Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment

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One of the greatest tragedies of extreme poverty is its intergenerational transmission. Children who grow up in poor families tend to be in poorer health and have lower levels of education. They thus enter adulthood without "the basic capabilities" necessary to take advantage of labor-market opportunities to pull themselves out of poverty and to enjoy an acceptable quality of life (Amartya Sen, 1999). As a result, children from poor families begin life at a distinct disadvantage.

In an effort to improve the circumstances in which children from poor families start out life, the Mexican government has spent considerable resources developing an anti-poverty program called PROGRESA. This program combines a traditional cash-transfer program with financial incentives for positive behavior in health, education, and nutrition. Specifically, cash transfers are disbursed conditional on the household engaging in a set of behaviors designed to improve health and nutrition, including prenatal care, well-baby care and immunization, nutrition monitoring and supplementation, preventive checkups, and participation in educational programs regarding health, hygiene, and nutrition. An additional cash transfer is given to households with school-age children if the children are enrolled and attend school. While financial incentives to encourage good health behavior have been used in Finland and France, PROGRESA is, at least to my knowledge, the first such program in a developing country.

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In this paper, I investigate the impact of PROGRESA on child health outcomes including morbidity, height, and anemia. The analysis takes advantage of a controlled randomized design.

I. The Intervention

PROGRESA began in 1997 as a national program designed to address the immediate needs of extreme poverty and break its intergenerational transmission. Over its first three years, PROGRESA extended benefits to approximately 2.6 million families in 50,000 rural villages, which is about 40 percent of rural families and 10 percent of all families in Mexico.

PROGRESA determined household eligibility in two stages, first by identifying underserved communities and then by choosing low-income households within those communities (Emmanuel Skoufias et al., 1999). On average, 78 percent of the households in selected communities were classified as eligible for program benefits. All eligible households living in treatment localities were offered PROGRESA, and almost all (93 percent) enrolled in the program.

Every two months PROGRESA families receive a cash transfer typically worth about 20 to 30 percent of household income if the following conditions are met.

(i) Children of age 0–23 months get immunized and visit nutrition monitoring clinics every two months where they get well-baby care, their growth is measured, they obtain nutrition supplements worth 100 percent of daily recommended micronutrients and 20 percent of protein, and their parents receive education on nutrition, health, and hygiene.

(ii) Children of age 24–60 months attend nutrition monitoring clinics every four months where their growth is measured, they obtain nutrition supplements if their growth is assessed as poor, and they re-

ceive education on nutrition, health, and hygiene.

(iii) Pregnant women visit clinics to obtain prenatal care, nutritional supplements, and health education. They are required to have five prenatal care visits starting in their first trimester.

(iv) Lactating women visit clinics to obtain postpartum care, growth monitoring, nutrition supplements, and education about health, nutrition, and hygiene.

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

(vi) All adult family members participate in regular meetings at which health, hygiene, and nutrition issues and best practices are discussed. Female head of households are required to attend bi-monthly meetings, while other adults have to attend once a year. Physicians and nurses specially trained in these topics conduct these sessions.

II. Experimental Design

The analysis takes advantage of a control-randomized design implemented by the Government of Mexico. Due to budgetary and logistical constraints, the government was unable to enroll all eligible families simultaneously. Rather, it needed to phase in enrollment over a period of time. For logistical reasons, the government decided that it would enroll whole villages at a time and that it would enroll them as fast as possible so that no eligible household would be kept out of the program if money was available. Because equity requires giving every eligible village an equal chance of receiving the benefits first, the government decided to randomly choose which villages would receive benefits first.

As a result of this process, the government randomly chose 320 treatment and 185 control villages in seven states for a total of 505 experimental villages. Eligible households in treatment villages received benefits immediately starting in August–September 1998, while benefits for eligible households in control villages were postponed for two years. 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.

III. Data

I use three indicators of child health outcomes to assess the impact of PROGRESA. The first measure is child morbidity measured as the mother's report as to whether the child experienced an illness in the four weeks prior to the survey. Child morbidity and socioeconomic characteristics were collected as part of a larger socioeconomic survey of all households in the experimental villages prior to the intervention baseline, again two months after the intervention began, and then three more times at about six-month intervals.

The survey included information that allowed me to apply the program's eligibility criteria and to identify those households that were eligible in treatment areas and those households that would be eligible in control areas. Using this information, I restricted the analysis sample to households eligible for PROGRESA. A treatment household is defined as an eligible household in a treatment village, and a control household was defined as an eligible household in a control village.

The next set of health outcomes measures are based on objective measurements. These include height measured in centimeters, "stunting" (defined as being two or more standard deviations below the age-sex standardized height of a healthy [U.S.] reference population [World Health Organization, 1979]), and anemia (defined as hemoglobin less than 11 g/dL adjusted for altitude using standard adjustments [Guillermo José Ruiz-Argüelles and Antonio Llorente-Peters, 1981]).

These objective health indicators are based on height and hemoglobin. However, because of the cost of collecting these measures, they were only collected in a subsample of the 505 experimental communities. A sample of treatment

communities was randomly selected, and control communities were matched to the treatment communities based on population size, a socioeconomic index (SES), community infrastructure, and geographical location. The objective health information used in this analysis was collected between 1998 and 2000.

IV. Statistical Methods

The randomization and the fact that the control and treatment samples are well balanced in the observed characteristics imply that a simple comparison of mean outcomes post-intervention will likely provide an unbiased estimate of program impacts. However, I also control for other observed socioeconomic characteristics in order to reduce idiosyncratic variation and to improve the power of the estimates.

To test whether morbidity was higher among children in PROGRESA-eligible treatment areas, I estimated a logistic regression of probability of illness with the key independent variable being a dummy indicating whether the individual was in a treatment village that was eligible for PROGRESA. The model also controls for socioeconomic characteristics measured just before the intervention. The specific variables included in the model are the child's age and sex; the mother's and father's ages, years of schooling, and ability to speak Spanish; and household ownership, whether the house had electricity, household income, and average male and female wage rates in the village measured at baseline. The economic variables were measured at baseline because the cash transfer likely affected their values, biasing the estimated impact.

The model allows for an individual random effect because of the multiple observations on the same child across the longitudinal survey, and for a village random effect because of the cluster sampling (Allan Donner and Neil Klar, 2000). Few households had more than one child less than age 3 at baseline. For those households that did, I randomly sampled one child to include in the analysis.

The above specification (model 1) restricts the program impact to be constant with respect to program exposure. I estimate a second specification (model 2) that allows the program impact to vary depending on how long the program has been operating in the village. Specifically, I include separate treatment dummies for six-month program exposure, 12-month exposure, 18-month exposure, and 24-month exposure.

I estimated the model separately for babies born during the intervention period and for children aged 0–35 months at baseline. While both of these cohorts experienced the benefits of the cash transfers, the well-baby care and nutrition monitoring, the nutrition supplements, and the general health, hygiene, and nutrition information provided their parents, the newborn sample also benefited from the prenatal interventions.

For the newborn sample, I use only the observations that first appear in the second follow-up survey (i.e., those whose families have been on the program for 6–12 months at the time of birth). This is to increase the likelihood that mothers have received full prenatal care benefits. Using data from the first follow-up would mean that most newborns did not receive prenatal benefits until well into the pregnancy. While this allows me to capture the prenatal care effect of PROGRESA, it limits the number of observations that I can use to estimate the effect of duration on the program. Therefore, given the sample size, I did not estimate model 2. Finally, newborns that were less than one month old at the time of the survey were excluded from the analysis.

I estimate a model similar to model 1 for stunting and anemia. Whether a child is stunted and/or anemic is only observed once in the post-intervention period. Therefore, I am unable to include individual random effects or estimate the effect of the duration of exposure to the program.

Unlike self-reported morbidity and anemia, height is a continuous variable, and I can use linear regression models with a village random effect to estimate program impact. In addition to the socioeconomic characteristics used in the other models, I also include a series of dummy variables indicating the child's age in the follow-up survey in three-month intervals, separately for male and females. This is important to control for highly nonlinear relationships involving height, age, and sex in the first three years of life.

TABLE 1—PRE-INTERVENTION DESCRIPTIVE STATISTICS FOR THE MORBIDITY SAMPLE OF CHILDREN AGE 0–35 MONTHS AT BASELINE

Variable	Treatment	Control	p value for difference
Child was ill in last 4 weeks (=1)	0.330	0.323	0.771
Age	1.625	1.612	0.914
Male (=1)	0.511	0.491	0.091
Father's years of education	3.803	3.840	0.980
Mother's years of education	3.495	3.829	0.062
Father speaks Spanish (=1)	0.942	0.929	0.276
Mother speaks Spanish (=1)	0.935	0.917	0.443
Own house (=1)	0.923	0.917	0.465
House has electricity (=1)	0.644	0.711	0.091
Hectares of land owned	0.809	0.791	0.553
Male daily wage rate (pesos)	30.483	31.219	0.370
Female daily wage rate (pesos)	27.258	27.844	0.493
Sample size:	4,519	3,306	

Notes: This table reports descriptive statistics for the sample of children age 0–35 months at baseline before the intervention. The p values in the third column are for the test of the hypothesis that the means of the treatment and control groups are equal and are adjusted for inter-cluster correlation at the village level.

TABLE 2—ESTIMATED LOG ODDS ESTIMATES OF THE IMPACT OF PROGRESA ON CHILDREN'S PROBABILITY OF ILLNESS

		Child age 0–35 months at baseline	
Variable	Newborns	Model 1	Model 2
PROGRESA eligible = 1	0.747 (0.013)	0.777 (0.000)	
PROGRESA eligible for 2 months = 1			0.940 (0.240)
PROGRESA eligible for 8 months = 1			0.749 (0.000)
PROGRESA eligible for 14 months = 1			0.836 (0.005)
PROGRESA eligible for 20 months = 1			0.605 (0.000)

Notes: The first two columns report the estimated log odds from coefficients on dummy variables indicating whether the child was in a treatment village and eligible for PROGRESA. The p value for the hypothesis test that the estimated log odds is equal to 1 is reported in parentheses. The third column reports the results for the length of time that the child could have been on PROGRESA. The coefficients for all three models are estimated from a random-effects logit model, which allows for inter-cluster correlation at the village level and controls for the socioeconomic variables reported in Table 1, measured at baseline prior to intervention.

V. Morbidity Results

The response rates to the baseline survey were quite high (93 percent), and sample attrition was low compared to other large longitudinal surveys. Specifically, over the two-year experimental period, 5.5 percent of the households and 5.1 percent of the individuals dropped from the sample. More importantly, there were no differences in attrition between the control and treatment areas, suggesting no systematic attrition bias in the analysis.

The morbidity analysis sample consists of children younger than age 3 at baseline and children born during the experimental period. The analysis sample consists of 7,703 children who were younger than age 3 at baseline and 1,501 newborns (i.e., children born during the intervention period).

Table 1 reports the means of individual, household, and village characteristics for the sample of children who were alive at baseline. The last column reports the p value for the test of the null hypothesis that the means of the control and treatment group are equal. The p values were calculated adjusting for inter-cluster correlation within villages. At baseline, all of the characteristics were statistically indistinguishable between control and treatment samples at the 5-percent significance level. Only three characteristics were statistically different at the 10-percent significance level.

The results of the logistic regressions reported in log odds are presented in Table 2. The first column reports the results for the newborn sample. The estimates suggest that the treatment newborns were 25.3 percent less likely than the controls to be reported as being ill in the previous month, a difference that is statistically significant at the 5-percent significance level. Similarly, the second column reports that treatment 0–3-year-olds were 22.3 percent less likely to be ill than controls, and this difference is significant at the 1-percent level. The third column reports the results for the program-exposure model. While there appears to be no program impact after only six months of program benefits, the illness rate of the treatment group was 39.5 percent lower than the control

TABLE 3—ESTIMATED IMPACT OF PROGRESA ON CHILDREN'S OBJECTIVE HEALTH MEASURES

Statistic	Height	Stunted	Anemia
Estimated program impact	0.959	0.914	0.745
	(0.004)	(0.495)	(0.012)
Treatment group mean	80.725	0.396	0.410
Control group mean	79.742	0.410	0.483
Sample size:	1,552	1,552	2,010

Notes: The first row in this table reports the estimated coefficient on a dummy variable indicating whether the child was in a treatment village for height from a linear regression with village random effects, and the estimated log odds from a coefficient on a dummy variable indicating whether the child was in a treatment village for stunting and anemia from a random-effects logistic regression. The p value for the test that the coefficients are different from zero in the first two columns and different from 1 in the third column are reported in parentheses.

group with 24 months of program exposure, and this difference is significant at the 1-percent level.

VI. Anemia and Height Results

The response rate for anthropometrics was 97 percent, and the response rate for hemoglobin was 92 percent. The sample for height consists of children age 12–36 months at the time of survey and children 12–48 months for anemia. The sample size for the height analysis is 1,049 treatments and 503 controls, whereas the sample size for anemia is 1,404 treatments and 608 controls. I matched the 1999 objective health survey to the 1997 baseline socioeconomic survey. Using these data I tested the hypothesis that the means of the variables in Table 1 are not different for the control and treatment groups for this subsample. Of the 11 baseline socioeconomic-characteristics means, only two are significantly different at the 5-percent level.

The estimated impacts are reported in the first row of Table 3. The second and third rows report the means for the treatment and control groups separately. The first column reports the results for height using the cross-section data set. I find that treatment children are 0.96 centimeters taller than control children, and this difference is statistically significant at the 1-percent level. The second column reports the log-odds difference for the probability of being stunted. The results show that treatment children are 8.6 percent less likely to be stunted, but this difference is not statistically significant at any conventional level. Finally, the last column reports the log-odds difference for the probability of being anemic. The results show that treatment children are 25.5 percent less likely to be anemic, and this difference is statistically significant at the 1-percent level.

VII. Discussion

I found a significant improvement in the health of children in response to PROGRESA. Specifically, children born during the two-year intervention to families benefiting from the program experienced an illness rate in the first six months of life that was 25.3 percent lower than that of control children. Treatment children aged 0–35 months at baseline experienced a reduction of 39.5 percent in their illness rates after 24 months in the program. Moreover, the effect of the program seems to increase the longer the children stayed on the program, suggesting that program benefits were cumulative. I also found that treatment children were 25.3 percent less likely to be anemic and grew about 1 centimeter more during the first year of the program.

While these results suggest that PROGRESA has had a positive effect on child health, they do not indicate which aspects of this complex program really matter. PROGRESA combines large cash transfers with requirements that individuals engage in a number of preventive health and nutrition activities. One cannot tell if the same results could have been achieved with just a large cash transfer and no behavioral requirements. In is also hard to distinguish between the relative effects of compliance with the various requirements. Answers to these questions would facilitate a better package and therefore improve the cost-effectiveness of the intervention.

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