries living in treatment areas. These regressions suggest that supply side improvements do not explain the higher quality received by program beneficiaries.

The remaining hypothesis is that the program empowered beneficiary women to demand higher quality. This finding is supported by qualitative research with beneficiaries and healthcare providers (Adato, de la Brière et al 2000). *Promotoras* and

beneficiaries report personal changes, including increased self-confidence, and freedom of movement and association. Medical doctors providing care to beneficiaries describe positive attitudinal changes with regard to healthcare, prevention and self-care, and patient participation. One doctor commented that "beneficiaries are the ones who request the most from us;" and a large proportion of health providers reported that beneficiary patients are "very demanding." Together, this evidence suggests that *OPORTUNIDADES* empowered women to insist on better care by informing them of what content to expect and by giving them skills to negotiate better quality care from healthcare providers.

VII. How Did *Oportunidades* Improve Birth Weight?

Lastly, we investigate the importance of different pathways that could explain the changes in birth weight. Because there appears to be no effect of the program on utilization, we focus on the contributions of nutrition, and quality of care. To do so, we re-estimate the reduced form birth weight and low birth weight regressions described in section IV replacing the treatment dummy variable with length of time on the program and quality of care.

Time on the program is a proxy for the nutrition content of the program and any other changes as a result of the program. Recall that improved nutrition could have resulted from either the supplements or more and better nutrition purchased by households. We measure the impact of nutritional supplements and purchased food through time on program as the effects of nutrition on a women's health is cumulative. The longer someone has been on the program, the more food they have been able to

purchase and the longer they could have benefited from the supplements. Time on the program would also pick up any other effects on health changes that the program might have caused through the platicas or the use of the cash transfers.

Time on program is measured as the number of program months from the start of cash payments to the date of delivery. Similar to the dummy variable identifying program participation at time of delivery, the number of program months is exogenous because the actual timing of incorporation into the program was randomized, and a previous study found no relationships between the program and fertility decisions (Steklov et al 2006).

We measure quality using the standardized composite index described in section VI. Quality, however, was not allocated randomly as part of the program and may be endogenous. For instance, concerned mothers who are experiencing difficult pregnancies may be receiving more clinical services and difficult pregnancies may be correlated with lower subsequent birth weight. In this case, least squares estimates would underestimate the true impact of quality on birth weight.

We use an instrumental variables approach to identify the impact of quality on birth weight. Our instruments are the average community quality supplied in public clinics and in private sector adjusted for the observed characteristics of the mothers receiving the care. We generated these instruments, by regressing the quality index on a set of maternal and household socioeconomic characteristics, beneficiary status, and community fixed effects interacted with whether the care was at a public clinic or private provider. The estimation sample included all women who had a prenatal care visit including beneficiaries, non-beneficiaries and ineligibles. Our instruments are the

community public provider fixed effects, community private provider fixed effects and the weighted average of the public and private fixed effects interacted with maternal education. The fixed effects reflect the average quality supplied by public and private providers in a given community, but are purged of differences in observed characteristics that represent individual or socioeconomic risk. The first stage regressions are presented in Appendix Table B.

The results are presented in Table 6 where columns 1 to 3 describe the results for birth weight in grams and columns 4 to 6 for low birth-weight. All models are estimated using community random effects and include the same controls used in the reduced form birth weight models reported in Table 3. Column 1 reports the results from a model that including program months and uninstrumented quality. Neither variable is significant at conventional levels. The second model replicates the first but the quality coefficient by instrumental variables. Quality becomes significant and we estimate that a one standard deviation unit increase in quality is associated with an increase in birth weight of 387.8-grams. However, program-months is not significant. In model 3, we remove use program months as a regressor and report the instrumental variables estimate of the coefficient on quality. We find that a one standard deviation increase in quality predicts an increase in birth weight of of 409.7-grams, significant at the 1 percent level.

Column 5 to 7 replicates the same specifications for low birth weight (2500-grams =1). Similar to the previous regressions, program-months as a separate regressor does not predict declines in low birth weight in Models 5 and 6. While quality is not significant in model 5, the magnitude of the coefficient is large. When we remove program-months, quality becomes significant at the .05 level and we estimate that a one

standard deviation increase in quality is associated with a 14-percentage point decrease in low birth weight.

We then use the information from the random effects models (Table 5) that beneficiaries received 0.36 SD higher quality and the estimates of quality on birthweight (Table 6, Models 3 and7). These results suggest that the program impact operating via quality improvements amounted to a 148.8-gram increase in birth weight, and 5.1-percentage point reduction in low birth weight. This estimate is very close to the 127.3-grams overall program impact on birth weight and 4.6-percentage point impact on low birth weight estimated in the reduced form models. These results support the hypothesis that the program impact on birth weight largely operated through improvements in quality.

VII. Discussion

This study demonstrates that *Oportunidades* resulted in improved birth outcomes. Specifically, using the randomized design, we find that beneficiary births were 127.3-gram higher and 44.5 percent less likely to be low birth weight than non-beneficiary births. In examining the pathways for this result, we conclude that these improvements in birth weight were primarily attributable to improvements in the quality of prenatal care, that the improved quality is a manifestation of the program's empowering women to demand their right to quality care. The program empowered women by informing women about the importance of prenatal care, the content of prenatal, their rights to this content, providing social support, and encouraging them to be informed and active health consumers.

These results contribute to the growing body of evidence cited earlier that CCTs are having a large impact on child health improvements attributable to the program (Lagrade et al 2007). Despite the fact that almost all of the previous analysis is limited to reduced form impacts, this literature attributes the program impact to a combination of price incentives for the use preventive medical care, the income effects of the cash transfer and the nutrition supplements. Ours is one the first to attribute the impacts to quality of medical care and empowerment as the mechanism.

Our results also provide empirical support to the theory of the economics of identity. The idea is that one's identity enters the utility functions of the beneficiary and the provider, and thereby affects equilibrium service provision. If poor and minority families view themselves as undeserving and those that provide them services hold similar views, then the less well off will not fully benefit from public services such as health and education. We show that empowerment is a means by which governments can change identity and improve service provision to previously underserved groups. The policy implication is that efforts to empower the less well off and change their 'identity' are necessary for public services to fully benefit the poor.

Our results are also among the first in to empirically demonstrate the importance of efforts to improve the quality of prenatal care on birth weight in the developing world. Indeed there is substantial potential for investment in quality as means to improve birth outcomes as there is wide variation in adherence to prenatal care clinical guidelines in a large number of countries covering all corners of the developing world (Barber 2006; Barber, Bertozzi and Gertler 2007; Barber Gertler and Harimurti 2007; Das, and Hammer 2005; Pallikadavath 2004; Piaggio et al 1998; and WHO 2003). In addition to

empowerment, other efforts to improve quality such as pay for performance and those promoted in the 2004 World Development Report on making services work for the poor should be considered (World Bank 2004).

Finally, while it is notable that the nutritional supplements seemed to have little contribution to improvements in birth weight, not too much should be read into this. Indeed, as discussed above, the distribution and consumption of the supplements had critical problems related to compliance, leakage, and availability. In addition, the effect of nutritional supplements during pregnancy may not be captured over a short study span. Previous studies in Guatemala have reported significantly higher birth weights (150-g) among women who received high-energy, high-protein supplements as children over a 30-year study period (Ramakrishnan et al 1999). This suggests that the benefits of improved nutrition could be intergenerational rather than immediate.

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Figure 1. Flow diagram of study design, participants and sample sizes

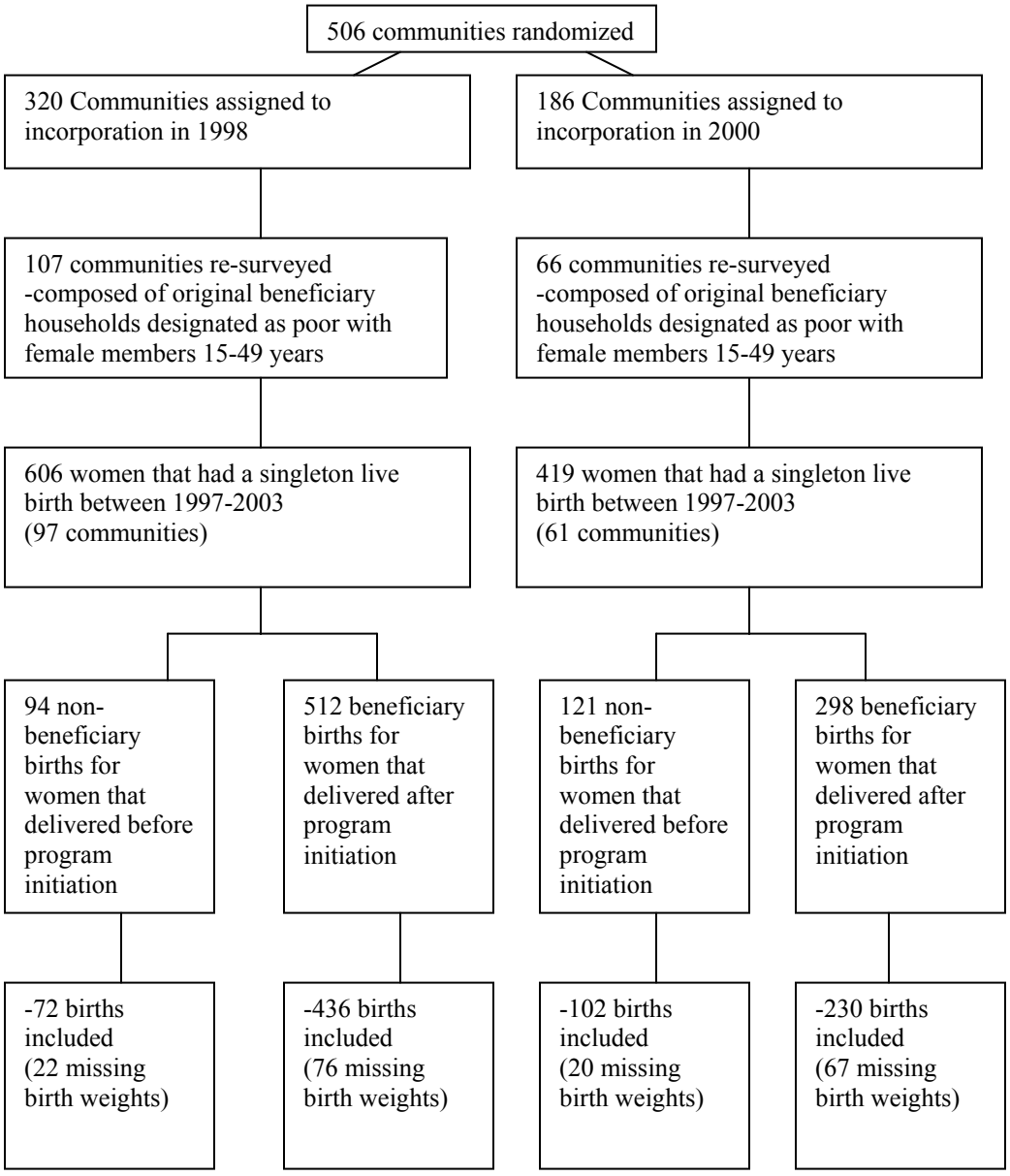

Table 1. Comparison of Means (Standard Deviations) of Baseline Characteristics (N = 840)

Variables	Non-beneficiaries	Beneficiaries		Difference	p-value
Maternal and infant characteristics					
Maternal age (yrs)	29.48 [6.38]	29.22 [6.75]		-0.25	0.659
Total prior pregnancies	5.05 [2.42]	4.62 [2.59]	∞	-0.43	0.041
Prior miscarriage or abortion (%)	8.05	6.61		-1.44	0.487
Mother smoked during pregnancy (%)	4.60	4.80		0.20	0.893
Days after birth weighed	3.37 [7.81]	2.48 [6.08]		-0.89	0.115
Alive at time of interview (%)	99.43	98.20		-1.23	0.260
Female (%)	43.68	46.85		3.17	0.493
Baseline household socioeconomics and demographics					
Household socioeconomic index (0-1) ‡	0.42 [0.18]	0.41 [0.18]		-0.02	0.359
Indigenous-speaking household (%)	27.01	34.53		7.52	0.067
Household head years of schooling	3.70 [2.71]	3.60 [2.57]		-0.10	0.725
Age of household head (yrs)	41.32 [8.91]	40.17 [9.92]		0.15	0.153
Maternal years of schooling	4.18 [2.54]	4.19 [2.73]		0.01	0.953
Household size	6.51 [2.23]	6.53 [2.43]		0.03	0.908
Males, age 0-5 years (proportion)	0.15	0.14		-0.01	0.399
Females, age 0-5 years (proportion)	0.16	0.14		-0.02	0.151
Males, age 6-17 years (proportion)	0.14	0.16		0.02	0.219
Females, age 6-17 years (proportion)	0.16	0.14		-0.01	0.247
Baseline community characteristics					
Altitude (meters)	1255.43 [855.58]	1333.69 [805.35]		78.26	0.342
Distance to urban center (km)	106.42 [43.94]	107.91 [43.16]		1.49	0.750
Health center in community (%)	78.13	81.23		3.10	0.321
Female wage (pesos per mo)	163.38 [507.28]	178.25 [576.46]		14.87	0.717
Male wage (pesos per mo)	221.10 [1218.51]	267.29 [1140.06]		46.19	0.417
Sample Size	**174**	**666**			

Notes: ‡ Household socioeconomic index is sum of whether the household owned land, their house, a refrigerator, a gas water heater, and a television, and had internal water and had electricity. ∞Differences significant at 5 percent or smaller level

Table 2. Regression models estimating program impact on birth outcomes

	Panel A: Eligible Population (n=840)							Panel B: Ineligible (Hypothetical) Population (n=391)			
	Birth Weight in Grams			Low birth weight (<2500-grams =1)				Birth Weight in Grams		Low Birth Weight = 1	
	Mean (std dev) birth weight of non-beneficiaries = 3,167 grams (632)			Proportion of non-beneficiary births that are low birth weight = 0.103				Mean (SD) birth weight of non-beneficiaries = 3,153 grams (482)		Proportion of non-beneficiary births that are low birth weight = 0.054	
	RE	RE	FE	RE Probit (log odds)	RE Probit (log odds)	RE linear prob	FE linear prob	RE	FE	RE linear prob	FE linear prob
	1	2	3	4	5	6	7	8	9	10	11
Beneficiary (=1)	81.9 [54.2]	127.3 ** [54.0]	101.8 * [58.3]	-0.186 [0.154]	-0.323 ** [0.161]	-0.046 ** [0.024]	-0.044 * [0.025]	37.12 [79.01]	6.46 [114.7]	-0.009 [0.034]	-0.004 [0.048]
Control Variables											
Maternal/infant characteristics	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline household socioeconomic & demographic	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline community characteristics	No	Yes	No	No	Yes	Yes	No	Yes	No	Yes	No
Community fixed effects	No	No	Yes	No	No	No	Yes	No	Yes	No	Yes
Hausman Prob>chi2 =			0.26				0.75		0.05		0.03

Notes: . RE = random effects; FE =fixed effects; Coefficients and standard errors reported in the Beneficiary row. Regressions include control variables listed in Table 1; * indicates significant at the .10 level and ** indicates significant at the .05 level.

Table 3. Regression models estimating program impact on prenatal care-seeking and utilization

	Sought prenatal care (=1)	Obtained five consultations or more (=1)	Number of consultations
Mean of Dependent Variable for Non-beneficiaries	0.943	0.742	6.40
	RE probit dy/dx	**RE probit dy/dx**	**RE poisson dy/dx**
Beneficiary (=1)	0.034 [0.236]	0.015 [0.130]	-0.0348 [0.037]
Control variables			
Maternal and infant characteristics	Yes	Yes	Yes
Household socioeconomics & demographics	Yes	Yes	Yes
Community socioeconomics	Yes	Yes	Yes
N of Observations	840	804	804

Notes: Coefficients and standard errors reported for program participation variables, RE=random effects, and specific control variables are listed in Table 1.

Table 4. Measuring quality: proportion of prenatal care clinical procedures received by beneficiary status

	Non-beneficiary birth	Beneficiary birth	Difference	p-values
Panel A. Prenatal procedures and quality scores				
History-taking and diagnostics summary score	0.5472 [0.3298]	0.6282 [0.3210]	0.081	0.006
Asked about bleeding	0.711	0.758	0.047	0.208
Asked about discharge	0.717	0.771	0.054	0.157
Blood sample taken	0.411	0.490	0.079	0.095
Urine sample taken	0.350	0.493	0.143	0.000
Physical examination summary score	0.763 [0.280]	0.821 [0.217]	0.059	0.009
Blood pressure taken	0.850	0.941	0.091	0.000
Weighed	0.922	0.951	0.029	0.147
Uterine height measured	0.828	0.883	0.056	0.061
Pelvic exam	0.450	0.510	0.060	0.159
Prevention and case management summary score	0.836 [0.265]	0.879 [0.215]	0.043	0.053
Tetanus toxoid immunization	0.894	0.931	0.037	0.105
Iron supplements	0.806	0.858	0.053	0.113
Advised about lactation	0.900	0.910	0.010	0.668
Advised about family planning methods	0.856	0.883	0.028	0.314
Recorded appointments	0.722	0.810	0.088	0.014
Total Quality summary scores				
Raw (0-1)	0.724 [0.252]	0.784 [0.201]	0.060	0.005
Standardized	-0.222 [1.178]	0.056 [0.943]	0.279	0.005
Panel B. Public and private quality scores				
Public clinical quality scores (standardized)	-0.003 [0.897]	0.190 [0.715]	0.193	0.036
Private clinical quality scores (standardized)	-1.719 [1.703]	-0.917 [1.609]	0.802	0.034
Panel C. Care seeking				
Sought care in public sector (=1)	0.872	0.879	0.007	0.774
Panel D: Comparison of Quality Scores for (Ineligible) Hypothetical Beneficiaries and Non-Beneficiaries				
	Hypothetical Non-beneficiary birth	**Hypothetical Beneficiary birth**	**Difference**	**p-values**
Quality scores (standardized)	-0.054 [1.043]	0.018 [0.987]	0.072	0.654

Table 5: Regression models estimating program impact on standardized quality of prenatal care for eligible and ineligible (hypothetical) populations

	Panel A: Eligible Population		Panel B: Ineligible (Hypothetical) Population	
	1	2	3	4
Beneficiary (=1)	0.363 *** [0.078]	0.405 *** [0.081]	0.102 [0.157]	0.142 [0.159]
Control Variables				
Maternal & infant characteristics	Yes	Yes	Yes	Yes
Household socioeconomics & demographics	Yes	Yes	Yes	Yes
Community socioeconomics	Yes	No	Yes	No
Community Fixed Effects	No	Yes	No	Yes

Notes: Regressions include all covariates listed in Table 1 and *** indicates coefficient is significant at the .01 or better level

Table 6. Regression estimates of the impact of length time on program and quality of care on birth outcomes

	Birth weight in grams			Low birth weight (< 2500 g=1)		
	1	2	3	5	6	7
	RE	IV RE	IV RE	RE	IV RE	IV RE
Program months	1.83 [1.13]	0.31 [1.55]		-0.001 [0.001]	-0.000 [0.001]	
Standardized quality (SD)	18.84 [24.01]	387.76 ** [193.36]	409.66 *** [165.17]	0.009 [0.011]	-0.112 [0.085]	-0.140 ** [0.070]
Covariates						
Maternal & infant characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Community characteristics	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Regressions include all covariates listed in Table 2, IV=instrumental variables and first stage regression results reported in Appendix Table C, ** indicates coefficient is significant at the .05 and *** indicates significant at the .01 or better level

Appendix Table A. Health requirements for *Oportunidades* beneficiaries, by age group[c]

Age group	Required number of visits	Health care content
Less than 4 months	3 visits at 7 days, 28 days, and 2 months	• Immunizations • neonatal screening • growth monitoring (height and weight) • breastfeeding promotion
4 to 23 months	6 visits at 4, 6, 10, 12, and 18 months	• immunizations • growth monitoring (height and weight) • psychomotor development and early stimulation • nutritional monitoring • early disease detection • nutritional supplements
2 to 4 years	2 visits per year at 6 month intervals	• immunizations • growth monitoring (height and weight) • treatment of parasitic infections • early disease detection
5 to 9 years	2 visits per year at 6 month intervals	• immunizations • assessment of growth and development • early disease detection
10 to 19 years	2 visits per year at 6 month intervals	• immunizations • physical and mental health education • sex education and family planning • prevention and sexually transmitted infections/ HIV/AIDs • early disease detection
Pregnant women	5 visits	• nutritional assessment • monitoring the progression of pregnancy • administration of iron and folic acid • immunizations, nutritional supplements • information, education and communication to the couple to promote healthy behavior during pregnancy, delivery, post-partum • prevention, detection and control of obstetrical and perinatal risk factors • family planning advice
Post-partum and lactation period	2 visits at 7 days and 28 days	• family planning • nutritional advice • care of newborn • promotion of breastfeeding • provision of nutritional supplements
Adults 20-49 years	2 visits per year at 6 month intervals	• reproductive health and family planning • prevention of sexually transmitted infections, including HIV/AIDs • early disease detection • physical and mental health education • completion of the women's health card[d]
Adults 50 years and older	1 visit per year	• early detection of chronic and degenerative diseases and cancers • completion of the women's health card

[c] Secretaria de Desarrollo Social. Agreement for issue and publication of the operational rules of the *Oportunidades* program for human development for the fiscal year 2003 (in Spanish). Diario oficial de la federación. México: SEDESOL; 8 May 2003. http://www.progresa.gob.mx/transparencia/Reglas_operacion_2003.pdf

[d] The *Cartilla Naciónal de Mujer* is a special initiative of the Ministry of Health established by presidential decree in 1998 to collect comprehensive information about women's health problems and medical care relating to prevention and control of cervical and breast cancer, perinatal health, family planning, detection and control of hypertension, immunizations, and menopause.

Appendix Table B. Comparison of means (std dev) of baseline characteristics for hypothetical groups (n=391)

Variables	Non-beneficiaries	Hypothetical Beneficiaries		Difference	p-value
Panel A. Control variables					
Maternal and infant characteristics					
Maternal age (yrs)	27.70	25.81	∞	-1.89	0.0410
	[6.77]	[6.32]			
Total prior pregnancies	3.73	3.17		-0.56	0.2420
	[2.69]	[2.32]			
Prior miscarriage or abortion (%)	7.14	6.63		-0.51	0.9068
Mother smoked during pregnancy (%)	0.00	5.42		5.42	0.1041
Days after birth weighed	1.61	2.44		0.83	0.2980
	[3.53]	[7.13]			
Alive at time of interview (%)	96.43	98.19		1.76	0.4954
Female (%)	44.64	42.17		-2.47	0.7411
Baseline hshd socioeconomics & demographics					
Household socioeconomic index (0-1) ‡	0.61	0.61		0.00	0.6110
	[0.21]	[0.22]			
Indigenous-speaking household (%)	24.40	14.95		-9.45	0.0686
Educational level of household head (yrs)	5.09	4.36		-0.73	0.0990
	[3.22]	[3.43]			
Age of household head (yrs)	42.45	43.15		0.15	0.6870
	[10.70]	[12.93]			
Maternal educational level (yrs)	5.25	5.53		0.28	0.5340
	[3.08]	[2.92]			
Household size	6.34	6.34		0.00	0.9960
	[2.80]	[2.49]			
Males, 0-5 years in household (%)	0.13	0.09	∞	-0.05	0.0300
Females, 0-5 years in household (%)	0.11	0.08		-0.03	0.0850
Males, 6-17 years in household (%)	0.14	0.15		0.02	0.4030
Females, 6-17 years in household (%)	0.12	0.14		0.02	0.2150
Baseline community characteristics					
Altitude (meters)	1688.93	1567.44		-121.49	0.4580
	[781.89]	[803.92]			
Distance to urban center (km)	94.15	90.83		-3.32	0.6630
	[38.13]	[41.25]			
Health center in community (%)	64.29	66.27		1.98	0.8192
Female wage rate (pesos per mo)	196.43	228.06		31.63	0.7150
	[439.74]	[605.27]			
Male wage rate (pesos per mo)	393.37	430.30		36.93	0.6640
	[1884.96]	[1923.15]			
Observations	85	306			

Notes: †The number with data for the prenatal care visits is 208. ∞Differences significant at 5 percent level

Appendix Table C: First Stage Regressions for Quality in Table 7

Instrumental variables	
Community Level Quality of Public Health Care	6.86 *** [2.38]
Community Level Quality of Private Health Care	-0.42 *** [0.15]
Mean Community quality times maternal years of school	-1.79 ** [0.79]
Beneficiary status (=1)	0.16 ** [0.08]
Additional Control variables	
Maternal & infant characteristics	Yes
Household characteristics	Yes
Community characteristics	Yes
Year fixed effects	Yes
F-statistic for joint significance of the instrumental variables	**73.3**

Notes: Regressions include all covariates listed in Table 2, , ** indicates coefficient is significant at the .05 and *** indicates significant at the .01 or better level