An Experiment in Incentive-Based Welfare: The Impact of PROGESA on Health in Mexico

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ABSTRACT

We investigate the impact of a unique anti-poverty program in Mexico on health outcomes. The program, *PROGRESA*, combines a traditional cash transfer program with financial incentives for families to invest in human capital of children (health, education and nutrition). In order to receive the cash transfer, families must obtain preventive health care, participate in growth monitoring and nutrition supplements programs, and attend education programs about health and hygiene. Incentive-based welfare programs like PROGRESA are being implemented throughout Latin America including Argentina, Brazil, Columbia, Honduras and Nicaragua.

Our analysis takes advantage of a controlled randomized study design with household panel data. We find that the program significantly increased utilization of public health clinics for preventive care. The program also lowered the number of inpatient hospitalizations and visits to private providers, which is consistent with the hypothesis that PROGESA lowered the incidence of severe illness.

We found a significant improvement in the health of both children and adults. Specifically, children had about a 23 percent reduction in the incidence of illness, a 1 to 4 percent increase in height, and an 18 percent reduction in anemia. Adults experienced a significant reduction in the number of days of difficulty with daily activities due to illness and in the number of days in bed due to illness. Adults also reported a significant increase in the number of kilometers able to walk without getting tired.

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0. INTRODUCTION

One of the greatest tragedies of extreme poverty is its intergenerational transmission. Many argue that one way to break the intergenerational transmission is to invest in the human capital of children living in poverty. Sen (1999) argues that children from poor families enter adulthood without "the basic capabilities" necessary to enjoy an acceptable quality of life and to take advantage of labor market opportunities to pull themselves out of poverty.

While much of the policy discussion focuses on education, children's health and nutrition is of also of tremendous importance. Health and nutrition in the formative years significantly improves physical and cognitive development.[1] Healthier children also start school earlier[2], receive more years of schooling, and do better in school.[3] In addition, healthier adults with better cognitive ability schooling have substantially higher wages.[4]

In this paper, we investigate the impact of a unique anti-poverty program in Mexico on the health of young children. The program, *PROGRESA*, combines a traditional cash transfer program with financial incentives for families to invest in the human capital of their children.

Program benefits include cash transfers that are disbursed conditional on the household engaging in a set of behaviors designed to improve health and nutrition. The family only receives the cash transfer if: (i) every family member accepts preventive health services; (ii) children age 0-5 and lactating mothers attend nutrition monitoring clinics where their growth is measured, they obtain nutrition supplements, and they receive education on nutrition and hygiene; and (iii) pregnant women visit clinics to obtain prenatal care, nutritional supplements, and health education[5]. The size of the cash transfer is large, corresponding on average to about one-third of household income for the beneficiary families. Another unique feature of the program is that the cash transfers are given to the mother of the family, a strategy designed to target the funds within the household to improving the children's education and nutrition (*PROGRESA*, 2001).

PROGRESA is a national program adopted in 1997. By 2000, *PROGRESA* covered approximately 2.6 million families, which is about one-third of rural families, or ten percent of all families in Mexico. The program operates in almost 50,000 rural villages in 31 states. *PROGRESA*'s budget is about US$800 million or 0.2% of GDP. The *PROGRESA* model is extremely popular and is in the process of being adopted by Argentina, Colombia, Honduras, and Nicaragua.

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[1] See, for example, Haas *et al*, 1996; Grantham-McGregor 1998; Martorell 1995 and 1999; Martorell, Khan and Schroeder 1994; and Martorell, Riveria and Kaplowitz 1989.
[2] See, for example, Alderman *et al*, 2000; Glewwe and Jacoby, 1995; and Glewwe, Jacoby and King 2000.
[3] See, for example, Behrman, 1993; Jamison, 1986; Leslie and Jamison 1990; Moock and Leslie, 1986; and Pollitt , 1990.
[4] See, for example, Alderman *et al*, 1996; Boissiere, Knight and Sabot, 1985, Glewwe, 1996; Lavy, Spratt and Leboucher, 1997; Behrmans Deolalikar 1989; Deolalikar, 1988; Haddad and Bouis 1991; Strauss 1986; Thomas and Strauss 1997.
[5] An additional cash transfer is given to households with school age children if the children are enrolled and attend school.

Our analysis takes advantage of a controlled randomized design. In 1998, 506 of the 50,000 *PROGRESA* villages were randomly assigned to control and treatment groups. Eligible households in treatment villages received benefits immediately, while benefits for eligible households in control villages were postponed for two years. A pre-intervention baseline survey of approximately 14,500 households with over 80,000 individuals and four follow-up surveys (at six month intervals) of the same households were conducted over the two-year experimental period.

1. THE INTERVENTION

PROGRESA is designed to overcome the problems in the traditional demand and supply approaches to improving child health and nutrition in developing countries; the demand approaches are variations of income transfers programs and the supply approaches cover improvements in the direct provision of medical and nutrition services**.**

Income transfers can raise the children's health if the primary cause of poor health in children is the liquidity constraint faced by the parents. Income transfers loosen this constraint and allow parents to allocate their resources to the child's most pressing needs (e.g. nutritious food), which differ widely among families and among children within a family. Providing purchasing power also permits parents to choose a high-quality provider of goods and services. However, parents may not understand the benefits of some health interventions and coupled with competing priorities, they many not use the cash transfers for its intended purpose, investment in child health and nutrition.

The limited empirical evidence on this topic suggests that cash transfers are a not an effective means of improving child health. In developed countries cash transfers do not appear to raise child health at all (Currie 1995). In developing countries, to our knowledge there is only one direct study that finds a positive impact of a cash transfer program on health outcomes. Duflo (2000) finds that extremely large transfers on the order of doubling household income to grandmothers in South Africa improved granddaughters' but not grandsons' health. Moreover, similar transfers to grandfathers had no impact on grandchildren's health of either gender. More generally, the effect of income on child health outcomes in low-income countries remains controversial.[6]

The other approach to improving child health and nutrition is through the direct provision of free health care and nutrition interventions. Unlike cash transfers, direct provision may better target the intervention to child health. The downsides can include crowding out of other parental expenditures on the child and low program take-up rates. Indeed, prenatal care and nutrition monitoring and supplementation programs only benefit those women and children who actually choose to attend prenatal care visits. For example, low-income women in the U.S. often did not take advantage of free prenatal care programs (Cook *et al*., 1999 and York *et al*., 1999). While there have been a large number of government run nutrition programs targeted to poor populations in developing

[6] See, for example, Alderman (1986, 1993), Behrman and Deolalikar (1987, 1988), Behrman, Foster and Rosenzweig (1997), Bouis (1994), Bouis and Haddad (1992), Strauss and Thomas (1995 1998), and Subramanian and Deaton (1996).

countries, there has been little formal evaluation of the impact on health outcomes (WHO/UNICEF/IFPRI, 2000). The results of those that have been rigorously studied are, at best, mixed. One of the biggest problems is the overall low take-up, of which families in greatest need have the lowest take-up rates.

PROGRESA combines the two strategies by relaxing budget constraints with a cash transfer, but using that transfer as an incentive to increase take-up rates in the direct provision of programs. The combination of these separate strategies creates the possibility of large complementarities since the subsidy is conditional on participating in the health care program. In such a fashion, the cash transfer can both alleviate liquidity constraints and raise take-up rates for prenatal care and nutrition programs.

Benefits and Incentive Structure

Program activities are aimed at improving the educational, health and nutritional status of children living in extreme poverty[7]. *PROGRESA*'s benefits are structured in a novel way such that the income transfers not only increase financial resources to the household but also provides incentives to participate in the other program activities.

The size of the cash transfer is large, corresponding to about a 25 percent average increase in income of households living in extreme poverty. A unique feature of the program is that the cash transfers are given to the mother of the family, under the belief that the cash will be invested in more food and other productive purposes. In fact, Hoddinott and Skoufios (2000) found that 70 percent of the cash transfer has been used to increase food availability in the household both in terms of quantity (calories) and quality (richer in protein and micronutrients).

The cash transfer is conditional on participating in three sets of activities to promote family health and nutrition:

1. Preventive medical care including prenatal care, well baby care, child immunizations, growth monitoring from conception through age 5, and adult preventive checkups
2. Nutritional supplements for children age 0-24 months, for child age 3-5 found to be malnourished during growth monitoring, and for pregnant and lactating women
3. Health, hygiene and nutrition education programs

Because the determinants of an individual's health over their lifetime begin *in utero*, PROGERSA is designed to improve the health of children starting at conception. The interventions are designed to first lower the number of low birth weight (LBW) babies. LBW may be due either to premature delivery or to the infant being small for gestational age due to intrauterine growth retardation (IUGR). LBW babies are at

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[7] *PROGRESA* promotes school attendance and educational performance by providing cash grants for each child enrolled in school in grades 3 through 9 who achieves minimum attendance during the school year. Schultz (2000) finds that the *PROGRESA* educational grants significantly increase enrollment at both the primary and secondary levels.

substantially higher risk of neonatal and infant mortality, severe diarrhea, and pneumonia (Ashworth, 1998 and McIntire *et al* 1999). LBW also has significant long-term consequences on body size, composition and muscle strength. While there is potential for some LBW newborns to catch up during the first two years of life, a healthy environment is typically not enough to compensate for prenatal growth retardation. In fact, surviving LBW infants tend to be about 5 centimeters shorter, 5 kilograms lighter and significantly weaker in adulthood.[8] There is also a greater risk of neurological dysfunction, hyperactivity, clumsiness and poor school performance (Goldenberg *et al*, 1998), poor cognitive development (Grantham-McGregor 1998), and impaired immune function.[9]

Both IUGR and premature delivery are associated with poor maternal nutrition and illness during pregnancy. The nutritional determinants of LBW include inadequate maternal nutrition before conception, short maternal stature, and poor nutrition during pregnancy, which usually corresponds to a low gestational weight gain (Miller and Merritt, 1979; Prada and Tsang, 1998) and the poor intake of protein, calories, and micro-nutrients.[10] Maternal diseases, especially diarrhea, intestinal parasitosis, pre-eclampsia, and respiratory infections also have an association with LBW.[11] Access to prenatal care that includes necessary nutritional supplements provides the most effective means to prevent, diagnose, and treat many of the problems listed above in a timely fashion in order to improve fetal growth.[12]

Low birth weight babies are more likely to become malnourished children and are more susceptible to illness and disease. Even infants born with adequate birth weight may become malnourished if inappropriate, inadequate or insufficient foods are provided in early childhood. Indeed, stunting and wasting are major problems in the developing world as over 32 percent of children under age 5 are stunted and 9.4 percent are wasted (WHO, 2000). Micronutrient deficiency is also a major concern among young children (Johnston, 1998). For example, anemia affects approximately 42 percent of preschool children and 56 percent of school age children in the developing world (WHO, 2000). The functional consequences of anemia include impaired psychomotor development and coordination, low scholastic achievement, and decreased physical activity (Politt, 1997).

PROGRESA tries to minimize LBW and subsequent child health and nutrition problems by tying the cash transfers to participating in nutrition programs, preventive medical care, and education programs. Children and pregnant and lactating women are required to participate in growth monitoring programs where they receive supplements equivalent to 100 percent of recommended daily micronutrient requirements and 20 percent of recommended protein. Each month pregnant and lactating women and children age 4-24 months are given enough supplements for one dose per day. Children age 25-60 months who are found to be malnourished during the growth monitoring visits also receive the supplements.

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[8] Lagerstrom *et al*, 1994; Martorell, 1998; Wiliams *et al*, 1992; Westwood, *et al* 1983.
[9] Chandra *et al*, 1977; Victoria, *et al*. 1988; Carter and Gill 1994; Godfrey *et al* 1994; Philips *et al* 1993.
[10] de Onis *et al* 1998; Huffman *et al*, 1999; Ramakrashnan and Neufeld, in press.
[11] Kramer, 1987 and 1998; McGregor *et al* 1983, Foster-Rosales, 2000.
[12] Kambarami *et al*, 1999, Alexander *et al*, 1996*,* Leveno *et al*., 1985, and Kogan *et al* 1994.

In addition, the cash transfer is conditional on all family members obtaining preventive health care visits. Pregnant women are required to have 5 prenatal care visits starting in their first trimester. Children less than 24 months are required to visit the clinic every 2 months for growth monitoring, immunizations, and well baby care. Conventional wisdom is that growth monitoring has a high payoff because it increases parents' awareness that their children suffer from malnutrition at an early stage before long-run damage can set in. Children between 24 and 60 months are required to visit every 3 months for growth monitoring, well-child care, and immunizations. Lactating women are required to have 2 visits a year where their nutrition is monitored and they obtain family planning information and they have physical checkups.

Other adolescents and adults are also required to visit clinics once a year for physical checkups. During these checkups special attention is paid to family planning, the detection and treatment of parasites, the detection and treatment of arterial hypertension and diabetes mellitus, and the detection and treatment of cervical cancer. The visits also include education about health habits, hygiene accident prevention, and first aid treatment.

Finally, all adult family members must also participate in regular meetings at which health, hygiene, and nutrition issues and best practices are discussed. Physicians and nurses, specially trained in these topics, conduct these sessions.

Eligibility, Take-up and Monitoring

The program identified eligible households in two stages. PROGRESA first identified poor communities and then choose poor households within those communities. Poor communities were determined using an index of "marginality". The marginality index was based on the proportion of households living in poverty, population density, and infrastructure such as access to health and education facilities.

In the second stage, *PROGRESA* identified poor households through a proxy means test (PMT). The idea was to construct an index of easily observed characteristics that proxy for poverty such as housing materials, water and sanitation facilities, education, and family structure. PROGRESA conducted a census of households in each poor (*PROGRESA* eligible) community to collect these proxy characteristics. The weights used to aggregate the characteristics into an index were constructed based on the analysis of an in-depth survey of a sample of households from the eligible communities. This sample survey collected consumption and income information in addition to the proxy characteristics. These data were used to estimate a regression of per capita consumption against the proxy characteristics. The regression coefficients were used as weights to construct the index for each household.

In November 1997, *PROGRESA* conducted the census of socio-economic conditions of rural Mexican households the chosen communities to determine which households would be eligible for benefits. On average 78% of the households in the chosen communities were classified as eligible for program benefits. Households did not have to apply, but rather were informed whether they were eligible. Households were

informed of their eligibility using door-to-door methods. In the end PROGESSA achieved a remarkable take-up rate of 97 percent.[13]

When PROGRESA certified eligibility in a community, eligible households were offered enrollment for a limited period time. Once enrolled, households were to receive benefits for a three-year period conditional on meeting the health care requirements. Once the enrollment period closed, new household were not able to enroll in the program until the next certification three years later. This prevented households from migrating into the communities for PROGRESA benefits.

Once enrolled in the program, households received the cash transfers every two months. In order to minimize corruption and improve the transparency of the cash transfer, PROGRESA established offices outside the normal government bureaucracy to manage the cash transfers. Publishing lists of households who were to receive the benefits also improved local accountability. Household surveys confirm that the vast majority of cash ended up in the hands of the intended beneficiaries.

In order to transfer the cash, PROGRESA needed to verify that households actually completed the behavioral health care activities. Medical providers were required to certify that house members actually completed the requirements. About 1 percent of households were denied the cash transfer for non-compliance.

2. EXPERIMENTAL DESIGN

The evaluation of *PROGRESA* was conceived from the beginning to be part of the design and implementation of the program. For budgetary and administrative reasons, all of the eligible communities could not be brought into the program at the same time. Instead, communities had to be phased into the program over at two-year period. The program took advantage of this phasing in to create treatment and control groups through random assignment.

Approximately 10 percent (506) of the 50,000 *PROGRESA* communities were chosen to participate in the evaluation. The experimental communities have the same index level of community poverty and are located in the seven states that were among the first states to receive *PROGRESA*, including Guerrero, Hidalgo, Michoacán, Puebla, Querétero, San Luis Potosi, and Veracruz. These communities were randomly assigned to either a treatment group that would receive PROGRESA benefits immediately or a control group that would be given benefits 2 years later. Of 506 experimental communities, 320 were assigned to the treatment group and 185 to the control group. The treatment and control groups had statistically indistinguishable characteristics, such as age, education and income (Behrman and Todd, 1999), which suggests that randomization of communities into control and treatment groups were successfully implemented.

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[13] Skoufias, Davis and Behrman (1999) provide a detailed description of the targeting procedures and demonstrate that *PROGRESA* did a good job of separating poor households from non-poor households.

In the summer of 1998, all eligible households living in treatment localities were offered *PROGRESA* and almost all (97%) enrolled in the program. In localities assigned to the control group, none of the households received *PROGRESA* benefits nor were they informed that *PROGRESA* would provide benefits to them at a later date. Most of the control communities were incorporated into *PROGRESA* in the summer of 2000, approximately two years after the treatment group.

3. DATA SOURCES

We use three sources of data for the analysis. The first source are utilization data from the administrative records of public clinics operated by IMSS-Solidaridad. These data will be used to analysis the impact of PROGRESA on visits to public health clinics. These analyses will be compared to similar analyses using household survey data in order to assess the accuracy of the household reports.

The second source of data is a large-scale panel survey of a random sample of PROGRESA eligible households from control and treatment communities. In November 1997, *PROGRESA* conducted a census of the socio-economic conditions of rural Mexican households in the experimental communities to determine which households would be eligible for benefits. Using *PROGRESA*'s beneficiary selection methods, households were classified as eligible and non-eligible for participation in the program in both treatment and control communities. On average 78% of the households in an experimental locality were classified as eligible for program benefits. A random sample of about 14,500 households was chosen from *PROGRESA* eligible households in control and treatment localities.

PROGRESA then conducted a baseline survey in March 1998 before the initiation of benefits in May 1998. Four follow-up surveys were conducted in approximately six-month intervals after beneficiary households started receiving benefits from *PROGRESA*. Unfortunately little health or health care utilization data was collected in the baseline. However, extensive health and health care utilization data were collected in the follow-up surveys and form the basis of our analysis. In addition, the surveys asked a number of core questions about the demographic composition of households and their socio-economic status.

The baseline sample includes 89,293 individuals from 14,488 households in 506 experimental communities. Approximately, 60 percent of the sample comes from treatment areas and 40 percent from control. Sample sizes by round and attrition rates are reported in Table 1. Attrition is extremely compared to other large panel surveys. Between the baseline and the third follow-up round 20 months later, 5.5 percent of the households and 5.1 percent of the individuals had dropped from the sample. More importantly, there seems to be no difference in attrition between the control and treatment areas, suggesting no attrition bias in the analysis.

To check the success of the randomization in balancing control and treatment groups, we present descriptive statistics in tables 2a and 2b disaggregated by control and treatment groups. The sample in table 2a consists of children age 0-5 at baseline and the sample in table 2b consists of adults age 18 and over. At baseline there is no difference in illness rates between control and treatment groups. Overall in both the child and adult samples, there is little difference in family demographics or economic status and little difference in labor markets, as the agricultural wages are close across control and treatment localities. However, some of these differences are statistically significant. This is not due to the fact the differences are large, but rather due to the large sample sizes. For example, the difference in community agricultural wage rates is only 2-3%, but is significantly different. The one characteristic in which there appears to a large difference is whether households have electricity. Households in control areas are 11 to 18 percent more likely to have electricity than in treatment areas. Overall, the evidence suggests that households in treatment areas are very slightly worse if at all. This analysis suggests that the randomization adequately balanced the control and treatment groups on observed characteristics. Indeed, as we will present later, controlling for these observed characteristics does not change the estimated effects of *PROGRESA* on utilization and outcomes.

3. UTILIZATION

In this section, we examine the impact of *PROGRESA* on health care utilization. At first blush, one would expect visits to public health clinics to rise for two reasons. First, *PROGRESA* provided monetary transfers for nutrition that are tied to the verification that household members attended preventive visits in the public health clinics. Second, *PROGRESA* monetary transfers for nutrition could be used for purchasing medical care.

However, there are other reasons why we might not to see an increase in visits to public clinics. First, if *PROGRESA*'s preventive interventions succeeded, then there should have been less illness, and therefore a lower demand for curative medical care. Another reason why we might not see an increase is that the number of public clinic visits by *PROGRESA* beneficiaries may have already outnumbered those required to obtain *PROGRESA* benefits. In addition, if PROGRESA reduced illness, we should see a reduction in the hospital inpatient stays and in the number of visits to private providers who are used primarily for curative purposes.

Visits to Public Clinics

We investigate the impact of PROGRESA on visits to public clinics first by using data from the administrative records of public clinics operated by IMSS-Solidaridad. There are 3,541 clinics and the data includes monthly information from January 1996 to December 1998. This information is complimented by the records of *PROGRESA* on the number of beneficiary families incorporated to the Program every month in each clinic. About two-thirds of the clinics are in *PROGRESA* areas, with the remaining one-third operating in control areas.

Figure 1 graphs average daily visits to a public health clinic in *PROGRESA* and non-PROGESA localities by month over time. The visit rates in the control and treatment areas are almost identical until the last quarter of 1997, when *PROGRESA* was beginning to be introduced in a number of localities. Beginning in the fourth quarter of 1997, visit rates to clinics in *PROGRESA* localities are on average higher than in non-*PROGRESA* localities, and the difference grows over time as more *PROGRESA* localities begin to provide benefits.

The corresponding average daily visit rates by treatment and control localities by year are presented in Table 3. In 1996, the year before PROGESA began, average visits to clinics were identical in control and treatment localities. However, in 1998, the first full year in which *PROGRESA* was operational in all treatment localities, visit rates to clinics in PROGESA communities were 12 percent higher than in clinics in control communities. This is consistent with the hypothesis that PROGESA increased utilization at public health clinics.

We next estimate the impact of *PROGRESA* on visits to public health clinics using a difference-in-difference estimator with facility-level panel data. The difference-in-difference specification compares the change (before and after *PROGRESA*) in visits per day in treatment localities with the corresponding change in control localities. By looking at the change over time, we are controlling for characteristics that do not change over time within control and treatment localities and for characteristics that change over time and are common to control and treatment areas. Thus, the difference-in-difference estimator controls for area specific characteristics and secular trends that might confound the estimated impact of *PROGRESA* on visits to public facilities.

The difference-in-difference model can be specified in regression form as:

$$Y_{it} = \alpha_i + \gamma_t + \beta T_i P_t + \sum_j \phi_j X_{it} + \varepsilon_{it} \qquad (1)$$

The dependent variable is the visits per day in facility *month and I t.* The right hand side variables include a fixed effect for each clinic (α_i), a fixed effect for each month (γ_t), and an interaction of a variable indicating whether the facility is in a treatment locality (T_i) and an indicator of whether it is the post-reform period (P_t). We also control for the size of the facility using the number families in the clinic's service area (X_{it}).

The difference-in-difference model makes the counterfactual assumption that absent an intervention, visits to clinics in treatment localities would grow at the same rate as in the control localities. While this assumption is not directly testable, we can test whether the visits to clinics in the treatment localities and in the control localities were growing at the same rate in the pre-intervention period. If we do find comparable growth rates, it would suggest that our counterfactual assumption is likely to be correct, unless there were other interventions, contemporaneous with *PROGRESA*, which were differentially implemented in treatment and control localities. We test this hypothesis and cannot reject the hypothesis that visits per day were growing at the same rate in control and treatment localities before the intervention. This result is also evident from Figure 1. In addition, not only are the pre-intervention growth rates the same, but also the level of pre-intervention visits is the same in treatment and control clinics (Table 3).

These difference-indifference results are reported in Table 4. The results indicate that there were about 2.09 more visits per day or about 18.2 percent more visits to clinics in *PROGRESA* areas than in non-*PROGRESA* areas. However, *PROGRESA* beneficiaries comprise only about one-third the total number of families in a clinic's service area. If all of the increase can be attributed to *PROGRESA* families, then visits by *PROGRESA* families increased by about 60 percent.

Visits By Provider Type

We now turn to examining the impact of *PROGRESA* on total utilization (i.e. visits to all provider types) and by provider type (public clinic, public hospital, private provider) using data from the third, fourth, and fifth waves of the household surveys. Questions pertaining to health care utilization were not asked in the first two waves, so there is no pre-intervention baseline information. Information was collected on health care utilization over the four weeks prior to the interview including the number inpatient stays in a hospital, visits to a public health clinic, and visits to a private a private provider. Table 5 presents the summary statistics on the utilization of a health care provider by the poor and non-poor in the treatment and control areas.

Overall, health care utilization of poor-rural Mexico is extremely low. On average, rural Mexicans make less than one visit to a medical provider per year. Overall they make about 0.72 visits per person per year. Disaggregating to the geographic area, we find a higher visit rate for the poor in the treatment areas than in the control areas. Most individuals in all age groups opt to receive treatment from standard health care institutions, with public institutions receiving more than double the visits of private doctors and private hospitals combined. Indeed, the majority of health care utilization occurs at public clinics for all age groups.

Now we turn to testing whether utilization is higher among *PROGRESA*-eligible individuals in treatment areas by comparing mean visit rates across control and treatment regions controlling for socio-economic differences. We estimate the following equation on those eligible for *PROGRESA* benefits in control and treatment communities:

$$Y_i = \alpha + \beta T_i + \sum_j \phi_j X_{ji} + \varepsilon_i \qquad (2)$$

where Y_i is individual i's number of visits in the month prior to the survey, T_i is an indicator of whether the individual lives in a treatment locality—i.e. in which *PROGRESA* is available, and the X_i's are individual and household controls. The controls include variables such as, age, sex, education, ability to speak Spanish, assets and community wage rates. When the observation is the child, we use the parental education and Spanish.

We estimate a number of different versions of equation (2) using random effects. There are four dependent variables: total visits, public clinic visits, public hospital visits, and private provider visits. We estimate each of the four models separately for each age group. Table 6 presents the results. Each entry in the Table reports the estimated impact of *PROGRESA* from a different regression and includes the corresponding information

from the total visits regression models. We only report the coefficient on the treatment dummy. Each regression model includes the additional covariates mentioned above.

The first row reports the effect of *PROGRESA* on total visits to all providers and the last 3 rows report the effects of *PROGESA* on visits to public clinics, hospitals and private providers, respectively. Within columns the coefficients in the last three rows sum to the coefficient in the first row. The standard errors and t-statistics are corrected for multiple observations on each village.

We first check to see that the estimated impact on public clinic visits is consistent with those found in the facility level analysis. The second row reports the impact of *PROGRESA* on utilization of public clinics. While there is a negative impact on children 0-2's utilization of public clinics, *PROGRESA* seems to have increased the utilization of all of the other age groups. Comparing the coefficients to baseline utilization suggests that *PROGRESA* increased utilization at public clinics by 53 percent overall. This is very close to 60 percent increase found in the facility level analysis.

The results are also consistent with the hypothesis that PROGRESA lowered illness and therefore demand for curative care. For 0-2 year olds, the point estimates suggest that total visits fell by PROGRESA beneficiaries are 25 percent lower than for non-beneficiaries. More importantly hospital inpatient stays of PROGRESA 0-2 year olds are fell by more than half and visits to private doctors all fell a third. Similarly, we find a very large reduction in hospitalization for individuals age 18-50 and for the over 50 group. This results are consistent with the hypothesis *PROGRESA* had a positive impact on health status. We will return to this issue explicitly later in section 4, where we examine the impact of *PROGRESA* on directly on measures of health outcomes.

4. HEALTH OUTCOMES

PROGRESA was designed as a method of improving the living standards of the segment of the Mexican population classified as poor. One means of this betterment is through investing in early childhood health care to combat the incidence of illness and improve nutritional status. For adults, the cash transfer is intended for families to use to purchase food and required preventive visits to higher quality facilities are intended to improvement health outcomes. In this section, we examine the impact of *PROGRESA* on health outcomes.

Child Health

We begin by examining the impact of *PROGRESA* on the probability that a mother reports that her child experienced an illness in the 4 weeks prior to the survey. There is some concern that such variables report illness with error. Specially, different individuals define illness differently, so what would be an illness for one family is not for another. We are able to control for this reporting bias through the randomization. As indicated in Table 2b, the mean self-reported illness is the same in control and treatment groups in the pre-intervention baseline survey.

However, a mother's definition of illness might have been changed for the treatment group as a result of the intervention. Specifically, during the growth monitoring and well baby visits mothers are educated about health and nutrition and might be more likely to call a problem an illness than before the intervention. In this case treatment mothers might be more likely to report an illness than a control mother holding the true level of illness constant. Therefore, our results are likely to be a lower bound estimate of the impact of PROGRESA.

We focus only on those children less than three years old at the baseline as any child older than three would not have received *PROGRESA* benefits for a long enough duration to see a measurable impact. Thus, the age 0 cohort indicates those children that would have been age 0 in November 1997, the date at which the baseline survey was conducted. The definition of the newborn cohort, however, differs as this cohort are those children aged 0 who, despite observing their parents in the sample in the previous wave, are themselves not observed.

Table 8 presents the summary statistics of the characteristics of the control and treatment areas for each of the age cohorts. The second column indicates the length of time that the individual could have received *PROGRESA* benefits, as measured by observed entry into the sample. In the treatment areas, the poor children tend to get sick less and for fewer days than the non-poor. The data in this table is pictured in Figure 2. Illness rates are the same in baseline period across control and treatment groups for the age 1 and age 2-3 cohorts. Illness rates in both treatment and control areas fall over time. However, the illness rate falls faster in the treatment areas than in the control areas for those children receiving *PROGRESA* benefits longer than seventeen months.

We estimate random effects models of the impact of *PROGRESA* on the probability of illness, conditioning on the incidence of sickness at the baseline or the first period observed in the sample. In Model 1, we estimate the difference between the treatment and control areas. Models 2 and 3 break apart the differences in duration for the treatment and control areas, allowing the treatment effect to vary with length of time the individual received benefits. In Model 3, we impose the restriction that the impact was the same across durations greater than five months. This hypothesis cannot be rejected for any of the models.

Table 9reports the results. We find that the impact of *PROGRESA* on the probability of a child getting ill is negative and significantly different from zero. Across cohorts, *PROGRESA* did not significantly lower illness rates until the child had been receiving benefits for at least 12 months. *PROGRESA* lowered illness rates for beneficiaries in the age 0 cohort by about 3.6 percentage points or 13 percent lower than baseline illness and for those in the age1 cohort, by 3.5 percentage points or 14 percent lower. Furthermore, *PROGRESA* lowered illness rates for beneficiaries in the age 2-3 cohort 2.9 percentage points or 14 percent lower than baseline.

Adolescent and Adult Health Status

While little of *PROGRESA* was targeting to improving adult health, there is reason to believe that adult health might improve as well. First, adults were required to obtain one preventive health care visit per year. Second, 70 percent of the income

transfer was used to increase food availability in the household both in terms of quantity -–calories-- and quality--richer in protein and micronutrients (Hoddinott and Skoufios, 2000).

Health status is directly related to nutritional intake. Adequate energy intakes are essential for maintaining health and productivity. Long term deprivation leads to chronic energy deficiency (CED), defined as 'a steady state at which a person is in energy balance although at a 'cost' either in terms of risk to health or as an impairment of function and health' (James, Ferro-Luzzi, and Waterlow 1988 p. 969). CED has been associated with a greater risk of illness, and lower physical activity levels[14].

In the last three rounds of the survey, adolescents and adults were asked a series of questions regarding their health status. All individuals 18 and above where asked how many kilometers that were able to walk without getting tired, and The following questions were asked of individuals 6 and older

In the past 4 weeks, how many days did you have difficulty performing daily tasks (such as going to work, doing housework, going to school, caring for your children) due to illness?

In the past 4 weeks, how many days were you not able to perform daily tasks (such as going to work, doing housework, going to school, caring for your children) due to illness?

In the past 4 weeks, how many days were you in bed due to illness?

The means and standard deviations for these variables are presented in Table 10 Note that the days lost due to illness increases with age and the differential between control and treatment groups also increases with age.

We estimate equation (2), where the dependent variables are the health status measures and the independent variables are a dummy indicating whether the individual was in a *PROGRESA* village as well as the age, sex and education of the individual and household assets.

Table 11reports the results of the estimation. As in the earlier tables, we only report the coefficient on the treatment variable for each model. We find no effect of *PROGRESA* on individuals age 6-17. This is not surprising since this is generally a healthy group to start with. However, for the age group (18-50) we find a significant reduction in the number of days of difficulty with daily activities due to illness and a significant increase in the number of kilometers able to walk without getting tired. Specifically, *PROGRESA* beneficiaries have 12 percent fewer days of difficulty due to illness than non-*PROGRESA* individuals, and are able to walk about 4 more without getting tired. For those over 50, *PROGRESA* beneficiaries have significantly fewer days of difficulty with daily activities, days incapacitated, and days in bed due to illness than do non-beneficiaries. Moreover, they are able to walk more kilometers without getting tired. Specifically, *PROGRESA* beneficiaries has 20 percent fewer days of difficulty with

[14] See, for example, Deolalikar 1988; Durnin 1994; Ferro-Luzzi et al. 1992; Garcia and Kennedy 1994; Immink and Viteri 1981; Kennedy and Garcia 1994; Kusin, Kardjati, and Renqvist 1994.

daily activities, 18 percent fewer days incapacitated, 17 percent fewer days in bed, and are able to walk about 3 percent more than non-beneficiaries.

5. SUMMARY AND CONCLUSIONS

In this paper, we investigated the impact of *PROGRESA* on health. *PROGRESA* combines a traditional cash transfer program with financial incentives for families to invest in the human capital of their children. Program benefits include cash transfers that are disbursed conditional on the household engaging in a set of behaviors designed to improve health and nutrition. The family only receives the cash transfer if: (i) every family member accepts preventive health services; (ii) children age 0-5 and lactating mothers attend nutrition monitoring clinics where their growth is measured, they obtain nutrition supplements, and they receive education on nutrition and hygiene; and (iii) pregnant women visit clinics to obtain prenatal care, nutritional supplements, and health education. An additional cash transfer is given to households with school age children if the children are enrolled and attend school. The size of the cash transfer is large, corresponding on average to about one-third of household income for the beneficiary families. Another unique feature of the program is that the cash transfers are given to the mother of the family, a strategy designed to target the funds within the household to improving her children's education and nutrition.

Our analysis takes advantage of a controlled randomized design. In 1998, 506 of the 50,000 *PROGRESA* villages were randomly assigned to control and treatment groups. Eligible households in treatment villages received benefits immediately, while benefits for eligible households in control villages were postponed until after the year 2000. A pre-intervention baseline survey of approximately 19,000 households with over 95,000 individuals and four follow-up surveys (at six month intervals) of the same households were conducted over the two-year experimental period.

We find that the utilization of public health clinics increased faster in *PROGRESA* villages than in control areas relative to control villages. In addition, we also find an increase in nutrition monitoring visits. This is not surprising given that households must go to public clinics for preventive care and nutrition monitoring. At the same, however, the utilization of public hospitals fell. This is consistent with the hypothesis that PROGESA's incentives for preventive care and nutrition improved health and lowered the incidence of severe illness. Moreover, there was no reduction in the utilization of private providers, suggesting that the increase in utilization at public clinics was not substituting public care for private care.

We also found a significant improvement in the health of *PROGRESA* beneficiaries—both children and adults. Specifically, we find that *PROGRESA* children 0-5 have a 12 percent lower incidence of illness than non-PROGESA children. In addition, *PROGRESA* children's weight for height, a measure of wasting and short-term health, significantly improved.

We also found that *PROGRESA* adults were significantly healthier. Prime age *PROGRESA* adults (18-50) had a significant reduction in the number of days of difficulty with daily activities due to illness and a significant increase in the number of kilometers

able to walk without getting tired. Specially, PRORGRESA beneficiaries have 19 percent fewer days of difficulty due to illness than non-*PROGRESA* individuals, and are able to walk about 7.5 percent more without getting tired. For those over 50, *PROGRESA* beneficiaries have significantly fewer days of difficulty with daily activities, days incapacitated, and days in bed due to illness than do non-beneficiaries. Moreover, they are able to walk more kilometers with out getting tired. Specifically, *PROGRESA* beneficiaries has 19 percent fewer days of difficulty with daily activities, 17 percent fewer days incapacitated, 22 percent fewer days in bed, and are able to walk about 7 percent more than non-beneficiaries.

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Table 1: Household and Individual Sample Sizes and Attrition Rates

	March 98	Oct 98	May 99	Nov 99	May 00
Number of Households					
Treatment	7926	7690	7381	7179	7142
Control	6,562	6239	5873	6029	5825
Total	14,488	13,929	13,254	13,208	12,967
*Household Attrition Rate From Baseline**					
Treatment		-0.008	-0.009	-0.016	-0.055
Control		-0.015	-0.013	-0.012	-0.055
Total		-0.011	-0.011	-0.014	-0.055
Number of Individuals					
Treatment	45675	44531	42656	41591	41377
Control	35618	34165	32179	33055	32033
Total	81,293	78,696	74,835	74,646	73,410
*Individual Attrition Rate From Baseline**					
Treatment		-0.007	-0.010	-0.016	-0.051
Control		-0.012	-0.013	-0.012	-0.050
Total		-0.009	-0.011	-0.014	-0.051

Table 2a: Descriptive Statistics at Baseline for Children Age 0-5

	Treatment		Control		Difference		
	Mean	**St. Dev.**	**Mean**	**St. Dev**	**Level**	**%**	**t-stat**
Ill last month (=1)	0.329	(0.470)	0.326	(0.469)	0.002	0.6%	(0.220)
Age	1.628	(1.099)	1.612	(1.110)	0.016	1.0%	(0.630)
Male (=1)	0.511	(0.500)	0.488	(0.500)	0.023	4.5%	(1.970)
Father's Years of Education	3.480	(2.746)	3.810	(2.884)	-0.330	-9.5%	(-5.060)
Mother's Years of Education	3.612	(2.820)	3.608	(2.915)	0.004	0.1%	(0.060)
Father Speaks Spanish (=1)	0.913	(0.282)	0.892	(0.311)	0.021	2.3%	(3.110)
Mother Speaks Spanish (=1)	0.927	(0.261)	0.910	(0.287)	0.017	1.8%	(2.650)
Own House (=1)	0.924	(0.265)	0.915	(0.278)	0.009	1.0%	(1.410)
Electricity (=1)	0.646	(0.478)	0.719	(0.450)	-0.073	-11.3%	(-6.860)
Hectares of Land Owned	0.814	(0.973)	0.790	(1.001)	0.023	2.8%	(1.020)
Male Agricultural Wage	23.071	(6.970)	23.494	(7.008)	-0.423	-1.8%	(-2.630)
Female Agricultural Wage	20.614	(6.821)	21.240	(7.024)	-0.625	-3.0%	(-3.910)
Sample Size	4,444		3,259				

Table 2b: Descriptive Statistics at Baseline for Adults Age 18 and over

	Treatment		Control		Difference		
	Mean	**St. Dev.**	**Mean**	**St. Dev**	**Level**	**%**	**t-stat**
Age	37.468	(16.382)	38.761	(17.299)	-1.293	-3.5%	(-7.400)
Male	0.486	(0.500)	0.479	(0.500)	0.007	1.4%	(1.330)
Years of Education	3.236	(2.966)	3.356	(3.039)	-0.120	-3.7%	(-3.850)
Speak Spanish (=1)	0.939	(0.240)	0.936	(0.245)	0.003	0.3%	(1.150)
Own House (=1)	0.947	(0.223)	0.943	(0.232)	0.004	0.4%	(1.820)
Electricity (=1)	0.634	(0.482)	0.751	(0.432)	-0.117	-18.5%	(-24.880)
Hectares of Land Owned	0.951	(1.023)	0.941	(1.082)	0.010	1.1%	(0.930)
Male Agricultural Wage	23.334	(7.396)	23.888	(6.871)	-0.554	-2.4%	(-7.520)
Female Agricultural Wage	20.873	(7.185)	21.603	(6.941)	-0.730	-3.5%	(-10.000)
Sample Size	20,456		17,180				

Table 3: Mean Consultations at Public Clinics Per Day

	NON-*PROGRESA* Communities	*PROGRESA* Communities
1996	9.13 (8.25)	9.11 (7.98)
1998	11.48 (9.80)	12.84 (11.32)

Notes: Standard deviations are presented in parentheses.

Table 4: Difference-in-Difference Estimates of Impact of *PROGRESA* on Total Daily Consultations Per Clinic

PROGRESA (=1)	2.09 (0.067)
Total Number of Families in Clinic's Service Area	2.30 (0.198)
F-Statistics For Clinic Fixed Effects	46.54 (P=0.00)
F-Statistics For Month Fixed Effects	122.02 (P=0.00)
Sample Size	126,665

Notes: Standard Errors in Parentheses

Table 5: Means and Standard Deviations of Visits to Medical Care Providers

Age	Sample	Mean Visits Per Month				Sample Size
		Total Visits	Public Clinics	Hospitals (Inpatient)	Private Providers	
0 to 2	*PROGRESA*	0.085 (0.390)	0.061 (0.319)	0.005 (0.156)	0.019 (0.167)	6,974
	Non-*PROGRESA*	0.120 (0.531)	0.080 (0.403)	0.011 (0.191)	0.029 (0.299)	6,247
3 to 5	*PROGRESA*	0.085 (0.376)	0.061 (0.319)	0.005 (0.092)	0.018 (0.182)	10,689
`	Non-*PROGRESA*	0.081 0.405)	0.059 (0.330)	0.004 (0.107)	0.018 (0.213)	9,472
6 to 17	*PROGRESA*	0.031 (0.257)	0.023 (0.218)	0.003 (0.087)	0.005 (0.102)	46,967
	Non-*PROGRESA*	0.026 (0.224)	0.018 (0.184)	0.002 (0.058)	0.006 (0.114)	41,214
18 to 49	*PROGRESA*	0.059 (0.370)	0.041 (0.296)	0.005 (0.111)	0.014 (0.191)	44,435
	Non-*PROGRESA*	0.053 (0.362)	0.032 (0.262)	0.007 (0.146)	0.015 (0.203)	43,505
50 +	*PROGRESA*	0.122 (0.596)	0.076 (0.462)	0.011 (0.192)	0.035 (0.330)	11,602
	Non-*PROGRESA*	0.119 (0.543)	0.064 (0.380)	0.015 (0.228)	0.041 (0.323)	14,003

Table 6: Estimates of the Impact of PROGRESA on Health Care Utilization

Dependent Variable	Age 0-2	Age 3-5	Age 6-17	Age 18-50	Age 51+
Total Visits	-0.030 (-3.630)	0.005 (0.800)	0.005 (2.920)	0.006 (2.180)	0.004 (0.550)
Public Clinic Visits	-0.015 (-2.280)	0.001 (0.280)	0.005 (3.260)	0.009 (4.500)	0.015 (2.600)
Public Hospital Visits	-0.006 (-1.930)	0.000 (0.250)	0.001 (2.450)	-0.002 (-2.190)	-0.005 (-1.770)
Private Provider Visits	-0.009 (-2.160)	0.002 (0.830)	-0.001 (-1.540)	-0.002 (-1.120)	-0.005 (-1.140)
Sample Size	12,875	19,852	86,927	86,775	25,309

Notes: This table reports the coefficients and t-statistics for regression models for different age groups, where the numbers of visits to specific provider types are the dependent variables. Each model is estimated for each age group/ provider type, with the main variable of interest, a treatment variable, indicating whether the individual is eligible for *PROGRESA*. The random effects regressions on the village-level include controls for sex, education, number of children, ability to speak Spanish, age dummies, unearned income, male and female agricultural wages, and household assets. For children, age, education, and ability to speak Spanish are replaced by age and education of the mother and father, number of siblings, the sex of the child, number of siblings, whether the child is the eldest and whether the mother and father speak Spanish. These tables can be found in the Appendix. These tables can be found in the Appendix. Robust standard errors, calculated using random effects at the individual level, are used to correct to compute the t-statistics.

Table 7: Child Illness Rates by Age and Treatment/Control

Age at Baseline	Duration on Program	*PROGRESA*	Non-*PROGRESA*
Newborn	Pooled Duration	0.218	0.174
	Sample Size	1,527	975
Age 0	6 months	0.310	0.299
	12 months	0.234	0.266
	18 months	0.236	0.242
	24 months	0.205	0.212
Age 1	6 months	0.255	0.269
	12 months	0.181	0.205
	18 months	0.207	0.205
	24 months	0.192	0.204
Age 2-3	6 months	0.217	0.213
	12 months	0.169	0.196
	18 months	0.159	0.176
	24 months	0.122	0.129

Table 8: Estimates of *PROGRESA* Program Impact on Children's Incidence of Illness

Cohort	Model 1	Model 2				Model 3		Sample Size
		6 months	12 months	18 months	24 months	6 months	12+ months	
Newborn	-0.051 (-2.300)							1,501
Age 0	-0.010 (-1.040)	0.015 (0.910)	-0.037 (-2.130)	-0.027 (-1.600)	-0.048 (-1.970)	0.015 (0.910)	-0.036 (-2.820)	7,718
Age 1	-0.023 (-2.680)	-0.031 (-1.850)	-0.031 (-1.830)	-0.028 (-1.650)	-0.061 (-3.070)	-0.031 (-1.870)	-0.035 (-3.180)	8,981
Age 2-3	-0.017 (-3.190)	-0.003 (-0.240)	-0.035 (-3.260)	-0.027 (-1.890)	-0.035 (-2.810)	-0.002 (-0.230)	-0.029 (-4.120)	19,430

Notes: This table reports the coefficients and t-statistics for difference-in-difference regression models of the impact of *PROGRESA* on the incidence of illness for different age groups. Each of the coefficients reported are whether the individual is eligible for *PROGRESA* benefits and the length of time the individual has been in the survey. Included in the model, but not reported, are dummies indicating the round of the observation. The random effects regressions on the individual-level include controls for age and education of the mother and father, number of siblings, the sex of the child, number of siblings, whether the child is the eldest, unearned income, male and female agricultural wages, household assets, and whether the mother and father speak Spanish. For the regression on the Age 1 cohort, the results indicated here do not include the above mentioned controls as these controls did not significantly control for any variation. These tables can be found in the Appendix.

Table 9: Means and Standard Deviations of Adult Health Measures

	Age 6-17		Age 18-50		Age 51+	
	Treatment	Non-Progresa	Progresa	Non-Progresa	Progresa	Non-Progresa
Days of Difficulty with Daily Activities Due to Illness	0.100 (1.340)	0.119 (1.444)	0.335 (2.683)	0.389 (2.906)	1.972 (6.801)	2.538 (7.727)
Days Incapacitated Due to Illness in last 4 Weeks	0.090 (1.328)	0.091 (1.276)	0.281 (2.477)	0.321 (2.638)	1.704 (6.328)	2.197 (7.228)
Days in Bed Due to Illness in last 4 Weeks	0.054 (0.994)	0.058 (0.986)	0.193 (2.009)	0.208 (2.072)	1.209 (5.355)	1.527 (6.051)
Kilometers Can Walk Without Getting Tired			5.046 (3.622)	4.820 (3.226)	2.971 (2.878)	2.803 (3.105)
Sample Size	41,634	36,250	38,271	37,564	11,124	13,540

Table 10: Estimates of *PROGRESA* Program Impact on Adult Health by Age

Dependent Variable	Age 6-17	Age 18-50	Age 51+
Days of Difficulty with Daily Activities Due to Illness	-0.017 (-1.590)	-0.047 (-2.190)	-0.504 (-4.810)
Days Incapacitated Due to Illness in Last 4 Weeks	0.002 (0.190)	-0.032 (-1.630)	-0.412 (-4.190)
Days in Bed Due to Illness in Last 4 Weeks	-0.002 (-0.340)	-0.010 (-0.680)	-0.268 (-3.260)
Kilometers Can Walk Without Getting Tired		0.203 (7.680)	0.092 (2.260)
Sample Size	76,827	74903	24389

Notes: This table reports the coefficients and t-statistics for regression models for different age groups. Each of the coefficients reported are whether the individual is eligible for *PROGRESA* benefits and the length of time the individual has been in the survey. Included in the model, but not reported, are dummies indicating the round of the observation. The random effects regressions on the village-level include controls for age and education of the mother and father, number of siblings, the sex of the child, number of siblings, whether the child is the eldest, unearned income, male and female agricultural wages, household assets, and whether the mother and father speak Spanish. These tables can be found in the Appendix.

Figure 1: Daily Visits to Public Clinics