Journal of Development Economics 94 (2011) 74–85

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

A healthier start: The effect of conditional cash transfers on neonatal and infant mortality in rural Mexico

Tania Barham *

Department of Economics and Institute of Behavioral Science, University of Colorado at Boulder, United States

ARTICLE INFO

Article history:
Received 31 May 2007
Received in revised form 18 December 2009
Accepted 8 January 2010

JEL classification:
O15
I18
I38
J18

Keywords:
Infant mortality
Neonatal mortality
Conditional cash transfers
Mexico
Impact evaluation
Demand incentive

ABSTRACT

Conditional cash transfer programs seek to break the intergenerational transmission of poverty by building the human capital of poor children. Despite their popularity throughout the developing world, relatively little is known about their effect on children's health outcomes. This paper evaluates the impact of the Mexican conditional cash transfer program, Progresa, on two important health outcomes: infant and neonatal mortality. It exploits the phasing-in of Progresa over time throughout rural Mexico to identify the impact of the program. The paper shows that Progresa led to a large 17% decline in rural infant mortality among the treated, but did not reduce neonatal mortality on average. The benefit–cost ratio is between 1.3 and 3.6. Tests for heterogeneity show larger declines for some groups including those municipalities whose pre-program levels of mortality were above the median, and those that prior to the program had higher illiteracy rates, and less access to electricity.

Published by Elsevier B.V.

1. Introduction

With infant mortality rates four to twenty times those of developed countries, poverty reduction is critical if less developed countries are to progress. Conditional cash transfer (CCT) programs are the latest in a series of social programs designed to alleviate short- and long-term poverty (Fiszbein and Schady, 2009). The long-term goals will be achieved by building the human capital of young children and thus breaking the intergenerational transmission of poverty. To reach this goal, cash transfers are provided conditional upon beneficiaries engaging in behaviors designed to improve their health, nutrition and/or education status. The first large scale CCT program started in Mexico in 1997. Since then, similar programs have been implemented in more than 12 developing countries and recently in New York City. Despite their current popularity and emphasis on human capital accumulation, there is limited analysis of their impact on children's health outcomes. In this paper, I use the phasing-in over time of the CCT program in rural Mexico, Progresa,[1] to evaluate the impact of this innovative policy tool on two important indicators of children's health, infant and neonatal mortality rates. Neonatal and infant mortality rates (NMR and IMR) are defined as the number of children who die before age 30 days and age 1, respectively, per 1000 live births in the same year.

Past research used randomized experiments and household survey data collected for the purpose of evaluating a particular CCT program. These studies show that CCT programs were effective in improving the nutritional status of young children in a number of different countries in Latin American (Gertler, 2004; Maluccio and Flores, 2004; Rivera et al., 2004; Attanasio et al., 2005; Behrman and Hoddinott, 2005), and in reducing acute diarrhea episodes in Colombia (Attanasio et al., 2005).[2,3] However, there are no studies on one of the main indicators of children's health, infant mortality. While

* Institute of Behavioral Science, University of Colorado at Boulder, 483 UCB, Boulder, CO 80309-0483, United States. Tel.: +1 303 492 0237; fax: +1 303 492 6924.
E-mail address: tania.barham@colorado.edu.

0304-3878/$ – see front matter. Published by Elsevier B.V.
doi:10.1016/j.jdeveco.2010.01.003

[1] Progresa stands for Programa de Educatión, Salud y Alimentación. This program is now known as Oportunidades.

[2] Other papers on the health impact of CCTs have focused on testing whether the health conditionalities led to appropriate increases in health care utilization and vaccination coverage (Gertler and Boyce, unpublished manuscript; Bautista-Arredondo et al., 2006; Barham et al., 2007; Barham and Maluccio, 2009).

[3] Improvements in the nutritional status of children have also been found in research on the effects of unconditional cash transfer program in South Africa (Duflo, 2003; Case, 2004).

mortality data are routinely collected in household surveys, mortality is a relatively rare event, so even large household surveys from randomized experiments, often do not have a sufficient sample size for accurate estimation of infant or neonatal mortality.

Empirically establishing causality between CCT programs and IMR and NMR is difficult without the use of randomized experiments. Owing to the extensiveness of Progresa and that it was phased-in over time and location throughout rural Mexico, a quasi-experimental method is exploited to determine program impacts. In particular, I use the percent of rural households receiving Progresa transfers in a given year and municipality as the treatment variable. Using a municipality-level dataset created from administrative, census, and vital statistics data covering the period 1992–2001, and municipality and time fixed-effects models, the impacts of the program on IMR and NMR are determined. In addition, the causes of death and types of communities for which the program has been most successful at reducing mortality are explored.

The findings suggest that the benefits and rate of return of Progresa on children's health are quite large. They show that Progresa led to a 17% reduction in infant mortality among the treated and an average treatment effect of 8%. This finding is particularly remarkable given there was less than a 1% reduction in infant mortality in program municipalities in the five years prior to the program. The effect on neonatal mortality is not consistently statistically significant. The benefit–cost ratio for Progresa's health component ranges from 1.3 to 3.6. This ratio is likely an under-estimate because it is based on benefits derived solely from reductions in infant mortality and does not include improvement in morbidity for children and adults that most certainly occurred as a result of the program.

2. The rural Progresa program

2.1. Background

Adopted in 1997, Progresa aims to break the intergenerational transmission of poverty by improving the human capital of poor children in rural Mexico. Before 2001, the program mainly targeted the rural poor reaching nearly 2.5 million rural households by 2000. The rigorous evaluation and the success of the rural program provided a stimulus for the program to be implemented in the urban areas of Mexico and by other countries. The Progresa model was first adopted mainly by middle income countries such as Mexico, Brazil, Argentina, Chile, and Turkey, but has since been implemented in low-income countries such as Nicaragua, Mozambique, and Yemen.

Progresa is unique in that it combines two traditional methods of poverty alleviation: cash transfers and free provision of health and education services. These programs are linked by conditioning the cash transfers on children attending school and family members obtaining sufficient preventative health care. Therefore, the income transfer not only relaxes the household budget constraint, but also improves utilization of health and education services. There are separate transfers provided for the health and education conditionalities, so receiving the health transfer does not depend on the family also meeting the education conditionalities. Together, these conditionalities led to an increase in average beneficiary income levels of 22% the in rural areas (Parker and Teruel, 2005).

2.2. The Progresa health and nutrition component

The health component of Progresa was designed to address many intractable health issues in rural Mexico. For instance, the program targets infants, children, and pregnant and lactating women in an effort to ensure a healthier start to life. In addition, the cash transfers are conditional on every household member's participation in three important health activities: growth monitoring from conception to age 5; regular preventative health check-ups for all family members, including prenatal care and immunizations, and; mother's attendance at health, hygiene and nutrition education talks.[4]

An appointment monitoring system which included providing beneficiaries with an appointment book helped facilitate compliance with conditionalities. Transfers based on the health conditionality were paid every two months and were only paid if beneficiary households attended all required visits and education talks for that two month period.

Since it was expected that health care utilization would rise as a result of the program, Progresa coordinated with other government ministries to ensure an adequate supply and that the quality of health care in program areas did not deteriorate. The improvement in supply included the use of mobile clinics and foot doctors to reach marginalized communities that did not have access to permanent health clinics. In addition, a basic health service package was available at health facilities to ensure some minimal level of quality of care. This package included: family planning; education on basic sanitation, and accident prevention; prenatal, childbirth and puerperal care; growth monitoring; vaccinations; anti-parasite treatment, and; prevention and treatment of diarrhea, respiratory infections, tuberculosis, high blood pressure, and diabetes (Adato et al., 2000).

In order to improve beneficiary nutritional status, Progresa provided a nutritional supplement to pregnant and lactating women, children under the age of 2, and children aged 2–5 with signs of malnutrition. The macro- and micronutrient content of the supplements were adapted to meet the specific nutritional needs of the Mexican population (Rosado et al., 2000). In particular, the supplement provided children with 20% of the caloric needs and 100% of the necessary micronutrients (Behrman and Hoddinott, 2005).[5]

2.3. Program mechanisms to reduce infant and neonatal mortality

Infant mortality is often broken down into neonatal mortality (deaths that occur within the first month of life), and post-neonatal mortality (deaths that occur after the first month of life but before age one) since some of the main causes of death differ. According to the 1996 Mexican death certificate data, deaths in the post-neonatal period were mainly attributed to respiratory and intestinal infections, congenital anomalies and malnutrition. Neonatal deaths resulted primarily from perinatal infections, respiratory conditions such as birth asphyxia, and congenital malformations. While often not the main cause of death, chronic maternal malnutrition and low birth weight of babies are known to be a principal contributor to neonatal death (Moss et al., 2002).

The Progresa program is designed to address many of the main causes of neonatal and post-neonatal mortality.[6] Due to both the scope of the conditionalities and concern about the quality of prenatal care, the program may have less of an effect on neonatal mortality. This is because the majority of neonatal deaths take place in the first seven days of life (Moss et al., 2002), and common strategies to reduce preventable causes of neonatal mortality (i.e. respiration conditions at birth), such as the presence of a trained birth attendant at delivery and a newborn visit within a few days of life (Moss et al., 2002), were not included in the conditionalities. However, prenatal care can lead to reductions in neonatal deaths due to, but not limited, to perinatal infections and some congenital anomalies (Gilbert, 2002; Penchaszadeh, 2002; Hollier and Workowski, 2005), though poor quality of

[4] Children from birth to age 12 months were required to have 7 check-ups. Pregnant women were required to have a minimum of 5 check-ups during the prenatal period and 2 check-ups after the birth. Depending on their age, older family member were required to have between 1 and 4 check-ups a year (Adato et al., 2000).

[5] See Rosado et al. (2000) and Rivera et al. (2000) for details of the nutrient content of the supplement.

[6] Reduction of some causes, such as congenital anomalies, may require preconception care rather than prenatal care (Lu et al., 2003), genetic tests, ultra sound or surgical procedures which are not commonly available in rural Mexico.

prenatal care may reduce the effect of the program (Bronfman-Pertovsky et al., 2003; Aguilar-Barradas et al., 2005; Barber, 2006).

Despite these concerns, the three main health conditionalities (preventative care visits, nutritional supplements, and health education) are designed to improve the health outcomes of young children. The conditionalities require children aged one month to a year to receive five preventative health care check-ups. Many of the services provided in the Basic Health Services Package are designed to reduce the major causes of post-neonatal mortality such as child respiratory and intestinal infections, and malnutrition. Research, though limited, suggests that the quality of treatment for diarrheal diseases and respiratory infections was adequate in the public sector in Mexico prior to Progresa (Bojalil et al., 1998), so the interventions should be effective.

Another Progresa intervention that could reduce mortality in both the neonatal and post-neonatal periods is the nutritional supplement and treatment for parasites. These are important interventions since not only is malnutrition a major cause of post-neonatal mortality, but it is an important underlying cause of respiratory and intestinal infections, and congenital anomalies (Victora et al., 1999; Penchaszadeh, 2002; Zar and Mulholland, 2003; O'Ryan et al., 2005). Also, treatment of parasites and micronutrient and protein/energy supplementation during pregnancy are interventions that have been shown to be effective at reducing low birth rate births and in some cases neonatal mortality (de Onis et al., 1998; de Silva et al., 1999; Ladipo, 2000; Kramer and Kakuma, 2003; Mishra et al., 2005; Shaheen et al., 2006).

The health education talks are also important because they teach mothers to prevent and recognize signs of acute respiratory and diarrheal diseases; how to make oral rehydration salts — the main therapy for managing intestinal infections (Phavichitr and Catto-Smith, 2003; Thapar and Sanderson, 2004); the benefits of exclusive breastfeeding and child nutrition; and provide important information on pregnancy, delivery, and newborn care. This education may also encourage families to spend part of the cash transfer on more nutritious food and improving their children's health.

2.4. The randomized experiment

A prominent feature of Progresa is the randomization of 506 program localities in seven states into treatment and control groups. While many studies on Progresa take advantage of the survey data collected to evaluate the program (henceforth called the Progresa randomized evaluation database), this database has an insufficient sample size to estimate a reasonable point estimate of the impact of Progresa on infant or child mortality. This is because there were only a handful of deaths of children under age one in the control areas in the post-intervention period.

2.5. Program targeting and phase-in

Progresa used a two-stage process to identify eligible beneficiary households in rural areas. In the first stage, rural localities[7] were selected. In order to meet the program's objectives, localities were chosen based on a number of attributes. Localities were first ranked by a marginality index[8] and only those with a high marginality[9] were

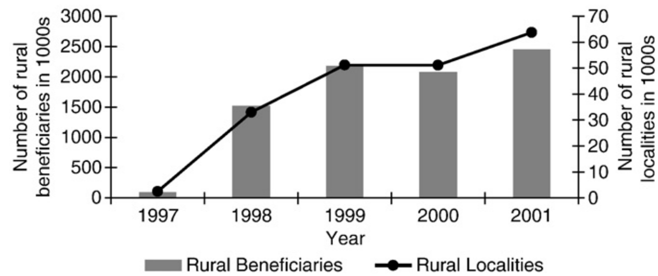

Fig. 1. Trends in the number of Progresa beneficiary families and localities.

included in the program. Next, localities were screened to ensure access to primary and secondary schools as well as to a permanent health care clinic.[10] Finally, the program used population density data and information on the proximity of localities to each other to determine the geographic isolation of the locality. This information was used to identify groups of localities where the maximum benefit per household in extreme poverty would be reached. As a result, any locality with less than 50 inhabitants, or that was determined to be geographically isolated was excluded from the program.

Once localities were selected, beneficiary households in each community were identified. A census, called the Encaseh, was taken of all households in the program localities. This census collected information on household income and characteristics that captured the multidimensional nature of poverty. Using these data, a welfare index was established and households were classified as poor or non-poor. Only the poor became eligible for benefits. Once the list of potential beneficiaries was drawn, it was presented to a community assembly for approval. As a result of this process, a different percent of the rural population is covered by the program in each locality.[11]

For logistical and financial reasons, the program was rolled out over time across localities. Fig. 1 provides the number of program beneficiaries and program localities over time. The program started in 2578 localities in 7 out of 32 states in 1997, with the first transfers being provided between September and October of 1997. In 1998, the program was greatly expanded, reaching almost 34,000 localities and all but two states. In this year, the requirement that localities must have access to a permanent health clinic was relaxed. Not all localities were incorporated in the same month in 1998, rather the expansion was spread throughout the year. For this reason, some beneficiaries received transfers starting in February 1998, while others had to wait until September or October of 1998. Between 1999 and 2001, localities that were eligible, but not yet included and some localities which were previously excluded due to geographical isolation were also incorporated into the program.

3. The data

Rural and urban IMR and NMR were constructed at the municipality[12] level using the 1992–2001 vital statistics data on births and deaths.[13] The mortality data are from a nation-wide database containing information on every certified death in Mexico and were provided by the Mexican Ministry of Public Health. These data are available at the

[7] A rural locality has 2500 or less inhabitants. Of the 199,391 localities in the 2000 census 196,350 were rural. The average number of people living in a rural locality is 126.

[8] The marginality index is constructed using the principal components method. The variables in the index include: the literacy rate; percent of dwellings with running water, drainage, and electricity; average occupants per room; percent of dwellings with a dirt floor; and percent of labor force working in the agriculture sector.

[9] The marginality index was divided into five grades based on the degree of marginality (for details see de la Vega, 1994). A grade of five indicates a high level of poverty and a grade of one a low level of poverty. Only those localities with marginality grades of four or five were considered eligible for Progresa.

[10] A locality was considered to have access to a health care clinic if the clinic was either in the locality or in a neighboring locality at most 15 km away (Skoufias et al., 1999).

[11] See Skoufias et al. (1999) and Coady (2000) for more details on program targeting and the welfare index.

[12] Localities are grouped into municipalities. The 2000 census recorded that there were 199,391 localities in 2445 municipalities in Mexico. A municipality is similar to a county in the US and in 2000 the average population of a municipality was 40,000. The average population of rural areas of a municipality was 10,306.

[13] Values of IMR greater than 240 were set to missing (0.3% of the data), but do not affect the results.

municipality level and distinguish whether the person who died resided in a rural or urban locality within that municipality. It is well known that infant mortality from the vital statistics data is under-reported in most countries and Mexico is no exception (Mathers et al., 2005; Braine, 2006). Since 1950, Mexico's vital statistics certification system has meet the World Health Organization (WHO) international standards, and ranks in the top 20 countries in the World according to the WHO for high quality mortality records (Mathers et al., 2005). Despite this high rating, under-reporting is an issue and is higher in the rural areas and in the neonatal period (Tome et al., 1997; Lozano-Ascencio, 2008). The live birth data are publicly available for every municipality in Mexico on the Mexican Statistical Agency, INEGI, website and contain information on every registered live birth. These data are annual and indicate whether the woman who gave birth resides in an urban or rural locality.[14] There is also under-reporting in these data. The infant mortality rate is not adjusted for under-reporting of deaths or births because estimated yearly rates may smooth over changes due to Progresa. Comparisons with rates that are adjusted using the census data (Sepulveda et al., 2006), show that the IMR rates are lower in this study indicating that under-reporting of deaths is a more common problem in Mexico. Discussion of how changes in under-reporting may bias the results is provided in Section 5.3.

The intensity of treatment indicator, referred to as *program intensity* in the tables, is the percent of rural households in a municipality receiving Progresa benefits. It was created using Progresa administrative and INEGI census data. The Progresa administrative data include the number of households registered for the program in December of each year. This information is available for each locality from the inception of the program in 1997 to 2001. Using INEGI census data on the number of rural and urban households in a municipality for 1990, 1995 and 2000, the number of households for each year between 1992 and 2001 is linearly interpolated. Thus, the percent of rural households receiving program benefits in a municipality is the ratio of the number of beneficiary households to the total number of households in the rural areas of a municipality.[15]

Data on municipality characteristics used as controls in the analysis, with the exception of two, are available in the INEGI 1990[16] and 2000 Censuses and the 1995 Conteo;[17] The percent of households with a dirt floor and percent of the population working in the primary sector are only available in the 1990 and 2000 Censuses. Again, linear interpolation is used to obtain the values for the years the data are not available. These control variables are available at the locality level and are aggregated to the municipality level for localities that received the Progresa benefits before 2001 in order to control for changes in characteristics over time in Progresa localities.

Health supply data are from the administrative records of the Ministry of Health and Instituto Mexicano del Seguro Social (IMSS)-Oportunidades and are not publically available. These data do not include information on the total supply of health care in Mexico, but rather cover the providers which supply health care to Progresa beneficiaries.

A municipality-level panel dataset was constructed covering the period 1992–2001. Municipality boundaries were redefined during this time period. In order to make a consistent panel of municipalities from 1992 to 2001, municipalities that were split in a particular year are amalgamated. This results in a balanced panel of 2391 municipalities each year.

[14] Data on births are missing for Ozxaca in 2000. These values were estimated by taking the average of the ratio of births (urban or rural) for 1999 and 2001 in Oaxaca and multiplying by the total number of births in 2000.

[15] Approximately 2% of all values of program intensity are greater than one. These values are set to missing and do not affect the results.

[16] The 1990 locality data were matched by name to the 1995 and 2000 data by the author. Locality names and codes changed over time and these changes were incorporated when identified.

[17] The Conteo is a shorter version of the Census.

4. Identification strategy

The objective is to estimate the impact of Progresa on rural infant and neonatal mortality. Ideally, I would compare the IMR and NMR in treated rural localities with the counterfactual — the IMR and NMR had Progresa not been available in these localities. Since the mortality data are not available at the locality level, I investigated the impact of the program at the municipality level, and use the percent of rural households within a municipality covered by the program in a given year as the treatment variable. This treatment variable combines three sources of variation. One source is the variation in treatment status across municipalities over time, which is referred to as municipality phase group. The second source of variation is the number of localities covered by the program over time within a municipality. Third, the percent of households benefiting from the program differs between localities due to poverty targeting within localities.

4.1. Sources of variation

Municipalities were incorporated into the program over time between 1997 and 2001. A municipality is defined as being part of Progresa the year the first program locality, in that municipality, is incorporated. Between 1997 and 2001 the program reached 2220 municipalities. During the first year, 1997, the program included 117 new municipalities. However, the majority of municipalities were incorporated in 1998 and 1999, 1326 and 648 respectively. Only 12 new municipalities became part of the program in 2000 and 117 in 2001. For simplicity, the municipality phase groups are assigned numbers 1 to 5 corresponding to the year, 1997–2001, they entered the program. This phasing-in of municipalities leads to variation in the treatment status across municipalities over time, and municipalities yet to be treated can be used as comparison municipalities. The identifying assumption in this case is that the changes in infant mortality observed in municipalities incorporated in different years would be the same, had they not received the program. Although it is not possible to test this assumption, I test that the pre-intervention trends in the IMR and the NMR are the same between municipalities that joined the program in different years. If the trends are the same in the pre-intervention period, they are likely to have been the same in the post-intervention period in the absence of the program.

I test that the pre-intervention trends for rural IMR are similar by comparing the difference in mean IMR between municipality phase groups for the years 1990–1996. Dummy variables are specified for the pre-intervention years 1990–1996, $Year_j$, and municipality phase groups, $PhaseGroup_k$, where 1990 is the excluded year and phase group 2 the excluded municipality phase group. In order to mimic the regression analysis, municipality fixed effects, τ, are included. The equation used to test the difference in means for municipality, m, in time, t, is:

$$IMR_{mt} = \beta_0 + \sum_{j=1991}^{1996} \beta_j Year_{jt} + \sum_{j=1991}^{1996} \sum_{k=1,3}^{5} \theta_{jk} Year_{jt} * PhaseGroup_{km} + \tau_m + u_{mt} \quad (1)$$

The $\theta's$ give the difference in the mean change in rural IMR since 1990 between municipality phase group 2 and each of the other phase groups for the years 1991–1996, and are reported in Table 1. If the differences ($\theta's$) are not significantly different from zero, then the pre-intervention trends for rural IMR are statistically similar between the municipality phase groups.

The differences in means between phase group 2 and the other phase groups are statistically insignificant on the whole for rural IMR and NMR. However, the magnitude of the differences is much larger when comparing municipality phase group 2 to 4 or 5, and each group has one year where the differences are statistically significant at the

Table 1
Difference in mean municipality rural IMR and NMR between phase group 2 and other phase groups, by year.

Year	Rural IMR				Rural NMR			
	1	3	4	5	1	3	4	5
1991	0.75	−0.26	17.10	2.90	0.62	−0.89	2.93	0.77
	(0.93)	(0.99)	(25.69)	(2.30)	(0.55)	(0.56)	(19.66)	(1.53)
1992	−1.93+	0.89	18.77*	4.04	−0.25	−0.08	2.41	0.25
	(1.07)	(1.16)	(10.39)	(2.46)	(0.58)	(0.60)	(10.81)	(1.54)
1993	−1.47	0.93	−1.69	1.40	−0.23	0.66	−13.29	0.19
	(1.10)	(1.14)	(10.03)	(2.66)	(0.57)	(0.60)	(8.92)	(1.61)
1994	−0.96	1.36	−4.71	3.12	−0.07	1.11+	−6.77	−0.29
	(1.09)	(1.18)	(11.63)	(2.59)	(0.70)	(0.59)	(10.40)	(1.77)
1995	−0.69	−0.46	3.48	5.45*	0.44	−0.20	−1.22	2.65
	(1.09)	(1.27)	(8.99)	(2.56)	(0.55)	(0.63)	(8.04)	(2.23)
1996	−1.55	0.48	21.86	−0.76	−0.35	0.45	1.48	−1.62
	(1.21)	(1.31)	(34.20)	(2.38)	(0.71)	(0.68)	(18.52)	(1.68)

Notes: Standard errors are in parentheses and are clustered at the municipality level. * and + denote significance at the 5% and 10% levels respectively. Phase groups 1 to 5 refer to municipalities that were incorporated into the program in 1997 to 2001 respectively. These results are the differences in means controlling for municipality fixed effects.

5% level. Therefore, municipality phase groups 4 and 5 will not be included in the analysis.

Fig. 2 shows the trend in average municipality rural IMR for municipality phase groups 1 to 3 (those incorporated in 1997–1999).[18] If Progresa is successful, there should be breaks in the trend in rural IMR corresponding to municipality phase groups. The break in the trend occurred the year the program entered the municipalities for each of the municipality phase groups except phase group 1. The break for the municipalities that enter the program in 1997 (phase group 1) occurred in 1998 due to beneficiaries being incorporated into the program late in the year. This late start makes it difficult to separately identify the effect of phase groups 1 and 2. The mean rural IMR is also much lower for municipality phase group 1 than 2 or 3.[19] Given that the IMR is much harder to reduce the lower it starts, there is concern that the trends in rural IMR between municipality phase group1 and the later groups would not be the same in the absence of the program. Municipality phase group 1 is therefore also not included in the analysis.

Another source of variation used to identify the program impact is the phasing-in of Progresa localities, within a municipality, over time. This source of variation arises because localities were incorporated into the program in different years within a municipality. For example, in municipality phase group 2 there were 28,561 Progresa localities in 1998, 12,261 were added in 1999, 231 in 2000, and 9687 in 2001.

Results may be biased if the trends in infant mortality in localities that were phased-in during different years within a municipality are not similar. Since mortality data are not available at the locality level, I examine some key determinants of infant mortality using locality level census data. These include literacy, access to services such as electricity, sanitation, and piped water, household size, and the ethnicity of the population. While female literacy would be preferable to overall literacy, changes in female literacy will be reflected in the overall literacy rate. Table 2 presents information on pre-intervention trends in locality characteristics for localities in municipality phase groups 2 and 3. The differences in means are adjusted for municipality fixed effects to mimic the regression analysis. The changes in means between 1990 and 1995 for localities that were incorporated into the program in 1998 are reported in the first row. The subsequent rows show how these changes differ between localities that were incorporated into the program in 1998 versus later years, and indicate if the changes are statistically different between the two groups. The changes in means are statistically different for almost all the variables, but are arguably small in magnitude.

Given the sample size is fairly large (over 50,000) it is not surprising there are so many statistically significant differences. Since statistical tests, such as *t*-tests, are affected by sample size, two other measures of the size of the difference are examined. First, normalized differences (difference in the changes between groups divided by the standard deviation of the difference) are examined because they are not affected by sample size. Normalized differences greater than 0.25 are viewed to be substantial (Imbens and Wooldridge, 2008). All the normalized differences are less than a quarter and all but 3 less than 0.1. Second, the differences in the change in means are compared to the differences in the quartile cut-off values (i.e. forth quartile cut-off minus third quartile cut-off, third quartile cut-off minus second quartile cut-off etc.) for each variable for 1990. The differences in the change in means are smaller than the non-zero differences between the quartiles of the distribution, indicating that they are not large enough to move a locality to a different part of the distribution. As a robustness check, these variables will be included as controls in the regression analysis.

The last sources of variation arise because the percent of households covered by the program in a locality vary between localities due to poverty targeting. The potential endogeneity of this variation with poverty is discussed in Section 5.2. To control for selection bias, it would be preferable to calculate the percent of rural households eligible for Progresa rather than actually receiving program benefits. However, given that take-up rates in the Progresa randomized evaluation sample are high, 96% (Parker and Teruel, 2005), selection into the program is not likely to pose a serious problem.

Using all these sources of variation the treatment variable (percent of rural households receiving Progresa in a municipality) in municipalities

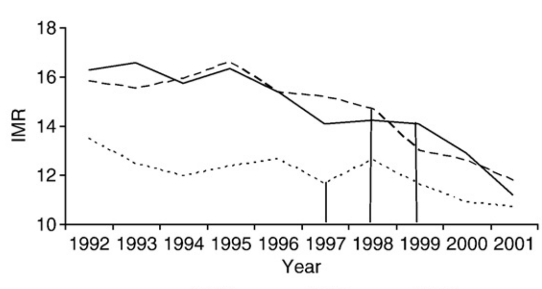

Fig. 2. Trends in rural municipality IMR by date Progresa became available in a municipality. Note: Only municipalities with an average of at least 30% of household receiving Progresa are included.

[18] The figure presents data only for municipalities that had an average participant rate of 30% or greater. Similar breaks are seen if all municipalities are included, the breaks are just not as pronounced.

[19] This is because the program was first rolled out in areas that had permanent health care clinics (Section 2.5).

Table 2
Difference in changes in pre-intervention locality characteristics, by year locality was incorporated into Progresa.

	Percent of population who are		Average number of occupants in a household	Percent of households with		
	indigenous speakers	illiterate		piped water	sewage	electricity
Change in mean for 1998 localities	0.1	−3.6	−0.1	13.5	5.1	21.9
	(0.0)	(0.1)	(0.0)	(0.2)	(0.2)	(0.2)
Difference in change in means between localities incorporated in later years and those incorporated in 1998						
1999 localities–1998 localities	−0.1	0.4*	−0.1*	1.1+	5.0*	−6.2*
	(0.1)	(0.1)	(0.0)	(0.5)	(0.4)	(0.5)
2000 localities–1998 localities	0.0	0.3	−0.0	1.2	4.9*	−2.5
	(0.5)	(0.5)	(0.1)	(2.4)	(1.8)	(2.2)
2001 localities–1998 localities	0.1	0.2	−0.1*	−2.6*	2.1*	−7.2*
	(0.1)	(0.2)	(0.0)	(0.6)	(0.4)	(0.6)
Observations	48,296	48,295	48,311	48,311	48,311	48,311

Notes: Standard errors are in parentheses. * and + denote significance at the 5% and 10% levels respectively. The change in means is the difference in means between 1995 and 1990. Means are adjusted for municipality fixed effects. Percent refers to the proportion of the population over age four for indigenous language speakers and over age 14 for the illiterate population.

phased-in during 1998 (phase group 2) increases from 37% in 1998 to 51% in 2001. For municipalities phased-in in 1999 the increase over time in program intensity is smaller, from 34% in 1999 to 38% in 2001.

4.2. Empirical model

The estimation equation is:

$$IMR_{mt} = \alpha_t + \tau_m + \beta_0 program\,intensity_{m,t-1} + u_{mt}, \quad (2)$$

where *IMR* is the rural infant mortality rate in municipality *m* in time *t* and *program intensity* is the measure of the intensity of treatment (percent of rural households receiving Progresa transfers in a municipality). The lag of *program intensity* is included since some new beneficiaries started receiving benefits in the second half of the year, and receiving benefits while pregnant can be important for health outcomes.[20] Year fixed effects, α_t, are included to control for general time trends common to all municipalities, and municipality fixed effects, τ_m, time-invariant municipal unobservables. In some specifications time-varying rural municipality characteristics, X_{mt}, are included to control for the program phase-in rule and as a robustness check for potential biases caused by differences in observable time-varying variables across municipalities phase groups. Due to the aggregated nature of the data, all regressions are weighted by the number of rural households in a municipality. Lastly, standard errors are adjusted for clustering at the municipality level.

The estimate of the treatment effect of Progresa on the treated is measured by β_0, while the average treatment effect is calculated by multiplying the impact on the treated by the average of the *program intensity* for the program municipalities. The estimate of the treatment effect will be unbiased if there are no unobserved time-varying rural municipality characteristics or trends that are correlated with the treatment variable.

5. Results

5.1. Program impacts

Table 3 presents the effects of Progresa on the IMR (Panel A) and NMR (Panel B). Panel A column (1) of Table 3 shows that the coefficient of the lag of *program intensity* is a statistically significant (at the 1% level) −3.01 indicating that Progresa led to a decrease of 3 deaths per 1000 live births among the treated. With an average rural IMR of 17.5 deaths per 1000 live births, this represents a 17% decline. At the municipality level, the percent of rural households covered by the program reached an average of 47% in 2001. Therefore, the average treatment effect is an 8% reduction in the rural IMR. Panel B column (1) reveals that there is a smaller and insignificant effect of Progresa on rural NMR of −0.85. The lack of a significant program effect on NMR could be a result of more severe under-reporting of mortality for this age group.

5.2. Robustness checks

As discussed in Section 4.1, the changes in means of characteristics for localities incorporated into the program in different years were arguably small but significantly different. To test if these differences could be biasing the results, observable time-varying municipality characteristics are presented in column (2). The controls include the variables presented in Table 2, controls for the phase-in rule (population density and the percent of rural Progresa localities in a municipality with access to a permanent health clinic in a given year), the percent of households with a dirt floor, and the percent of the population working in the primary sector. These control variables are available at the locality level and are aggregated to the municipality level for localities that received Progresa benefits before 2001, to better approximate changes in Progresa areas. Approximately 400 observations are lost when the municipality characteristics are included. This change in sample decreases the treatment effect without the addition of the controls, by .2 for IMR and NMR leading to estimates of −3.21 to −1.05 respectively (results not reported). The inclusion of the time-varying municipality characteristics further increases the magnitude of the effect to −3.88 for IMR and −1.3 for NMR. NMR is also now statistically significant at the 5% level. The point estimates including the controls are well within the 95% confidence interval of the estimates without controls providing further confidence that the differences in controls prior to the program are small, and if anything results in an under-estimate of the program effect.

Key determinants of IMR found to be significant in a meta-analysis of the determinants of infant mortality (Charmarbagwala et al., 2004), but not included as controls in column (2) are immunization rates, average duration of breastfeeding, and birth order. It is unlikely that unobserved vaccination rates bias the results as rates were very high prior to the program in Mexico. Vaccination rates were greater than 92% in Mexico for the third dose of DPT (diphtheria, pertussis, and tetanus) in 1995 (USAID, 2008), and baseline data from the Progresa randomized evaluation database show that they are above 92% for

[20] Specification tests on the number of lags and linearity of the intensity variable support including only the first lag.

Table 3
Impact of Progresa on IMR, NMR, and the birth rate.

	Rural IMR							Rural IMR lagged 5 years	Urban IMR	Birth rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: IMR										
Lag of program intensity	−3.01*	−3.88*		−3.33*		−3.35*	−3.13*	0.47	0.43	−0.003
	(0.88)	(0.90)		(0.96)		(0.96)	(0.89)	(0.94)	(0.84)	(0.006)
Lag of locality intensity			−3.25*							
			(0.78)							
Lag of municipality phase					−1.12*					
					(0.56)					
Observations	17,722	17,304	17,304	13,972	13,972	17,304	17,722	12,678	12,001	17,707
Adjusted R^2	0.63	0.63	0.63	0.66	0.66	0.67	0.63	0.70	0.72	0.57
Mean of dependant variable	17.5	17.5	17.5	18.2	18.2	17.5	17.5	20.1	16.5	0.14
Panel B: NMR										
Lag of program intensity	−0.85	−1.30*		−0.91		−1.32*	−0.89+	0.54	0.79	
	(0.53)	(0.55)		(0.63)			(0.53)	(0.53)	(0.50)	
Lag of locality intensity			−1.27*							
			(0.51)							
Lag of municipality phase					−0.64+					
					(0.39)					
Observations	17,713	17,304	17,295	13,966	13,966	17,295	17,713	12,672	11,998	
Adjusted R^2	0.50	0.51	0.51	0.54	0.54	0.54	0.50	0.58	0.63	
Mean of dependant variable	8.8	8.9	8.9	9.0	9.0	8.9	8.8	9.0	9.5	
Municipality characteristics	N	Y	Y	Y	Y	Y	N	N	N	N
Drop 2001 and 2002 data	N	N	N	Y	Y	N	N	N	N	N
Municipality time trend	N	N	N	N	N	Y	N	N	N	N
Health supply variables	N	N	N	N	N	N	Y	N	N	N

Notes: All regressions include municipality and time fixed effects and are weighted by number of rural households in a municipality. Standard errors are in parentheses and are clustered at the municipality level. * and + denote significance at the 5% and 10% levels respectively. The municipality characteristics include variables presented in Table 2, controls for the phase-in rule, percent of household with a dirt floor, and percent of population working in the primary sector. Health supply controls are per 1000 population and include: health clinics, hospitals, mobile health clinics, health brigades, doctors, residents, and nurses.

measles for this poor population (Barham et al., 2007). In Mexico in 1999, exclusive breastfeeding of children less than 6 months of age varies most strongly by indigenous and urban–rural status (Gonzalez-Cossio et al., 2003). I control for both indigenous status and urban–rural location, so likely control for differences in breastfeeding habits. Lastly, changes in birth order are likely to be correlated with changes in household size, which is included in the controls.[21]

It is possible that the percent of households covered by the program in a given locality is endogenous as it is correlated with poverty due to poverty targeting of household within localities (Section 2.5). To address this potential bias a new treatment variable is created which is not based on the number of households, the percent of localities which have Progresa beneficiaries in a municipality, *locality intensity*. As shown in column (3) of Table 3, the results are still similar for both IMR (−3.25) and NMR (−1.27).

The type of variation used to identify the impact is further restricted to only the variation at the municipality level to explore the possibility that localities phased-in during different years within a municipality are biasing the results. The new treatment variable, *municipality phase,* takes on the value 1 once any locality in a municipality receives Progresa (i.e. starting in 1998 for phase group 2 and 1999 for phase group 3) and is zero otherwise. It is expected that this will lead to a lower program effect due to the loss in variation in the treatment variable. In addition, data for 2000 and 2001 are dropped so that the analysis focuses only on the effect of localities phased-in during 1998 and 1999. Results for the smaller sample using the original treatment variable, *program intensity*, are reported in column (4) and using the new treatment variable, *municipality phase,* in column (5). While the effect is lower using the treatment variable *municipality phase*, Progresa still has a statistically significant −1.12 effect on IMR and a marginally significant −0.64 effect on NMR.

In addition, municipality specific time trends are included to determine if time-varying unobservables could be biasing the results, column (6). The impact of Progresa is similar for IMR (−3.35) and NMR (−1.32), showing the time specific municipality trends are not likely biasing the results.

A number of falsification exercises are also performed to examine if unoservables are biasing the results. First, to test that pre-existing trends are not related to future program status the dependent variable is lagged five years. The coefficients on the treatment variable in column (8) are small and not significantly different from zero, suggesting that pre-existing trends are not biasing the results. Second, since Progresa operated in the rural localities before 2001, it is possible to test if there are unobservable municipality time trends common to rural and urban areas that are biasing the results by examining the program effect on urban IMR and NMR. Table 3, column (9), confirms that the program had an insignificant effect in urban areas providing evidence that common rural and urban municipality trends are also not biasing the results.

Lastly, a reduction in the infant mortality rate could be a result of an increase in births if program incentives encouraged women to have more births. Past research using the Progresa randomized evaluation database report that the program did not lead to an increase in fertility (Skoufias and McClafferty, 2001; Stecklov et al., 2006). The effect of Progresa on the birth rate, defined as the number of live births per woman of approximately fertility age (ages 15–40) that is presented in column (10) of Table 3 confirms that the program had no effect on the birth rate.

[21] Measures of income or wealth are believed to be important for infant mortality, but are often insignificant in within country analyses (Charmarbagwala et al., 2004).

5.3. Biases from under-reporting of vital statistics data

It is well known that vital statistic data on births and deaths suffer from under-reporting (Mathers et al., 2005). To the extent that under-reporting does not change over time within a municipality, it is controlled for by the municipality fixed effects. However, changes in under-reporting over time that is correlated with Progresa will bias the results.

Mexico started its death certification system in 1950. Under this system, a death is reported to a health official who certifies the death, determines the cause of death (via a verbal description of the death or through an examination) and gives the family a death certificate (Braine, 2006). Under-reporting occurs when there is no health official located close to the family of the person who died, or the time or money costs associated with reporting the death are too high (Braine, 2006). Progresa may lead to an increase in reporting of deaths among the beneficiaries for several reasons. First, all members of the family under the program must regularly see a health provider. Second, if any family member misses a scheduled appointment they will not receive the cash transfer for meeting the health conditionalities. So it is in the financial interest of the family to report to the health official a child's death. As a result, it is possible that in areas where there are more Progresa beneficiaries there will be more infant deaths reported than before the program began.

A reduction in the infant mortality rate could also result from an increase in births rather than a decrease in deaths. Birth registration is not the responsibility of the health officials, and there are no program conditionalities that would lead to an increase in birth registration. Section 5.2 showed that Progresa did not lead to an increase in the birth rate. Therefore, any under-reporting will cause the program effect to under-estimate the reduction in infant mortality.

5.4. Impact of the program on selected causes of deaths

Table 4 presents the program impact for the five main causes of infant mortality broken down by the NMR and post-neonatal mortality rate (PNMR). The PNMR is the number of deaths of children greater than 30 days old and less than age one per 1000 live births. It is important to examine the program impacts by disease as the aggregation of diseases may mask significant results, especially for neonatal mortality, and provides evidence of which major causes of deaths Progresa has been able to address. The results are presented controlling for municipality characteristics, but are similar without the controls unless noted in a footnote. During the post-neonatal period there is a statistically significant reduction per 1000 live births of 0.65 deaths due to intestinal infections, 1.12 deaths due to respiratory diseases, and 0.33 due to nutritional deficiencies. Neonatal deaths due to respiratory infections are also significantly reduced by 0.44. There is also a marginally significant (at the 10% level) 0.30 reduction in the PNMR related to congenital anomalies and 0.85 reduction in the NMR due to conditions in the perinatal period.[22]

As expected, reductions in deaths are concentrated in the causes of death for which the program was most suited to address: intestinal and respiratory diseases, and nutritional deficiencies. There is, however, suggestive evidence that Progresa led to a decrease in deaths in the perinatal period due to congenital anomalies. This illustrates that the program may be able to address causes of death that are more difficult to treat and require prenatal and even preconception care to prevent.

Table 4
Impact of Progresa by disease.

	IMR	NMR	PNMR
	Intestinal infections		
Lag of program intensity	−0.73*	−0.07+	−0.65*
	(0.20)	(0.03)	(0.18)
Adjusted R^2	0.34	0.16	0.32
Mean of dependant variable	1.2	0.1	1.1
	Respiratory infections		
Lag of program intensity	−1.56*	−0.44*	−1.12**
	(0.40)	(0.12)	(0.32)
Adjusted R^2	0.49	0.29	0.44
Mean of dependant variable	3.1	0.5	2.6
	Nutritional deficiencies		
Lag of program intensity	−0.35*	−0.01	−0.33*
	(0.13)	(0.04)	(0.12)
Adjusted R^2	0.26	0.17	0.23
Mean of dependant variable	1.0	0.2	0.9
	Conditions originating in the perinatal period		
Lag of program intensity	−0.91+	−0.85+	−0.06
	(0.45)	(0.43)	(0.08)
Adjusted R^2	0.45	0.44	0.17
Mean of dependant variable	6.7	6.2	0.3
	Congenital anomalies		
Lag of program intensity	−0.29	−0.01	−0.30+
	(0.23)	(0.16)	(0.14)
Adjusted R^2	0.27	0.23	0.19
Mean of dependant variable	2.4	1.4	1.0
Observations	17,304	17,295	17,304

Notes: All regressions include municipality and time fixed effects and municipality characteristics and are weighted by number of rural households in a municipality. Standard errors are in parentheses and are clustered at the municipality level. * and + denote significance at the 5% and 10% levels respectively. PNMR stands for post-neonatal mortality.

5.5. Heterogeneity of the treatment effect by pre-intervention levels of IMR and NMR

It is likely that the impact of the program will vary considerably depending on the pre-program level of the IMR and NMR. This is because it is easier to reduce the mortality rate when it starts at a higher level. To determine if there are differences in program impacts by pre-program levels of IMR and NMR the sample is split into those municipalities whose average IMR or NMR is below or above the 1995 sample median.

The findings in Table 5 for the two groups are indeed divergent and there is a significant decrease in rural IMR and NMR only for those municipalities whose pre-program levels were above the median. Specifically, there is a significant decrease of 6.9 infant deaths (25%) and 2.5 neonatal deaths (18%) per thousand live births as a result of the program on the treated. These represent average program effects which are slightly higher than for the sample as a whole at 10 and 7% for rural IMR and NMR respectively.

The results also highlight that Progresa may not be effective at reducing rural IMR and NMR in those municipalities that had relatively low rates prior to the program. This is because the percent of deaths due to causes that Progresa was more effective at and designed to reduce were lower in these areas prior to the program.

5.6. Heterogeneity of the treatment effect by pre-intervention characteristics

Data from 1995 are used to examine if the program impact varies by pre-intervention municipality characteristics which are common determinants of infant mortality.[23] To create the pre-intervention characteristics, locality data are aggregated to the municipality level

[22] These variables are not statistically significant even at the 10% level when controls are excluded.

[23] Data on the percent of households with a dirt floor and percent of the rural population working in the primary sector are from 1990 since 1995 data are not available.

Table 5
Heterogeneity of the impact by pre-program levels of rural IMR and NMR.

	Rural IMR		Rural NMR	
	<Pre-program median IMR	≥Pre-program Median IMR	<Pre-program median NMR	≥Pre-program Median NMR
Lag of program intensity	−0.96 (0.85)	−6.85* (1.34)	−0.62 (0.53)	−2.50* (0.88)
Observations	8687	8617	8697	8598
Adjusted R^2	0.39	0.46	0.30	0.31
Mean of dependant variable	8.6	26.5	4.3	13.5

Notes: All regressions include municipality and time fixed effects and municipality characteristics and are weighted by number of rural households in a municipality. Standard errors are in parentheses and are clustered at the municipality level. * denotes significance at the 5% level.

for all localities incorporated into Progresa by 2001. Each characteristic, X^{95}_{mt}, is created so that higher values reflect a worse off state. Terciles of each characteristic are generated and the second and third terciles are interacted with the treatment variable.

The results are reported in Table 6 and highlight that the program was more successful at reducing infant mortality in municipalities that were disadvantaged prior to Progresa in that a greater percent of the households did not have electricity, there was a higher incidence of illiteracy among the population, and more people lived in a household. For example, reductions in IMR were large in municipalities where less than 80% of the households (third tercile) in Progresa localities had electricity prior to the intervention. These areas experienced a reduction of approximately 8 deaths per 1000 live births (47%), or an average treatment effect of 19%. A similar percent decrease in IMR was experienced in municipalities where in Progresa areas the average number of occupants in a household was greater than 5.44 (third tercile) or the illiteracy rate was above 27% (third tercile).

Surprisingly, the program was less successful in municipalities in which Progresa areas appeared to be worse off in that a greater percent did not have access to piped water, more households had dirt floors, and a greater percent of the population that worked in the primary sector.[24] The results for NMR are not as significant, but follow the same general pattern as the IMR. One possible explanation for Progresa not being more effective is that these variables are likely more highly correlated with an unsanitary household environment. For example, parasites live and breed in feces and this poses a threat to children's health. More than 3 million children die each year from parasitic diseases (World Bank, 2005). Fecal matter could enter a house via shoes of people, animals coming in the home, spillage of unclean water (even if electricity is used to boil water to drink), and from human excrement from lack of diapers. Animal fecal matter will be more of an issue if a person works in the primary sector since they are more likely to be around animals, and dirt floors will exacerbate the problem since they are difficult to clean. Even if deworming is included in the Progresa health interventions, re-infection may occur quickly due to poor sanitation levels in the household. A study in rural Mexico showed that children from households with dirt floors or poor water quality were more likely to be infected with intestinal parasites (Morales-Espinoza et al., 2003). Cattaneo et al. (2008) also found that a program providing cement floors to households with dirt floors reduces childhood illness which they believe is a result of reduced intestinal parasites. As a result, Progresa may be less effective for households that do not meet a minimum level of sanitation in their home.

Table 6
Heterogeneity of the impact by pre-intervention municipality characteristics.

	Tercile	Rural IMR	Rural NMR
Lag of program intensity	1	−4.62* (1.95)	−2.55* (1.28)
Interaction with tercile indicators of pre-program municipality characteristics			
Percent of households in Progresa localities with			
No piped water	2	2.30* (1.15)	1.54* (0.67)
	3	3.95* (1.28)	2.11* (0.74)
No electricity	2	−0.87 (1.13)	−0.42 (0.70)
	3	−3.59* (1.38)	−1.53+ (0.85)
No drainage	2	−0.77 (0.98)	−0.82 (0.60)
	3	0.35 (1.10)	−0.04 (0.67)
Dirt floor	2	3.50* (1.14)	1.43+ (0.73)
	3	5.00* (1.46)	1.78+ (0.93)
Percent in Progresa localities of			
Rural population >4 that speak an indigenous language	2	1.05 (1.06)	0.75 (0.67)
	3	−0.13 (1.10)	0.72 (0.70)
Rural population >14 that are illiterate	2	−2.76* (1.08)	−0.60 (0.70)
	3	−2.95* (1.36)	−0.86 (0.81)
Rural population working in the primary sector	2	2.69* (0.95)	0.82 (0.60)
	3	4.14* (1.14)	1.27+ (0.69)
Average number of occupants in rural households	2	−3.50* (1.13)	−1.16 (0.73)
	3	−4.88* (1.17)	−1.48+ (0.78)
Observations		17,257	17,248
Adjusted R^2		0.63	0.51
Mean of dependant variable		17.56	8.86

Notes: All regressions include municipality and time fixed effects and municipality characteristics and are weighted by number of rural households in a municipality. Standard errors are in parentheses and are clustered at the municipality level. * and + denote significance at the 5% and 10% levels respectively.

5.7. Changes in health supply

There was an expansion of health care in rural communities that preceded Progresa to ensure that health supply did not deteriorate with the increase in health care utilization resulting from the program (Bautista-Arredondo et al, 2006), and that beneficiaries could meet their health conditionalities. This makes it difficult to determine if the reductions in mortality are due solely to the demand-side incentives. Health supply variables are likely to be endogenous but are included in Table 3 column (7) to examine if changes in health supply are mechanisms through which Progresa reduced mortality. The point estimates on the lag of the *program intensity* when health supply controls are included remain unchanged providing some evidence that the program effects are not likely a result of changes in health care supply.[25]

6. Benefit–cost analysis

The estimates suggest that Progresa led to large health benefit on the basis of reduced infant mortality, but it is also important to

[24] Primary sector work includes forestry, hunting, fishing, agriculture and live-stock raising.

[25] Results are similar when several lags of the health supply variables were included.

evaluate the costs. Using data from Coady (2000), the estimated net present value of the program costs for the health and nutrition component over the period 1997 to 2000 in 2000 US dollars was 1722 million assuming a 0% discount rate to be on the conservative side.[26]

Program health benefits are converted into the number of deaths averted using the lower bound estimate of infant mortality (when no controls are included). To compare these benefits to the costs, the value of a statistical life (VSL) is used to monetarize the health benefits. Since the VSL varies widely depending on the methodology, estimates are taken from a meta-analysis that finds the VSL in the US ranges between 4 and 9 million in 2000 US dollars and that the income elasticity of the VSL across countries (including some developing countries) is 0.5–0.6 (Viscusi and Aldy, 2003). There is uncertainty about the correct income elasticity and Evans et al. (2002) argue it maybe above one, therefore the VSL is calculated using income elasticities of 0.6 and 1.25. Following a common practice in the environmental literature the VSL for the US is extrapolated to Mexico by adjusting for average differences in income.[27] In contrast to the environmental literature, the measure of income used is purchasing power parity adjusted per capita hourly wage taken from recent national labor market surveys rather than the gross domestic product per capita, as the wage data provide a more accurate measure of actual lifetime earnings.[28] Since wages differ by gender and level of education, and educational attainment is quite different between the two countries, the VSL is calculated separately for men and women and for four education groups.[29] The VSL is averaged across these eight groups using the proportion of employed people in each group in Mexico as a weight.[30] Using a VSL in the US of 4 million and income elasticities of 0.6 and 1.25, the VSL in Mexico ranges from 0.53 to 1.5 million dollars. These VSLs are similar to the values calculated for other developing countries (Evans et al., 2002).

Bringing the estimates of program costs and benefits together, a rate of return is calculated in Table 7. The benefit–cost ratio ranges from 1.3 to 3.6 depending on the income elasticity of the VSL. This benefit–cost ratio is likely to be an under-estimate of the health benefits of Progresa since they do not take into account reductions in mortality for other age groups or long-term morbidity.

Table 7
Rates of return on the Progresa health component.

	VSL 0.53 million (income elasticity of VSL = 1.25)	VSL 1.5 million (income elasticity of VLS = 0.6)
Program Benefits		
Infant mortality reductions per 1000 live births due to Progresa	3.01	
Number of live births to Progresa households 1997–2001	1,368,583	
Deaths averted between 1997 and 2001	4119	
Benefits in millions of 2000 dollars 1997–2001[1]	2191	6226
1997–2001 program costs in millions of 2000 dollars[2]	1722	
Rate of return (benefit–cost ratio)	1.3	3.6

Notes: 1. Benefits are calculated as the number of deaths averted times the VSL.
2. The rate of return was calculated using a zero discount rate for the present value of program costs to be conservative.

7. Conclusions

This paper investigates the extent to which conditional cash transfers can reduce infant and neonatal mortality. Taking advantage of a large, national program in rural Mexico, Progresa, I show that Progesa resulted in a 17% reduction in the infant mortality rate among the treated and an 8% reduction on average in program municipalities. Rural IMR fell by less than 1% between 1992 and 1996, thus Progresa brought about an important decline in rural infant mortality in Mexico. Inclusion of time-varying health supply variables indicates that changes in health supply that accompanied the program are not driving these results. These findings are important from a development perspective. With more than 10 million children dying every year often from causes of poverty, reducing infant mortality is a current policy objective and is one of the Millennium Development goals (World Bank, 2003). The findings also have reverberations for the developed world where CCTs may well impact disadvantaged populations.

More than half of this decline resulted from reductions in the major causes of post-neonatal mortality, such as respiratory or intestinal infections, demonstrating that the Mexican CCT program was well designed and effective at addressing important causes of infant mortality. The success of the Progresa health program as a whole is further confirmed by a benefit–cost ratio of the health component which ranges from 1.3 to 3.6. Because these benefits were based solely on infant deaths averted, the benefit–cost ratio is an under-estimate of the total health benefits of Progresa.

The program did not have a consistent statistically significant impact on the neonatal mortality on average, but did reduce deaths due to respiratory infections. The lack of a consistent program effect on NMR may be a result of either under-reporting of neonatal deaths in Mexico, or increased reporting (due to Progresa) of neonatal death, rather than the ineffectiveness of the program itself. Indeed, NMR has proven to be difficult to reduce in other developing countries as well, as is highlighted by the stagnation of neonatal mortality rates in recent decades (Moss et al., 2002).

However, there were large significant declines in both rural IMR and NMR for those Progresa municipalities whose average pre-program infant and neonatal mortality rates were above the sample median and insignificant and small declines for those below the sample median. In particular, in those municipalities with pre-program rates above the median, there was a 24 and 18% decline as a result of the program on the treated for IMR and NMR respectively. Taking into account that not all households in a municipality received the program, the average program impact was 10 and 7% respectively for rural municipality IMR and NMR. These results highlight other interventions than conditional cash transfers may be needed to reduce infant mortality rates below a certain level of infant mortality.

The findings also suggest that Progresa led to a large (up to 47% on the treated) reduction in infant mortality in those municipalities where, prior to the program, Progresa localities were more disadvantaged in terms of poorer access to electricity, higher illiterate rates,

[26] These costs include transfers for health and nutrition, the nutritional supplement, and operational and personnel costs associated with the health and nutrition component. The health and nutrition transfers and the nutrition supplement accounted for 55% of total transfer and supplement costs (the rest of the transfers are for education). Therefore, 55% of program's operational and personnel expenses are attributed to the health and nutrition components. Operational and personnel expenses were included for the following activities: targeting of localities, selection of beneficiaries, incorporation of family, certification of compliance with conditionalities, the cash transfers, and program monitoring.

[27] The US value of a statistical life can be extrapolated for Mexico using the formula $VSL_{Mex} = VLS_{US} * (Income_{Mex}/Income_{US})^{\varepsilon}$, where ε is the income elasticity of the VSL. This method implicitly assumes that preferences are the same in the two countries.

[28] Wage data on all employed people greater than age 14 was used in both countries. The Mexican data are from the 2005 Encuesta Nacional de Ocupación y Empleo (National Occupation and Employment survey) which are available from the INEGI website (http://www.inegi.org.mex/), and the United States data are from the 2005 American Community Survey available from the Integrated Public Use Microdata Series website (http://www.usa.ipums.org/usa/).

[29] The four education groups include people: (i) who have not completed primary school; (ii) who have completed primary school but not secondary school; (iii) who have completed secondary school but have no further education, and (iv) who have greater than secondary education.

[30] Alternatively, the VLS for the eight groups could be averaged using a weight that is the proportion of people in each group in the US instead of Mexico. This method adjusts the VSL for income differences but holds the education distribution constant. Using this method of averaging the cost–benefit ratio is higher and ranges from 1.5 to 3.9 for income elasticities of VSL of 1.25 and 0.6 respectively.

and families having larger households. Interestingly, the program was less effective in areas where household sanitation levels were likely to be worse in that there was less access to piped water, more dirt floors, and a great percent of the population working in the primary sector. It is possible that children in these households have a higher incidence of intestinal worms which can cause death. Indeed, poor quality water or dirt floors, but not the presence of electricity or a drainage system, are linked to intestinal worm infections (Morales-Espinoza et al., 2003). And, Cattaneo et al. (2008) find that cementing of dirt floors in houses in Mexico can result in significant reductions in the number of parasites found in children, and the incidence of diarrheal diseases and anemia. These findings are suggestive that Progresa may be less effective in areas that do not meet a minimum level of sanitation.

Acknowledgements

I would like to thank Elisabeth Sadoulet, Alain de Janvry, and Paul Gertler for their invaluable support of this work. I am also grateful to Jean Lanjouw, Guido Imbens, Orazio Attanasio, Terra McKinnish, and students and professors in the UC Berkeley development community, and participants of the 2006 Stanford SITE health and development workshop for their many useful comments. In addition, I am thankful to all those in the Department of Health Economics at the Mexican National Institute of Public Health, Mexican Ministry of Health, IMSS-Oportunidades and Oportunidades who graciously provided me with the data and their institutional knowledge. Finally, this work was made possible by the financial support of the Institute of Business and Economic Research at UC Berkeley and UC MEXUS.

References

Adato, M., Coady, D., Ruel, M., 2000. Final report: an operations evaluation of PROGRESA from the perspective of beneficiaries, promotoras, school directors, and health staff. International Food Policy Research Institute, Washington, DC.

Aguilar-Barradas, R., Méndez-Machado, G., Guevara-Arenas, F., Caballero-Leal, L., 2005. Mortalidad perinatal hospitalaria y su relación con la calidad de la atención prenatal. Revista Médica del Instituto Mexicano del Seguro Social 43 (3), 215–220.

Attanasio, O., Gómez, L., Heredia, P., Vera-Herández, M., 2005. The short-term impact of conditional cash subsidy on children's health and nutrition in Colombia. Center for the Evaluation of Development Policies, Institute for Fiscal Studies Report Summary Familias 03.

Barber, S., 2006. Does the quality of prenatal care matter in promoting skilled institutional delivery? A study in rural Mexico. Journal of Maternal and Child Health 10 (5), 419–425.

Barham, T., Maluccio, J., 2009. Eradicating diseases: the effect of conditional cash transfers on vaccination coverage in rural Nicaragua. Journal of Health Economics 38 (3), 611–621.

Barham, T., Brenzel, L., Maluccio, J.A., 2007. Beyond 80%: are there new ways of increasing vaccination coverage? Evaluation of CCT programs in Mexico and Nicaragua. The World Bank. Health, Nutrition and Population Discussion Paper.

Bautista-Arredondo, S., Bertozzi, S., Gertler, P., Martinez, S., 2006. How health care supply constraints limit the effectiveness of conditional cash transfer programs: evidence from Mexicos's Oportunidades program. University of California at Berkeley, unpublished manuscript.

Behrman, J., Hoddinott, J., 2005. Programme evaluation with unobserved heterogeneity and selective implementation: the Mexican PROGRESA impact on child nutrition. Oxford Bulletin of Economics and Statistics 67 (4), 547–569.

Bojalil, R., Guiscafre, H., Espinosa, P., Martinez, H., Palafox, M., Romero, G., Gutierrez, G., 1998. The quality of private and public primary health care management of children with diarrhea and acute respiratory infections in Tlaxcala, Mexico. Health Policy and Planning 13 (3), 323–331.

Braine, T., 2006. Mexico's quest for a complete mortality data set. Bulletin of the World Health Organization 84 (3), 161–256.

Bronfman-Pertovsky, M., Lopez-Moreno, S., Magis-Rodriguez, C., Moreno-Altamirano, A., Rutstein, S., 2003. Prenatal care in the primary level of health care: provider characteristics which influence users' satisfaction. Salud Publica de Mexico 45 (6), 445–454.

Case, A., 2004. Does money protect health status? Evidence from South African pensions. Perspectives on the Economics of Aging 287–305.

Cattaneo, M., Galiani, S., Gertler, P., Martinez, S., Titiunik, R., 2008. Housing, health and happiness. American Economic Journal of Economic Policy 1 (1), 75–105.

Charmarbagwala, R., Ranger, M., Waddington, H., White, H., 2004. The determinants of child health and nutrition: A meta-analysis. The World Bank. Independent Evaluation Group Working Paper.

Coady, D., 2000. The application of social cost–benefit analysis to the evaluation of Progresa: final report. International Food Policy Research Institute, Washington, DC.

de la Vega, S., 1994. Construcción de un Indice de Marginacion. Masters Thesis, Statistics and Operations Research, UNAM, Mexico City, Mexico.

de Onis, M., Villar, J., Gulmezoglu, M., 1998. Nutritional interventions to prevent intrauterine growth retardation: evidence from randomized controlled trials. European Journal of Clinical Nutrition 52 (S1), S83–S93.

de Silva, N., Sirisena, J., Gunasekera, D., Ismail, M., de Silva, H., 1999. Effect of mebendazole therapy during pregnancy on birth outcome. Lancet 353, 1145–1149.

Duflo, E., 2003. Grandmothers and granddaughters: old-age pensions and intra-household allocation in South Africa. World Bank Economic Review 17 (1), 1–25.

Evans, J. Levy, Hammitt, J., Santos-Burgoa, C., Castillejos, M., 2002. Chapter 4: health benefits of air pollution control. In: Molina, T., Molina, J. (Eds.), Air Quality in the Mexico Megacity: An Integrated Assessment. Kluwer Academic Publishers, Boston.

Fiszbein, A., Schady, N., 2009. Conditional Cash Transfers: Reducing Present and Future Poverty. The World Bank, Washington D.C.. World Bank Policy Research Report.

Gertler, P., 2004. Do conditional cash transfers improve child health? Evidence from PROGRESA's control randomized experiment. American Economic Review 94 (2), 336–341.

Gertler, P., Boyce, S., unpublished manuscript. An experiment in incentive-based welfare: The impact of PROGRESA on Health in Mexico. University of California at Berkeley.

Gilbert, G., 2002. Infections in pregnant women. Medical Journal of Australia 176 (5), 229–236.

Gonzalez-Cossio, R.H., Moreno-Macias, J. Rivera, et al., 2003. Breast-feeding practices in Mexico: results from the second national nutrition survey 1999. Salud Publica de Mexico 45 (4), S477–S489.

Hollier, L., Workowski, K., 2005. Treatment of sexually transmitted infections in pregnancy. Clinics in Perinatology 32 (3), 629–656.

Imbens, W. Guido, Wooldridge, Jeffrey M., 2008. Recent Developments in Econometrics of Program Evaluation, NBER Working Paper No. 14251.

Kramer, M., Kakuma, R., 2003. Energy and protein intake in pregnancy. Cochrane Database of Systematic Reviews 5, CD32.

Ladipo, O., 2000. Nutrition in pregnancy: mineral and vitamin supplements. American Journal of Clinical Nutrition 72 (S1), 280S–290S.

Lozano-Ascencio, R., 2008. Is it possible to improve the death registries in Mexico? Gaceta Medica de Mexico 144 (6), 525–533.

Lu, M., Tache, V., Alexander, G., Kotelchuck, M., Halfon, N., 2003. Preventing low birth weight: is prenatal care the answer? The Journal of Maternal–Fetal and Neonatal Medicine 13 (6), 362–380.

Maluccio, J., Flores, R., 2004. Impact evaluation of a conditional cash transfer program: the Nicaraguan Red de Protección Social. International Food Policy Research Institute, Washington, DC.

Mathers, C.D., Fat, D.M., Inoue, M., Rao, C., Lopez, A., 2005. Counting the dead and what they died from: an assessment of the global status of cause of death data. Bulletin of the World Health Organization 83 (3), 171–177.

Mishra, V., Thapa, S., Retherford, R., Dai, X., 2005. Effect of iron supplementation during pregnancy on birthweight: evidence from Zimbabwe. Food Nutrition Bulletin 26 (4), 484–495.

Morales-Espinoza, E., Sanchez-Perez, H., Garcia-Gil, M., 2003. Intestinal parasites in children, in highly deprived areas in the border region of Chiapas, Mexico. Salud Publica de Mexico 45 (5), 379–388.

Moss, W., Darmstadt, G., Marsh, D., Black, R., Santosham, M., 2002. Research priorities for the reduction of perinatal and neonatal morbidity and mortality in developing country communities. Journal of Perinatology 22 (6), 484–495.

O'Ryan, M., Prado, V., Pickering, L., 2005. A millennium update on pediatric diarrheal illness in the developing world. Seminars in Pediatric Infectious Diseases 16 (2), 125–136.

Parker, S., Teruel, G., 2005. Randomization and social program evaluation: the case of Progresa. Annals of the American Academy of Political and Social Science 599, 199–219.

Penchaszadeh, V., 2002. Preventing congenital anomalies in developing countries. Community Genetics 5, 61–69.

Phavichitr, N., Catto-Smith, A., 2003. Acute gastroenteritis in children: what role for antibacterials? Pediatric Drugs 5 (5), 279–290.

Rivera, J., Rodriquez, G., Shamah, T., Rosado, J., Casanueva, E., Maulen, I., Toussaint, G., Garcia-Aranda, A., 2000. Implementation, monitoring, and evaluation of the nutrition component of the Mexican social programme (PROGRESA). Food and Nutrition Bulletin 21 (1), 35–42.

Rivera, J., Sotres-Alvarez, D., Habicht, J.-P., Shamah, T., Villalpando, S., 2004. Impact of the Mexican program for education, health, and nutrition (Progresa) on rates of growth and anemia in infants and young children — a randomized effectiveness study. Journal of the American Medical Association 291 (21), 2563–2570.

Rosado, J., Rivera, J., Lopez, G., Solano, L., 2000. Development, production, and quality control of nutritional supplements for a national supplementation programme in Mexico. Food and Nutrition Bulletin 21 (1), 30–34.

Sepulveda, J.F., Bustero, R. Tapia, et al., 2006. Improvement of child survival in Mexico: the diagonal approach. The Lancet 368, 2017–2027.

Shaheen, R., de Francisco, A., El Arifeen, S., Ekstrom, E.-C., Ake Persson, L., 2006. Effect of prenatal food supplementation on birth weight: an observational study ferom Bangladesh. American Journal of Clinical Nutrition 83 (6), 1355–1361.

Skoufias, E., McClafferty, B., 2001. Is Progresa working? Summary of the results of an evaluation by IFPRI. International Food Policy Research Institute, Washington D.C.

Skoufias, E., Davis, B., Vega, S., 1999. Targeting the poor in Mexico: an evaluation of the selection of households into OPORTUNIDADES. International Food Policy Research Institute, Washington, DC.

Stecklov, G., Winders, P., Todd, J., Regalia, F., 2006. Demographic externalities from poverty programs in developing countries: experimental evidence from Latin America. American University Department of Economics Working Paper No. 2006-01.

Thapar, N., Sanderson, I., 2004. Diarrhea in children: an interface between developing and developed countries. Lancet 363, 641–653.
Tome, P., Reyes, H., Pina, C., Rodriguez, L., Gutierrez, G., 1997. Characteristics associated with the under-registration of deaths in the state of Guerrero, Mexico. Salud Publica de Mexico 39 (6), 523–529.
USAID, 2008. USAID Country Health Statistical Report, Mexico.
Victora, C., Kirkwood, B., Ashworth, A., Black, R., Rogers, S., Sazawal, S., Campbell, H., Gove, S., 1999. Potential interventions for the prevention of childhood pneumonia in developing countries: improving nutrition. American Journal of Clinical Nutrition 70 (3), 309–320.
Viscusi, K., Aldy, J., 2003. The value of a statistical life: a critical review of market estimates throughout the world. Journal of Risk and Uncertainty 27 (1), 5–27.
World Bank, 2003. Millennium Development Goals [on-line]. [Accessed on October 21]. Available from the World Wide Web <http://www.ddp-ext.worldbank.org/ext/MDG/gdmis.do>.
World Bank, 2005. World Development Indicators 2005. The World Bank, Washington, D.C.
Zar, H., Mulholland, K., 2003. Global burden of pediatric respiratory illness and the implications for management and prevention. Pediatric Pulmonology 36 (6), 457–461.