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Providing a Healthier Start to Life: The Impact of Conditional Cash Transfers on Infant Mortality*

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Abstract

In this paper, I evaluate the impact of Mexico's conditional cash transfer program, Progresa, on infant mortality. While studies on other aspects of Progresa make use of a randomized treatment and control evaluation database performed in 506 communities, this database lacks sufficient sample size to measure the effect on infant mortality. Instead, I use vital statistics data to determine municipality-level, rural infant mortality rates and create a panel dataset covering the period 1992-2001. I take advantage of the phasing-in of the program over time both between and within municipalities to identify the impact of the program. I find that Progresa led to an 11 percent decline in rural infant mortality among households treated in Progresa municipalities. Reductions are as high as 36 percent in those communities where, prior to program interventions, the population all spoke some Spanish and had better access to piped water.

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1 Introduction

Every year more than 10 million children die from preventable diseases such as malnutrition and intestinal infections in developing countries (World Bank, 2003). The majority of these deaths take place during infancy, before the child reaches the age of one.[1] Consequently, finding effective policies to reduce mortality among infants is a key part of the development agenda. This is evidenced by the selection of infant mortality as one of the targets of the Millennium Development goals (World Bank, 2003). Conditional cash transfer programs are a new type of social investment tool designed, amongst other goals, to improve the health of children, but which may also lead to important reductions in infant mortality. However, empirically establishing causality between the implementation of conditional cash transfers and infant mortality is difficult because the death of an infant is a relatively rare event. Thus, large sample sizes are a requirement for accurate estimation. Even large household surveys commonly do not have a sufficient number of observations to examine infant mortality. In 1997, Mexico implemented one of the first, largest, and most innovative conditional income transfer programs, Progresa.[2] Owing to its extensiveness, Progresa provides an opportunity to test the causality of conditional cash transfers on the infant mortality rate (IMR).[3] In this paper, we use non-experimental methods that exploit the phasing-in of Progresa over time throughout rural Mexico to examine if this new policy tool reduced the rural IMR in Mexico.

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[1]According to the World Bank's World Development Indicators, the 2002 mortality rate for children (the probability that a child dies before reaching the age of five per 1000 live births) in low and middle income countries was 88, while the infant mortality rate (the probability that a child dies before the age of one per 1000 live births) was 60.
[2]Progresa stands for Programa de Educatió n, Salud y Alimentació n. This program is now known as Oportunidades.
[3]The infant mortality rate is defined as the number of children in a given year who die before the age of one per 1000 live births in the same year.

Progresa differs from typical income transfer programs since the cash transfers to beneficiary households are made conditional upon household members engaging in a set of actions designed to improve their health, nutrition and education status. The aim of the program is to build the human capital of young children and thereby break the intergenerational transmission of poverty. Previous research on Progresa has taken advantage of a randomized treatment and control evaluation database to investigate if the program improved various aspects of children's health.[4] This research has shown that the nutritional status of children improved and the number of days a mother reported her child ill decreased for treatment households as compared to those from similar families that did do not receive the transfer (Behrman and Hoddinott, 2001; Gertler and Boyce, 2001; Gertler, 2004). These findings indicate that there are some important nutritional benefits of conditional cash transfers, but most other indicators of children's health used in these studies rely on parent's recall and perceptions of good health which have potential reporting biases. This paper therefore focuses on infant mortality, which is a broader and more objective measure of children's health.[5]

In addition, the sample size in the Progresa randomized treatment and control database is too small to accurately estimate the impact of the program on infant mortality. This paper resolves the sample size problem by constructing a panel data set of 2,399 municipalities[6] from 1992 to 2001 and uses a non-experimental research design. The treatment effect of Progresa on

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[4]The evaluation database is a panel of household surveys that contains information on the treatment and control households both before and after the intervention.

[5] The IMR is commonly used as a primary indicator of children's health, especially in developing countries. This is partly due to inadequate information systems to gather data on child morbidity in many countries, and because obtaining objective measures of children's health that does not rely on parent's recall or perceptions of good health is difficult. In addition, infants are especially susceptible to many common diseases. Thus, their death rate serves as an indicator of the overall health of children in areas that suffer from high rates of preventable diseases (Lederman, 1990).

[6]In the 2000 Census there were 2445 municipalities in Mexico with an average population of 40,000 people and an average size of 800 square kilometers. They are often compared to the size of a county in the US.

rural infant mortality is identified using the phasing-in of the program over time in rural Mexico. This leads to a variation in the intensity of treatment indicator -- the percent of the rural population covered by the program -- both within and between municipalities. The econometric model employs municipality and time fixed effects, and includes variables associated with the program phase-in rule to control for program timing bias. The analysis also explicitly controls for changes in the supply of health care in rural areas. Additionally, the identification strategy takes advantage of the fact that Progresa was not provided in urban areas prior to 2000, and uses the urban IMR to test whether unobservable municipal time-variant variables are biasing the results. Using these techniques, we find that the program led to a reduction of approximately 2 deaths per 1000 live births among program participants. From an average IMR of 18, this is an 11 percent reduction. Reductions in infant mortality were even higher in Progresa areas where, prior to the program, houses had better access to piped water, fewer sewage systems, and in areas where the population spoke some Spanish.[7] Furthermore, robustness checks show that the program had no spurious impact on urban infant mortality, and also show that the impact is not the result of an endogenous increase in the number of live births.

With the exception of Progresa, there is very little evidence at this time of the causal impact of conditional or unconditional cash transfer programs on children's health outcomes or mortality in other developing countries. Results from the Colombian conditional cash transfer program show that while the number of episodes of acute diarrhea decreased among children less 6 years of age, there was no improvement in nutrition (Rawlings, 2004). In contrast, the conditional cash transfer program in Nicaragua led to a significant reduction in malnutrition

[7]Mexico has a large indigenous population and there are areas where this population does not speak Spanish.

(Maluccio & Flores, 2004). Studies on the effect of increasing the amount and coverage of the social pension program in South Africa for the elderly black population found that income transfers also led to nutritional improvements among girls (Duflo, 2003; Case, 2001). The present study therefore makes an important contribution to the literature on health impacts of cash transfer programs by investigating a different and important children's health indicator, infant mortality. It is also the first study to use government administrative data to investigate outcomes of conditional cash transfer programs that could not have been studied otherwise.

The remainder of the paper is organized as follows. Section 2 describes the Progresa program including the targeting mechanism and the phase-in rule. A description of the data is provided in section 3. The identification strategy, including an explanation of the sources of variation in the treatment variable and the empirical model is presented in section 4. Results are provided in section 5 and section 6 concludes.

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 Mexico. The program targets the rural poor reaching nearly 2.5 million rural households by 2000. The Progresa model is extremely popular throughout the Latin American region and has been adopted by Argentina, Colombia, Honduras, Jamaica, and Nicaragua.

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 provides an increase utilization of health and education services. While the program was first introduced in rural areas, it expanded into urban areas in 2000. This study focuses on the rural program.

The health component of Progresa was designed to address many recalcitrant 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 live. In addition, the cash transfers are conditional on the household's participation in four important health activities:

1. growth monitoring from conception to age 5;
2. regular preventative health check-ups for all family members, including prenatal care, well baby care and immunizations;
3. mother's attendance at health, hygiene and nutrition education programs; and
4. children ages 0-2 and pregnant and lactating women taking nutritional supplements.

Adequate prenatal care, medical assistance at birth, immunizations and good breast-feeding practices -- all aspects of the Progresa program -- are known to be important for proper in-uterine growth of a child and for reducing the probability of infant death (Murata et. al., 1992; Costello and Manandhar, 2000; World Bank, 2003). Thus, we may expect the program to reduce infant mortality. In fact, research has shown that programs in the US that target poor families and are similar to Progresa in terms of the type of health interventions, but do not provide an income transfer, have led to reductions in infant mortality (Currie and Gruber, 1996; Devaney et. al., 1990).

Since it was expected that health care utilization would rise as a result of the program, Progresa coordinated with other government ministries responsible for health care delivery to ensure an adequate supply and quality of health care in program areas. In addition, the program used mobile clinics and foot doctors to reach many marginalized communities that did not have access to permanent health clinics.

2.2 Targeting and Program Phase-In

Progresa used a two-stage process to identify eligible beneficiary households in rural areas (Skoufias et. al., 1999). In the first stage, rural localities[8] were selected. Localities with 2,500 inhabitants or less are denominated as rural.[9] In order to meet the program's objectives, localities where chosen based on a number of attributes. Localities were first ranked by a marginality index[10] and only those with a high marginality[11] were 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.[12] 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

[8]A locality is a cluster of inhabited houses that can vary in size from 1 dwelling to over a million and has an average population size of 489. Localities are grouped into municipalities. The 2000 census recorded that there were 199,391 localities in 2,445 municipalities in Mexico. This leads to an average of 80 localities in a municipality with the range from 1 to 1630. A municipality is approximately 100 times larger than a locality with an average population of 40,000 as compared to 489 in 2000. The average population in rural areas of a municipality is 10,306, while the mean population of a rural locality is 125.
[9]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.
[10]This index is constructed using the principal components method. The variables that make up the index include: 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.
[11]The marginality index was divided into quintiles based on the degree of marginality (for details, see de la Vega, 1994). A grade of 5 indicates a high level of poverty and a grade of 1 a low level of poverty. Only those localities with a marginality grade of 4 or 5 were considered.
[12]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 kilometers away (Skoufias et. al., 1999).

than 50 inhabitants or that was determined to be geographically isolated was excluded from the program.

While the exact program phase-in rule is not clearly documented, the general criteria are known (Skoufias et. al., 1999). For logistical and financial reasons, the program was phased-in over time starting with 2,578 localities in 7 out of 32 states in 1997 (Figure 1). 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. In 1999, 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.

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. Lastly, the list of potential beneficiaries was presented to a community assembly for approval. As a result, a different percent of the rural population is covered by the program in each locality. Recertification of eligibility began in 2000.

2.3 The Randomized Experiment

A prominent feature of Progresa is the randomization of 506 program localities in seven states into treatment and control groups. Eligible households in treatment villages received benefits immediately, while eligible household in control villages became part of the program about 2 years later. A baseline survey was performed in October 1997 and six follow-up surveys were taken at approximately 6 month intervals. The design was created in order to ensure rigorous evaluation of the program impacts. The delay in the implementation of the program in control villages was justified since the government lacked sufficient funds to provide the program nationally from the outset. While many studies on Progresa take advantage of these data, there are only two deaths of children under age one in the control areas in the post-intervention period. For this reason, we use vital statistics data and a different identification strategy to study the program impacts on IMR as explained in the following sections.

3 The Data

We construct infant mortality rates using 1992-2001 vital statistics data. The mortality data are from a nation-wide database containing information on every registered death in Mexico and were provided by the Mexican Ministry of Public Health. While these data are available at the municipality level, they do distinguish whether the death occurred in a rural or urban locality within that municipality. The live birth data are publicly available for every municipality in Mexico from the Mexican Statistical Agency, INEGI, except for the state of Oaxaca in the year 2000.[13] These data are provided annually by municipality and size of the locality where the mother who gave birth resided. The rural and urban infant mortality data are constructed by linking these two databases by municipality.[14]

[13]While the urban and rural breakdown of the number of live births is missing for Oaxaca, the total number of births is available from INEGI. To fill in the missing values for the number of rural births in 2000, we calculated the average of the ratio of rural to total birth for 1999 and 2001 in Oaxaca. We multiply this ratio by the total number of births in 2000. We used a similar process to determine the number of urban births.

[14]Values for municipal rural infant mortality rates greater than 240 were set to missing. These values were removed from the analysis because they are outliers. Removal of these values affected a total of 58 observations or less than 0.3 percent of the data.

The intensity of treatment indicator is the percent of rural households in a municipality receiving Progresa benefits. It is determined using Progresa administrative data and INEGI census data. Progresa provided administrative data on 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 (Figure 1). However, we aggregate these data to the municipality level since the infant mortality rate is only available at this level. Using INEGI census data on the number of rural and urban households in a municipality for 1990, 1995 and 2000, we linearly interpolate the number of households for each year between 1992 and 2001. Thus, the percent of rural households receiving program benefits is simply the ratio of the number of beneficiary households to the total number of households in rural areas of a municipality.[15]

A variety of municipality characteristics are used as control variables in the analysis. The marginality index is publicly available at the locality and municipality levels on the CONAPO website for 1990, 1995 and 2000. Health supply data are not publicly available; we collected them from the Ministry of Health and IMSS-Oportunidades at the locality level. Data on other municipality characteristics were obtained from the INEGI 1995 Conteo[16] and 2000 Census and are also at the locality level. Here again, this locality data is aggregated to the municipality level. Lastly, the INEGI 1990 Census is used to provide information on some locality characteristics.

Using these data, a municipality-level, panel dataset was constructed from 1992-2001. However, municipality boundaries were redefined during this time period. In order to make a

[15]Approximately 2 percent of all positive values of the treatment variable are greater than one. These values are set to missing.

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

consistent panel of municipalities from 1992-2001, municipalities which were split in a particular year are amalgamated. This results in a balanced panel of 2,399 municipalities.

4 Identification Strategy

4.1 Sources of Variation

The objective is to estimate the treatment effect of Progresa on rural infant mortality. Ideally, we would compare the IMR in treated rural localities with the counterfactual ─the IMR had Progresa not been available in the locality. Since the counterfactual is never observed, we would take advantage of the phasing-in of the program over time and use rural localities yet to be treated as the comparison group. Since infant mortality is not available at the locality level, we instead investigate the impact of the program on municipality-level, rural IMR. Similar to localities, new municipalities came onto the program over time between 1997 and 2001 (Figure 2) leading to variation in the intensity of treatment across municipalities over time. Therefore, 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 the comparison group are the same as in the treatment group had they not received the program. Although it is not possible to test this assumption, we can test that the pre-intervention trends in infant mortality 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.

We test that the pre-intervention trends in rural IMR, IMR^r, between municipalities that joined the program in different years are the similar. Two sets of dummy variables are used

$ENTERk$ and $YEARj$, where k=1998-2001 and j=1991-1996. $ENTERk$ takes on the value 1 if the first program locality of municipality m was phased-in during year k, and is zero otherwise. $YEARj$ are year dummy variables for 1991-1996 (years prior to the program introduction). Using data prior to 1997, the equation used to test the difference in trends is:

$$IMR^r_{mt} = \beta_0 + \sum_j \beta_j YEARj_t + \sum_j \sum_k \theta_{jk} YEARj_t * ENTERk_m + u_{mt} \qquad (1)$$

If the θ 's are not significantly different from zero, then the pre-intervention trends do not statistically differ between municipalities entering the program in subsequent years. Results are reported in Table 1. With the exception of the group of municipalities that joined the program in 2001 and those municipalities that have no Progresa, the results show that the pre-intervention trends in the rural IMR are not significantly different from municipalities that entered the program in 1997. Municipalities that joined the program in 2001 and those that do not have Progresa will therefore not be included in the comparison group.

Not all Progresa localities within a municipality were phased-in to the program during the same year. As a result, the program intensity also varies over time within a municipality. For example, Table 2 shows that there were 2,424 Progresa localities in 1997. In 1998, the number of Progresa localities in those same municipalities almost doubled to 4,705. This variation in program intensity within a municipality over time is another source of variation used to identify the program impact.

Results may be biased if Progresa localities that were phased-in during different years within a municipality are not similar. One way to reduce this bias is to control for the program phase-in rule. Since localities that joined the program in 1997 had better access to permanent health care clinics than those that joined the program later, we control for changes in the supply of health care in rural municipalities, as well as the percent of Progresa localities with access to a permanent health care clinic. Furthermore, localities with lower population densities were phased-in during 1999. While we do no know the density of rural areas of the municipality we can control for the density as a whole for the municipality.

Ideally, we would also test that the pre-intervention trends in rural IMR are the same between localities that were phased-in to the program in the various years. Since these data do not exist, instead we examine if locality characteristics in the pre-program period (1995 or 1990), and the change in locality characteristics between 2000 and the pre-program period are the same across phase groups. To the extent that the level and change in locality characteristics are correlated with the trends in rural IMR, their similarity across phase groups is an indication that the trends in rural IMR are also likely to have been similar in these localities.

Table 3 presents the difference in locality characteristics across phase groups in the pre-intervention period. The means for localities that were incorporated into the program in 1997 (phase group 1997) are reported in the first row. The difference in the locality characteristics between phase group 1997 and each of the other phase groups are reported in subsequent rows. These differences in these means are almost all significant. With the exception of the percent of population with dirt floors in 1990 and localities that where brought into the program in 2001, means are within 10 percentage points. While these differences are arguably small, there is concern that they could bias the results. The trends in the infant mortality rate between phase groups may be more likely to be determined by the changes in locality characteristics rather than their level. Table 4 presents the change in mean locality characteristics between 2000 and 1995 for localities that were phased-in during 1997 in the first row. The subsequent row show how this change differed between the 1997 phase-in group and those localities the joined the program in later years. Now the majority of the differences in the changes between phase group 1997 and each of the other groups are not significant (Table 4). In order to account for these differences in the observables, these variables are included as covariates. If the findings do not vary when these variables are included, it is hoped that similar changes in the unobservables would also not bias the results. However, locality observables must be aggregated to the municipality level in order to be included in the analysis.

In addition, inclusion of municipality fixed effects controls for biases due to differences in time-invariant variables across municipalities arising from non-random program placement (Rosenzweig and Wolpin, 1986). The estimate of the treatment effect will be unbiased if there are no unobserved time-varying municipality characteristics or trends that are correlated with the intensity of treatment variable. If this is the case, the urban infant mortality rate should not be affected since the program targeted rural areas. However, if there were important omitted municipality time trends correlated with the treatment variable, we would expect to find an impact of the program on urban infant mortality due to the unobservables. Therefore, in the results section we also present results for urban IMR to test if there are municipality time trends that could be biasing the results. Lastly, we will also present a validity check where we include a time trend for each municipality to account for further variation over time between municipalities resulting from to these unobservables.

4.2 Graphical Analysis

Due to the variation in the intensity of treatment both between and within municipalities over time, it is difficult to show the treatment effect graphically. However, graphs can provide suggestive evidence. In Figures 3-5, trends in average municipality rural IMR are provided for three groups of municipalities, based on the year the program was first offered in the municipality. Only municipalities that entered the program in 1997, 1998 and 1999 are shown on the graphs. Municipalities that entered in 2000 are not displayed since there are just 12 observations. Those that joined in 2001 are also excluded since the pre-intervention trend for this group is statistically different from the other municipalities. Trends in urban IMR over the same time period are presented in Figure 6. Finally, since program intensity varies between municipalities, trends in rural IMR are also presented only for municipalities that had an average program intensity of 30 percent or more over the program period (Figure 7).

If Progresa is successful, there should notice a break in the trend in rural IMR soon after the program entered the municipality. However, since the program intensity increased over time within a municipality, these breaks may not be visible in the first year of the program. Mean municipality program intensity by year for each of the three groups is presented in Table 5. The first group of municipalities began to receive the program in 1997. Only 24 percent of rural households in these municipalities were covered by the program in that year. In 1998, the program was greatly expanded covering 55 percent of rural households in these same municipalities. Thus, there may be a larger impact of the program in 1998 rather than 1997 for this group. Figure 3 demonstrates that this is indeed the case for the municipalities that entered the program in 1997. The break in the trends for the two other groups occurs the year the program entered the municipalities. We verify that these breaks are not due to general trends in the municipalities by presenting a similar graph for urban IMR. As expected, there are no breaks in the trend in urban IMR the year the program entered the municipalities.

4.3 Empirical Model

We develop the empirical model by first considering a cohort of infants that dies in year t, in municipality m. Whether an infant dies, $(D = 1)$, during that year depends on (i) whether the infant was born in a household registered for Progresa benefits or not that year, and if the infant's mother was registered for the program during her pregnancy, H^t, H^{t-1}, H^{t-2}; (ii) mother and household characteristics, I, and; (iii) municipality characteristics such as the supply of health care or the quality of the environment (both time-varying and time-invariant), X. Time fixed effects are included to control for time trends. Assuming a linear relationship,

$$\Pr(D_{imt} = 1) = \alpha_t + \sum_j \beta_j H_{imt}^{t-j} + \sum_g \phi_g I_{imtg} + \sum_p \phi_p X_{mtp} + \varepsilon_{imt}, \qquad (2)$$

where imt indexes infant i born alive in municipality m in year t, and $j = 0 - 2$. Year fixed effects are represented by α_t, and ε_{imt} is the error term, which is assumed to have a zero mean and be orthogonal to the independent variables.

There are a number of variables in equation individual that are not observed in the data. The indicator variable H_{imt} (if child imt is from a program household or not) does not exist at the individual level in the dataset, however, the probability of treatment at the municipality level does. This probability is the percent of live births to beneficiary households in municipality m in year t, and is the same for all infants in the municipality. Thus, we use this value in lieu of the individual H_{imt}. Also, mother and household characteristics of the infant are not available in the Mexican vital statistics.

Given the lack of individual-level data and because mortality is identified at the municipality level, equation individual is aggregated to the rural municipality level as follows:

$$\sum_{i \in I_m} D_{imt} = N_{mt}\alpha_t + \sum_j \beta_j PB_{mt}^{t-j} + N_{mt}\sum_p \phi_p X_{mtp} + \sum_{i \in I} \varepsilon_{imt}, \tag{3}$$

where N_{mt} is the population of the infants (<1 year old) born alive in the rural areas of municipality m in year t and I_m is the set of infants born alive in municipality m. The dependent variable is now the number of deaths among infants born alive in a municipality in a given year, and the treatment variable, PB_{mt}, is the number of live births in municipality m in year t to Progresa households in year $t-j$. To make comparisons across municipalities, equation summation is normalized by the number of live births in each municipality. At the municipality level, the equation is written as follows:

$$\frac{1}{N_{mt}}\sum_{i \in I} D_{imt} = \alpha_t + \sum_j \beta_j \frac{PB_{mt}^{t-j}}{N_{mt}} + \sum_p \phi_p X_{mtp} + \sum_{i \in I} \frac{\varepsilon_{imt}}{N_{mt}}. \tag{4}$$

The database provides information on the number of program households not the number of births to Progresa households, PB. Assuming that the fertility rate remains constant over the period of the program (1997 - 2001), we redefine $\frac{PB_{mt}^{t-j}}{N_{mt}}$ to be the ratio of the number of beneficiary households over the total number of households in rural areas of the municipality in a given year. This is the intensity of treatment or program intensity variable, referred to as *Intensity*. Municipality fixed effects are also added to equation normalize to control for time-invariant municipality characteristics that could be correlated with both infant mortality and program intensity due to program placement bias.

The estimation equation is

$$IMR^r_{mt} = \alpha_t + \tau_m + \sum_j \beta_j Intensity^{r,t-j}_{mt} + \sum_p \phi_p X^r_{mtp} + u_{mt}, \qquad (2.5)$$

where the r superscript is added to emphasize that these data are for rural areas of the municipality. The dependent variable is now labeled IMR^r since it is a measure of the rural infant mortality rate. Heteroskedasticity and serial correlation maybe both be present in the error term. Thus, the regressions are weighted by the number of rural households[17] and robust standard errors that are corrected for serial correlation[18] are used. The estimate of the treatment effect of Progresa on the treated is measured by the β 's, while the average treatment effect can be calculated by multiplying the impact on the treated by the average of the *Intensity*.

5 Results

5.1 General Impact of the Program

We start by estimating the treatment effect of Progresa on the rural IMR. Columns 1 through 5 of Table.6 present different specifications for estimating this impact. The adjusted R^2 is the same for each of the specifications, and the lag of the treatment variable, *program intensity*, consistently provides the only significant result. Therefore, the specification depicted in column 5, which includes only the lag of program intensity as an explanatory variable, is the primary estimation of the treatment effect. This result shows that among the treated the probability of an infant dying is reduced by almost 2 deaths per 1000 live births on an average of 18 deaths, or 11 percent. At the municipality level, the percent of rural households covered by

[17]While the equations suggest weighting by the number of live births, this variable suffers from under-reporting in Mexico so the number of rural households is used because it provides a more consistent weight.
[18]The correction for serial correlation is made by clustering the standard errors at the municipality level.

the program reached an average of 47 percent. Therefore, the average treatment effect is a 5 percent reduction in the rural IMR.

5.2 Spillover Effects

Reduction in infant mortality among the treated may be overestimated due to the inability to exclude non-eligibles (non-poor in a locality) from benefiting from the improved health supply or due to program spillover effects. While cash transfers are only provided to beneficiaries, improvements in the health supply associated with the program could potentially lead to mortality reduction in the non-eligible group. Furthermore, program beneficiaries may inform those not in the program of the health gains they experienced from increased health care utilization or share their knowledge from the health education session. These health spillover effects could also generate lower infant mortality rates among the untreated.

Bobonis and Finan (2002) study health spillover effects and find no indication of such effects on the incidence of illness or on self-reported health indicators for children. This provides partial evidence that spillover effects may not be a concern. However, it may be that women's health behaviors during pregnancy and their child's infancy are not related to behaviors that affected the children's health outcomes mentioned above. While this question can be investigated further using the randomized treatment and control evaluation database, the average treatment affect reported in this paper provides a lower bound on the impact of the program on the treated.

5.3 Validity Checks

Although the model controls for time-invariant unobserved municipal heterogeneity, it cannot control for unobserved time-varying municipality factors that may be correlated with the treatment variable and infant mortality. We take advantage of the fact that Progresa mainly operated in rural localities before 2001 and test whether the program had a significant impact on urban IMR.[19] If there are indeed municipal-level omitted variables, *program intensity* might also impact urban IMR due to these unobservables. Table 6, column 6, shows that the program had no significant impact on urban IMR, thereby providing some evidence that unobservables are not biasing the results.

A further concern is that during program implementation there was an expansion of health care in rural communities. To control for possible biases, information on per capita health care infrastructure and personnel are included in the regression equation. Although many of these regressors are likely to be endogenous, if their inclusion does not influence the coefficient on the lag of the *program intensity*, we gain some confidence that health care supply is not correlated with the phasing-in of the program. The results in columns 1 to 3 of Table 7 demonstrate that the program impact remains unchanged.

During the first three years of the program, two criteria for choosing localities were relaxed. After 1997, the condition that beneficiaries had to have access to permanent health clinic no longer applied as mobile clinics and foot doctors also provided health care in many areas. Also, in 1999, localities that had a lower population density and were isolated from other Progresa localities were incorporated in the program. We include a variable defined as the percent of rural Progresa localities with access to permanent health clinic in a given year to take into account the first change in the phase-in rule. The addition of this control has almost no effect on the estimate of the impact and is not significantly different from zero (Table 7, column

[19]There are some semi-urban localities that joined the program before 2000. The program did expand to urban localities in 2000 but this should not affect our analysis.

4). Additionally, we control for the density of the municipality and the inclusion of this variable also does not change the estimate of the impact (Table 7, column 5).

We also control for all other observable time-varying municipality characteristics and individual municipality time trends (Table 8). The municipality characteristics are generated from the locality census data and are the municipality means made by aggregating data for localities that received Progresa benefits before 2001.[20] The results do not differ if municipality characteristics for the rural areas or the municipalities as a whole are used. Columns 1-8 clearly show that adding the available covariates does not affect the estimate of the impact. However, once a time trend is added for each municipality to account for the trends in unobservables, the impact of the program on infant mortality is higher. Progresa leads to a reduction of approximately 3 deaths per 1000 live births, or 17 percent among the treated. This estimate is still inside the 95 percent confidence interval for the impact of the program with the individual time trends in column 5 of Table 7. However, the result suggests that omitted other time-varying municipal characteristics may result in an under-estimate of the effect of Progresa on infant mortality.

Finally, as discussed earlier, the means and changes in means of locality characteristics across phase-in groups were arguably small but significantly different. Using data on 1995 locality characteristics, we estimate the municipality mean by aggregating the data only for localities that received Progresa for that particular year. So, as localities are phased-in, the municipality mean will change to reflect the difference in pre-intervention characteristics of the phase-in groups.[21] Results are presented in Table 9 and demonstrate that the point estimate of

[20]At present, the locality data is only available for 1995 and 2000. Therefore, we linearly interpolate between these points to generate data for the missing years.
[21]The municipality mean is set to zero in the time period before Progresa is available in a municipality.

the treatment effect varies from -1.6 to -2.6. However, none of these values are significantly different from the comparable program impact of -1.86 in column 5 of Table 7.

5.4 Under-Reporting of Births and Death

Under-reporting of both births and deaths is common in rural Mexico. The fact that the urban municipality IMR is higher than the rural municipality IMR is partly a reflection of this phenomenon. As long as the under-reporting does not change in a manner that is correlated with the lag of *program intensity* the estimates will be unbiased. However, one might be concerned that mothers in program areas may be more likely to register their child's birth in hopes of receiving a cash transfer in the future. Or, more babies may be born alive due to increased prenatal care utilization or improved mother's health. Thus, it is possible that the program impact is a result of an increase in the number of registered live births rather than a reduction in mortality. To investigate if this is the case, the impact of Progresa on the number of registered lives births per 1000 population in a municipality is also examined. Results in Table 10 demonstrate that the treatment variable, the lag of *program intensity*, had no impact on the number of live births per 1000 population. Thus, the estimate of the program impact is not the result of an endogenous increase in the number of births[22].

5.5 Heterogeneity of the Treatment Effect

Data from 1995 is used to examine if the program impact varies by pre-intervention characteristics of Progresa areas within the municipalities.[23] Findings from Table 11 highlight

[22]Skoufias, 2001 reports a similar result.
[23]Since the 1995 Conteo data is available at the locality level, it is possible to calculate the characteristics of just the localities that eventually receive Progresa in a municipality.

that the program was more successful at reducing infant mortality in municipalities where Progresa areas had better access to piped water, less access to sewage systems, and where all the population spoke some Spanish. The treatment effect does not vary due to differences in the percent of households with electricity[24] or the percent of the population 15 years of age or older that are literate.

In particular, program impacts are higher in municipalities where at least 75 percent of households in Progresa localities had access to piped water prior to the intervention. Approximately a third of the Progresa municipalities fall into this group. The treated in these municipalities experienced a reduction in infant mortality of approximately 5 deaths per 1000 live births, while those in areas with less access to piped water only experienced a reduction of 1.7 deaths. Given that the mean rural IMR over the sample period for the group of municipalities with better access is 19 as compared to 17 in areas with less access, this represents a decline in infant mortality of 28 and 10 percent respectively. The average percent of beneficiary rural households in municipalities in 1999 for these same groupings is 40 as compared to 46. Therefore, the average treatment effect of the program resulted in a 4 percent reduction in rural IMR in those municipalities where access to piped water is lower and a 12 percent decline in those municipalities with better access to piped water 1995.

The program also led to a much greater reduction in rural IMR in Progresa localities where the population over four years of age all spoke some Spanish. This is the case for 57 percent of the municipalities in the estimation sample. In particular, the rural IMR for the treated declined by 6 deaths per 1000 live births, on an average rural IMR of 17, or 33 percent. The average intensity of treatment in these municipalities reached 35 percent, so for these

[24]This is significant at the 10.5 percent level.

municipalities as a whole the infant mortality rate declined by 13 percent. In contrast, the rural infant mortality rate declined by 2 deaths per 1000 live births in areas where some of the population in Progresa areas only spoke an indigenous language. The mean rural IMR was 18 and the program intensity reached 53 percent in these areas. Therefore the rural IMR fell by 11 percent among the treated and 6 percent on average in these municipalities.

Lastly, the reductions in rural IMR mainly took place in the three quarters of the municipalities where less than 30 percent of the households in Progresa localities had some type of sewage system prior to program implementation. The decline in infant mortality among the treated in these areas is similar to the main impact of the program at 2 deaths per 1000 live births, or 11 percent.[25] The treated in those municipalities with better access to sewage experienced almost no decline in their infant mortality as a result of the program. However, the average rural IMR was also lower in these areas prior to the program at 17 as compared to 19.5 in areas with less access to sewage. This may seem contradictory to the results from piped water, but less than 35 percent of the municipalities had Progresa areas with both good access to piped water and sewage systems.

6 Discussion

The conditional cash transfer program, Progresa, led to a significant decline in infant mortality in rural Mexico. Findings suggest that the program resulted in an 11 percent reduction of the infant mortality rate among the treated. While we cannot test if there are spillover effects using the present dataset, their possible presence may lead to an over-estimation of the impact.

[25]Approximately 40 percent of the observations fall into the group with better electricity access. The mean IMR for this group is 19 as compared 17 in areas with less access, and the intensity of treatment is 40 percent in areas with more access as compared to 46.

The average treatment effect, which is a 5 percent reduction in the rural infant mortality rate in municipalities where some of the population received Progresa, is on the other hand a lower bound on the estimate of the impact on the treated. Given that on average the rural IMR fell by less than 1 percent each year between 1992 and 1996, these are large declines in infant mortality.

Program effects were even greater in areas where, prior to the program, Progresa localities had better access to piped water, and a population that spoke some Spanish. In particular, infant mortality declined by 28 and 33 percent among the treated in Progresa areas that had more piped water and not only indigenous language speakers respectively. The declines in infant mortality also mainly occurred in Progresa areas where fewer houses had a sewage disposal system prior to the program. The municipalities that had a relatively high level of sewage disposal, experienced little reduction in their mortality rate, though the mortality rate was lower in these areas before the program.

Unfortunately, it is somewhat difficult to interpret these results since these variables could be proxies for a number of different attributes. It is often argued that piped water is correlated with clean water; if this is the case, these findings highlight that there is an association between having safe drinking water prior to the program and more substantial reductions in the rural IMR from conditional cash transfers in Mexico. Also, if the presence of a sewage system is a proxy for a sanitary environment, larger reductions in rural IMR are also associated with areas that were less sanitary prior to the program. This may be a result of the health education component of Progresa. However, these are just hypotheses and these data cannot provide further evidence. They would be interesting questions to examine further using the nutrition information from the randomized treatment and control database.

We presented evidence on the internal validity of these results. We showed that the program did not lead to a reduction in the urban IMR which might have been the case if the phasing-in of the program over time was correlated with other municipality trends. We also controlled for the change in the supply of free health care in rural areas. This is important since Progresa worked closely with other ministries to ensure an adequate supply of health care. In addition, we provided evidence that the reduction in infant mortality is not the result of an endogenous increase in the number of live births.

It is also of interest to policy makers to understand the mechanisms that led to this reduction in infant mortality in Mexico. Extensions of this work will examine this question by taking advantage of the randomized treatment and control database to explore the kinds of health behavior changes that occurred as a result of Progresa. For example, among other factors we will explore: if treated babies weighted more at birth than non-treated babies; if treated mothers received more prenatal care, were more likely to have their delivery attended by a medical attendant, or had better knowledge of how to make oral rehydration salts; and, if treated families were more likely to make home improvements leading to a more sanitary environment.

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8 Tables and Figures

Figure 1: Trends in the Number of Progresa Beneficiary Families and Localities.

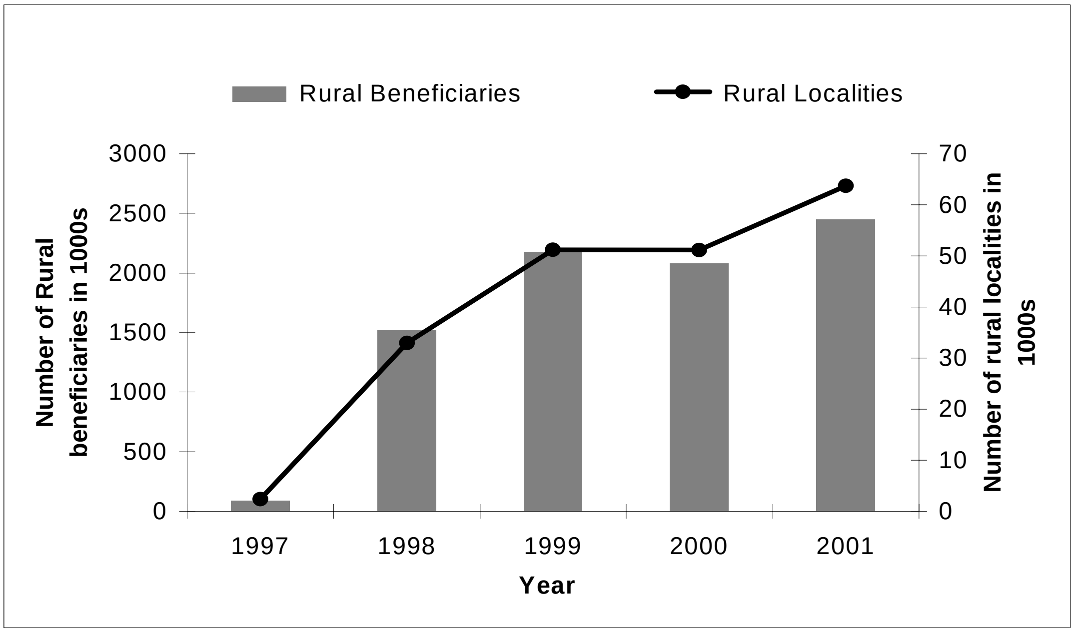


Figure 2: Number of New Program Municipalities by Year.

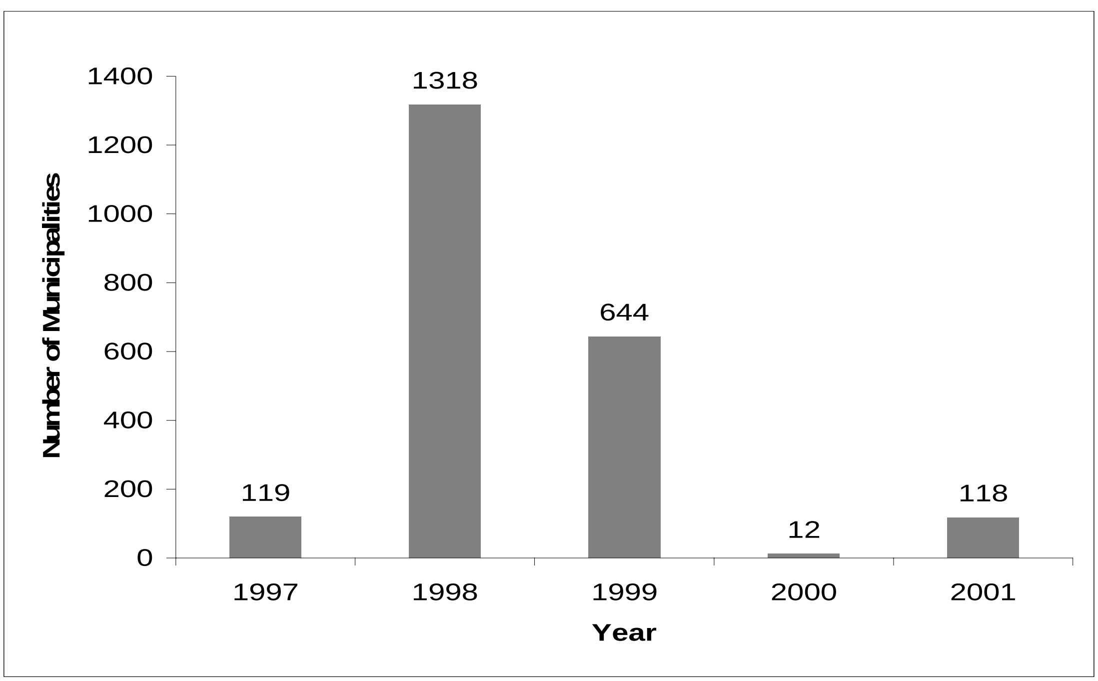


Figure 3: Trends in Rural IMR for Municipalities That Enter the Program in 1997.

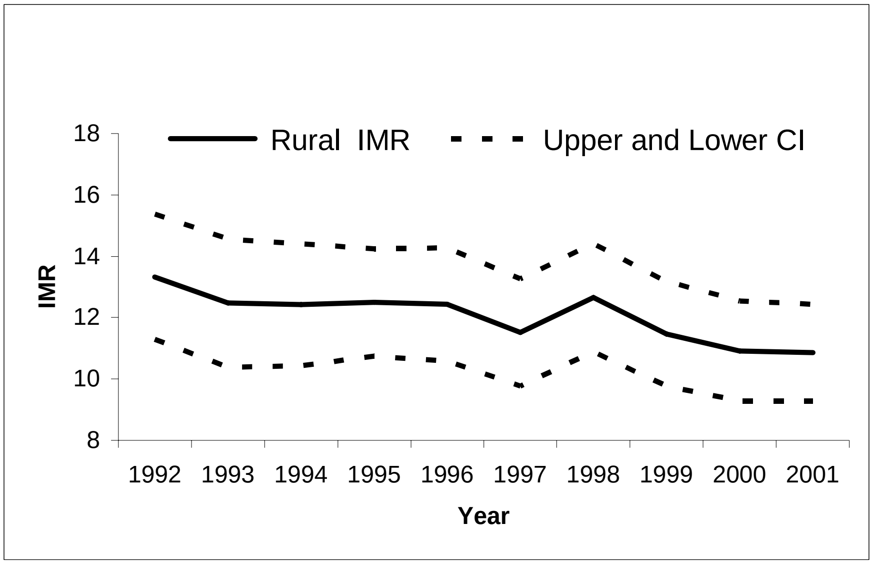


Figure 4: Trends in Rural IMR for Municipalities That Entered the Program in 1998.

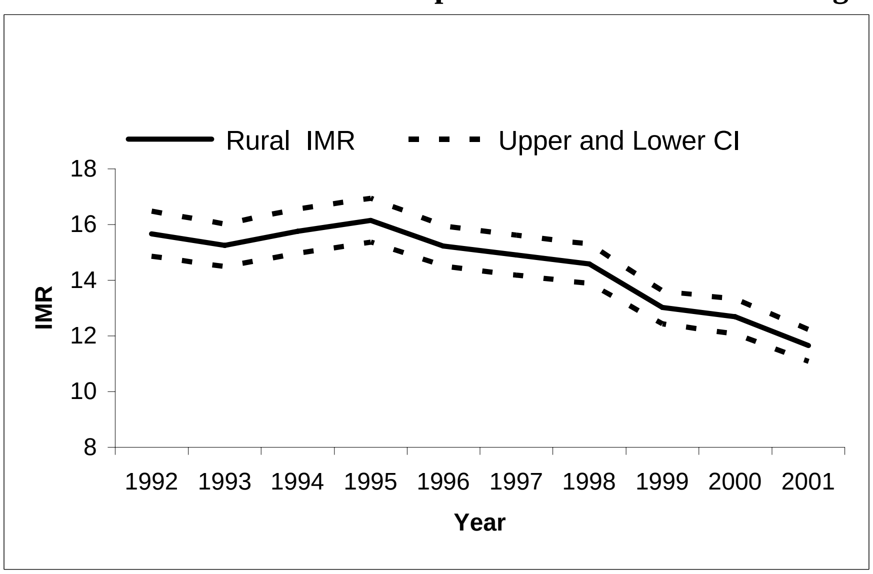


Figure 5: Trends in Rural IMR for Municipalities That Enter the Program in 1999.

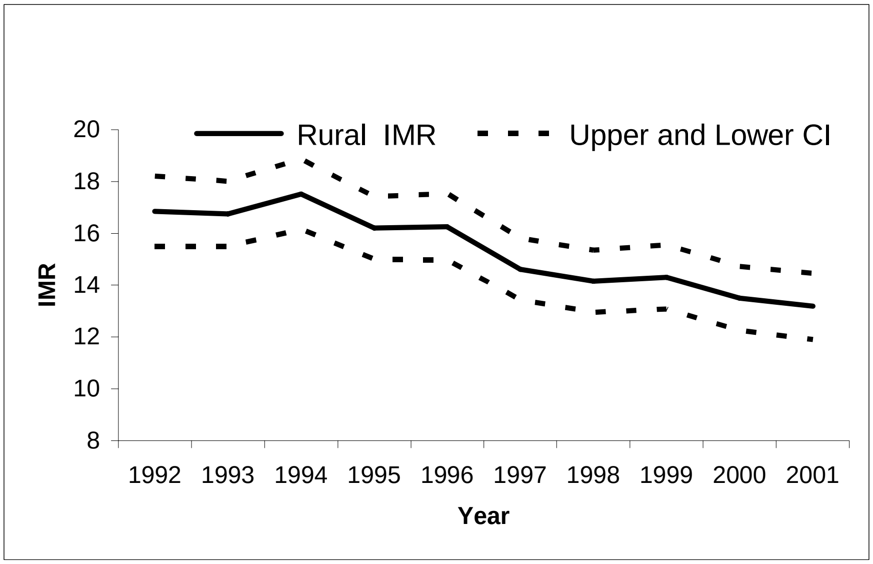


Figure 6: Trends in Urban IMR by Year Municipality Entered Program.

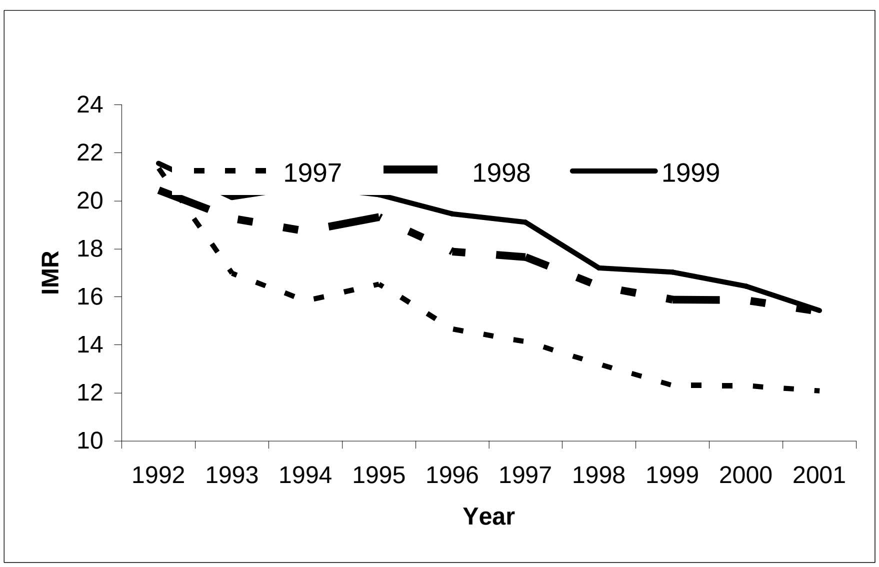
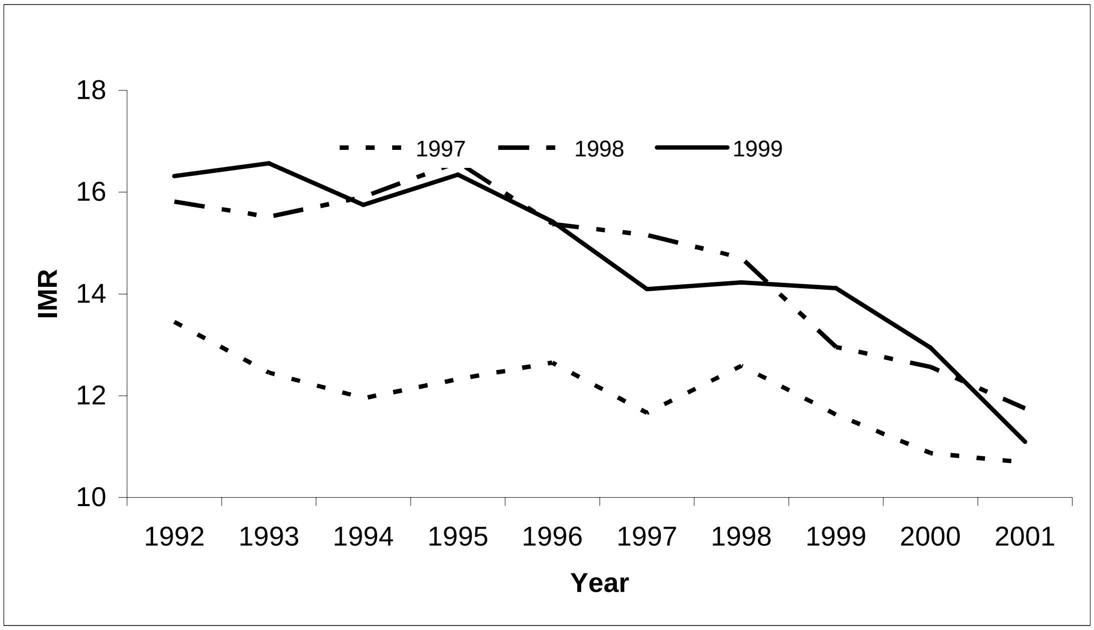


Figure 7: Trends in Rural IMR by Municipality Entry Date.



Note: Only municipalities with an average program intensity of at least 30% included.

Table 1: Difference in Pre-Intervention Trends in Rural Infant Mortality Rate by Date Municipality Entered Program.

Year	Municipalities that entered in 1997	Difference in IMR between municipality by entry date compared to 1997				
		No Progresa	1998	1999	2000	2001
Mean IMR 1990 = 21.17						
1991	-3.704	5.813	0.99	0.462	17.876	3.793
	[0.903]	[6.765]	[0.999]	[1.349]	[22.539]	[2.534]
1992	-3.758	-3.436	-1.809*	-1.065	16.823	2.415
	[0.863]	[4.660]	[0.952]	[1.305]	[12.120]	[2.612]
1993	-4.605	-5.882	-1.289	-0.495	-3.135	-0.148
	[0.892]	[4.626]	[0.979]	[1.301]	[10.327]	[2.435]
1994	-4.624	-10.010**	-0.822	0.31	-5.713	2.221
	[0.908]	[4.346]	[0.996]	[1.330]	[11.242]	[2.354]
1995	-4.519	-12.081***	-0.54	-1.182	2.781	5.315**
	[0.871]	[4.192]	[0.960]	[1.324]	[12.304]	[2.557]
1996	-4.609	-10.494**	-1.45	-1.07	20.293	-2.145
	[0.905]	[4.194]	[0.991]	[1.344]	[29.969]	[2.204]

Notes:

1. Standard errors in brackets.
2. * significant at 10%; ** significant at 5%; *** significant at 1%.
3. See equation 1 for the specification of the equation corresponding to these results.
4. 1990 was the year left out and municipalities that entered in 1997 was the group of municipalities left out.
5. Column 2, is the decrease in the rural IMR between 1990 (21.17) and the other years for municipalities that entered in 1997.
6. Column 3, is the difference in the decrease in rural IMR between municipalities that entered in 1997 and those that never received Progresa.
7. Columns 4-7 show the difference in the decrease in rural IMR between municipalities that entered in 1997 and those that entered in later years.

Table 2: Number of New Program Localities Between 1997-2001 by the Date the Municipality Started the Program.

Year the Municipality Entered the Program	Year 1997	1998	1999	2000	2001
1997	2,424	4,705	5,560	5,538	5,927
1998		28,261	35,222	440	9,413
1999			16,726	240	2,548
2000				46	23
2001					376

Table 3: Differences in Means of Pre-Program Locality Characteristics, by phase group.

	Percent of: Workers in the primary sector (1990)	Percent of: Indigenous speakers (1995)[a]	Percent of: Illiterates (1995)[b]	Average number of occupants in a household (1995)	Marginalization Grade (00-95)[c]	Percent of Households With: Dirt floor (1990)	Dirt floor (2000)	Piped water (1995)	Sewage (1995)	Electricity (1995)
Mean for Phase Group 1997	76.4	22.7	27.1	5.4	4.5	71.9	50.2	41.4	13.2	65.2
	[0.6]	[0.7]	[0.4]	[0.0]	[0.0]	[0.8]	[0.6]	[1.0]	[0.6]	[1.1]
Differences in Means Between other Groups and Phase Group 1997										
Phase 1998 - Phase 1997	2.8***	-3.9***	1.5***	0.1***	0.1***	-13.5***	1.6**	-4.7***	-2.9***	-5.5***
	[0.6]	[0.8]	[0.4]	[0.0]	[0.0]	[0.9]	[0.7]	[1.0]	[0.6]	[1.2]
Phase 1999 - Phase 1997	-2.4***	-6.1***	-0.7*	-0.2***	-0.4***	-28.3***	-5.4***	3.9***	5.7***	-3.0**
	[0.7]	[0.8]	[0.4]	[0.0]	[0.0]	[0.9]	[0.7]	[1.1]	[0.6]	[1.2]
Phase 2000 - Phase 1997	0.2	-5.3***	-0.6	-0.2***	-0.5***	-32.0***	-8.0***	1.9	6.1***	-2.8
	[1.5]	[1.2]	[0.8]	[0.1]	[0.1]	[1.9]	[1.5]	[2.5]	[1.4]	[2.2]
Phase 2001 - Phase 1997	2.3***	-5.5***	2.1***	-0.2***	-0.1***	-33.7***	1.4**	-7.9***	1.3**	-20.4***
	[0.7]	[0.8]	[0.4]	[0.0]	[0.0]	[0.9]	[0.7]	[1.1]	[0.6]	[1.2]
Observations	53624	63771	63771	63771	64213	64328	62023	63771	63771	63771

Notes:

a. Percent of population over 4 year olds.

b. Percent of population over 14 year olds.

c. The marginalization grade ranges from 0 to 5 with 5 being the most marginalized.

1. Standard errors in brackets.

2. * significant at 10%; ** significant at 5%; *** significant at 1%.

3. Time and municipality fixed effects included.

Table 4: Change in Mean Locality Characteristics Between 2000 and Pre-Program Time Period, by phase group.

	Percent of			Average	Marginal-	Percent of Households With			
	Workers In the Primary Sector (00-90)	Indigenous Speakers (00-95)[a]	Iliterates (00-95)[b]	Number of Occupants in a Household (00-95)	ization Grade (00-95)[c]	Dirt Floor (00-90)	Piped Water (00-95)	Sewage (00-95)	Electricity (00-95)
Mean for Phase Group 1997	-10.292	-0.406	-2.595	-0.461	-0.368	-21.838	7.921	6.936	11.947
	[0.640]	[0.229]	[0.264]	[0.022]	[0.017]	[0.897]	[0.845]	[0.720]	[0.961]
Differences in the change between other Phase Groups and Phase Group 1997									
Phase 1998 - Phase 1997	0.305	0.326	-0.377	0.012	0.002	14.346***	0.07	1.745**	0.853
	[0.666]	[0.238]	[0.273]	[0.023]	[0.018]	[0.945]	[0.871]	[0.744]	[1.000]
Phase 1999 - Phase 1997	1.058	0.264	0.329	0.056**	0.368***	21.426***	-4.926***	0.679	-3.413***
	[0.694]	[0.244]	[0.287]	[0.025]	[0.019]	[0.983]	[0.905]	[0.773]	[1.018]
Phase 2000 - Phase 1997	1.745	-0.032	-0.658	0.081	0.345***	22.513***	-6.218***	-0.828	-3.552**
	[1.533]	[0.521]	[0.576]	[0.063]	[0.049]	[1.944]	[2.234]	[1.647]	[1.714]
Phase 2001 - Phase 1997	3.034***	0.332	0.323	0.114***	0.245***	33.007***	-5.154***	-1.158	-1.221
	[0.744]	[0.250]	[0.304]	[0.026]	[0.019]	[1.031]	[0.941]	[0.792]	[1.049]
Observations	58039	68043	68043	68043	68859	67661	68043	68043	68043

Notes:

a. Percent of over 4 year olds.

b. Percent of over 14 year olds.

c. The marginalization grade ranges from 0 to 5 with 5 being the most marginalized.

1. Robust standard errors in brackets.

2. * significant at 10%; ** significant at 5%; *** significant at 1%.

3. Time and municipality fixed effects taken out.

Table 5: Mean Municipality Program Intensity by the Year the Municipality Entered the Program.

Year the Municipality Entered the Program	Year 1997	1998	1999	2000	2001
1997	0.24	0.55	0.59	0.55	0.57
1998		0.34	0.46	0.44	0.49
1999			0.30	0.29	0.36

Notes

1. Program intensity is defined as the proportion of rural household receiving Progresa benefits in December of a given year.

Table 6: Impact of Progresa on IMR.

	Rural IMR					Urban IMR
	[1]	[2]	[3]	[4]	[5]	[6]
Program intensity	-0.812	0.169	0.1			
	[0.736]	[0.682]	[0.679]			
Lag of program intensity		-1.909**	-2.164***	-4.868**	-1.898**	0.38
		[0.873]	[0.820]	[2.304]	[0.865]	[1.388]
Lag of lag of pogram intensity			0.202			
			[0.787]			
Lag of program intensity squared				3.861		
				[2.776]		
Year 1993 (=1)	-0.365	-0.366	-0.366	-0.366	-0.366	-1.392***
	[0.270]	[0.270]	[0.270]	[0.270]	[0.270]	[0.284]
Year 1994 (=1)	0.100	0.100	0.100	0.101	0.101	-1.750***
	[0.298]	[0.298]	[0.298]	[0.298]	[0.298]	[0.361]
Year 1995 (=1)	0.177	0.177	0.177	0.178	0.178	-1.406***
	[0.310]	[0.310]	[0.310]	[0.310]	[0.310]	[0.348]
Year 1996 (=1)	-0.527*	-0.527*	-0.527*	-0.527*	-0.527*	-2.757***
	[0.290]	[0.290]	[0.290]	[0.290]	[0.290]	[0.401]
Year 1997 (=1)	-1.161***	-1.180***	-1.179***	-1.176***	-1.176***	-3.013***
	[0.294]	[0.294]	[0.295]	[0.294]	[0.294]	[0.374]
Year 1998 (=1)	-1.162***	-1.429***	-1.418***	-1.331***	-1.361***	-4.440***
	[0.371]	[0.363]	[0.363]	[0.303]	[0.304]	[0.414]
Year 1999 (=1)	-2.283***	-2.185***	-2.094***	-1.798***	-2.110***	-5.045***
	[0.450]	[0.468]	[0.461]	[0.451]	[0.406]	[0.446]
Year 2000 (=1)	-2.752***	-2.316***	-2.334***	-1.869***	-2.249***	-5.213***
	[0.474]	[0.564]	[0.585]	[0.598]	[0.534]	[0.731]
Year 2001 (=1)	-3.521***	-3.194***	-3.147***	-2.764***	-3.170***	-5.867***
	[0.508]	[0.566]	[0.647]	[0.598]	[0.514]	[0.946]
Observations	18891	18852	18818	18956	18956	12164
Adjusted R^2	0.59	0.59	0.59	0.59	0.59	0.56
Mean of dependent variable	17.5	17.5	17.51	17.5	17.5	19.07
Municipality fixed effects	Y	Y	Y	Y	Y	Y

Notes:

1. Standard errors in brackets. Standard errors are robust and clustered at the municipality level.
2. * significant at 10%; ** significant at 5%; *** significant at 1%
3. All regressions are weighted by number of rural/urban households in municipality.
4. Program intensity is defined as the proportion of rural household receiving Progresa benefits in December of a given year.
5. IMR=infant mortality rate, it is the number of deaths before the age of 1 per 1000 live births.

Table 7: The Impact of Progresa on IMR Controlling for Health Supply.

	Rural IMR						Urban IMR
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Lag of program intensity	-1.898**	-1.790**	-1.836**	-1.920**	-1.889**	-1.883**	0.168
	[0.865]	[0.887]	[0.888]	[0.864]	-0.867	[0.888]	[1.387]
% of Progresa localities with free health clinic				0.011		0.014	0
				[0.023]		[0.024]	[0.020]
Population density					0.365	-2.922	-5.489
					[1.11]	[2.577]	[5.929]
Observations	18956	18940	18940	18956	18940	18940	12164
Adjusted R^2	0.59	0.58	0.58	0.59	0.59	0.58	0.56
Mean of dependant variable	17.5	17.51	17.51	17.5	17.5	17.51	19.07
Year effects	Y	Y	Y	Y	Y	Y	Y
Municipality fixed effects	Y	Y	Y	Y	Y	Y	Y
Health infrastructure		Y	Y			Y	Y
Health personnel			Y			Y	Y

Notes:

1. Standard errors in brackets. Standard errors are robust and clustered at the municipality level.
2. * significant at 10%; ** significant at 5%; *** significant at 1%
3. All regressions are weighted by number of rural/urban households in municipality.
4. Program intensity is defined as the proportion of rural household receiving Progresa benefits in December of a given year.
5. IMR=infant mortality rate, it is the number of deaths before the age of 1 per 1000 live births.
6. Health clilnic information for SSA and IMSS-SOL only. This is health infrastructure for the uninsured.
7. Health infrastructure variables are all per 1000 population and include the number of: rural clinic rooms, mobile clinics, hospitals, walking health teams
8. Health personnel variables are all per 1000 population and included the number of: doctors, residents, and nurses in contact with the patient in rural areas.

Table 8: The Impact of Progresa on IMR Controlling for Municipality Characteristics and Time Trends.

	Rural IMR								Urban IMR	
	[1]	[2]	[3]	[4]	[6]	[7]	[8]	[9]	[10]	[11]
Lag of program intensity	-1.919**	-1.968**	-1.970**	-1.869**	-1.855**	-1.899**	-1.825**	-3.06***	-0.513	0.53
	[0.898]	[0.898]	[0.898]	[0.901]	[0.910]	[0.885]	[0.890]	[0.96]	[1.480]	[1.006]
Municipality characteristics for localities that eventually receive Progresa benefits										
Percent of households with :										
Piped water	0.007						0.003		-0.030	
	[0.020]						[0.021]		[0.017]	
Electricity		0.066					0.070*		-0.016	
		[0.040]					[0.041]		[0.0030]	
Sewage			-0.014				-0.013		-0.017	
			[0.018]				[0.018]		[0.018]	
Percent of:										
Rural population >4 that speaks an indigenous language				0.118			0.132		-0.032	
				[0.145]			[0.161]		[0.037]	
Rural population >14 that is illiterate					0.07		0.113		-0.033	
					[0.120]		[0.131]		[0.152]	
Average number of occupants in rural households						1.78	0.787		-2.681	
						[1.692]	[1.990]		[1.700]	
Observations	18804	18804	18804	18804	18804	18804	18804	18940	12037	12164
Adjusted R^2	0.58	0.58	0.58	0.58	0.58	0.58	0.58	0.62	0.56	0.62
Mean dependent variable	17.55	17.55	17.55	17.55	17.55	17.55	17.55	17.55	19.04	19.04
Year effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Health supply controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual municipality time trend!	N	N	N	N	N	N	N	Y	Y	N

Notes:

1. Standard errors in brackets. Standard errors are robust and clustered at the municipality level.
2. * significant at 10%; ** significant at 5%; *** significant at 1%
3. All regressions are weighted by number of rural/urban households in municipality.
4. Program intensity is defined as the proportion of rural household receiving Progresa benefits in December of a given year.
5. IMR=infant mortality rate, it is the number of deaths before the age of 1 per 1000 live births.

Table 9: The Impact of Progresa on IMR Controlling for Municipality Characteristics in Progresa Areas.

	Rural IMR							Urban IMR
	[1]	[2]	[3]	[4]	[6]	[7]	[8]	[10]
Lag of program intensity	-2.623***	-2.074**	-1.602*	-2.002**	-2.117**	-1.892**	-2.602***	0.684
	[0.911]	[0.922]	[0.934]	[0.923]	[0.895]	[0.888]	[0.989]	[1.230]
Municipality characteristics for localities that receive Progresa benefits								
Percent of households with :								
Piped water	-0.027***						-0.033***	0.027**
	[0.006]						[0.007]	[0.011]
Electricity		-0.009					0.009	-0.003
		[0.006]					[0.012]	[0.014]
Sewage			0.012				0.019*	0.046*
			[0.009]				[0.010]	[0.027]
Percent of:								
Rural population >4 that speaks an indigenous language				0.003			-0.003	0.004
				[0.006]			[0.008]	[0.010]
Rural population >14 that is illiterate					0.016		0.037*	-0.022
					[0.011]		[0.020]	[0.026]
Average number of occupants in rural households						-0.09	-0.158	-0.283
						[0.075]	[0.210]	[0.226]
Observations	18940	18804	18804	18804	18804	18804	18804	12037
Adjusted R^2	0.58	0.58	0.58	0.58	0.58	0.58	0.58	0.56
Mean dependent variable	17.55	17.55	17.55	17.55	17.55	17.55	17.55	19.04
Year effects	Y	Y	Y	Y	Y	Y	Y	Y
Municipality fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Health supply controls	Y	Y	Y	Y	Y	Y	Y	Y

Notes:

1. Standard errors in brackets. Standard errors are robust and clustered at the municipality level.
2. * significant at 10%; ** significant at 5%; *** significant at 1%
3. All regressions are weighted by number of rural/urban households in municipality.
4. Program intensity is defined as the proportion of rural household receiving Progresa benefits in December of a given year.
5. IMR=infant mortality rate, it is the number of deaths before the age of 1 per 1000 live births.

Table 10: Impact of Progresa on the Number of Registered Live Births per 1000 Population.

	Rural		Urban
	[1]	[2]	[3]
Lag of program intensity	0.344	-0.124	-1.249
	[1.273]	[1.247]	[0.785]
Observations	20922	20842	12709
Adjusted R^2	0.49	0.5	0.63
Mean dependent variable	31.63	31.59	30.88
Year effects	Y	Y	Y
Municipality fixed effects	Y	Y	Y
Health suppy controls	N	Y	Y

Notes:

1. Standard errors in brackets. Standard errors are robust and clustered at the municipality level.

2 * significant at 10%; ** significant at 5%; *** significant at 1%

3. All regressions are weighted by number of rural/urban households in municipality.

4. Program intensity is defined as the proportion of rural household receiving Progresa benefits in December of a given year.

5. IMR=infant mortality rate, it is the number of deaths before the age of 1 per 1000 live births.

Table 11: Heterogeneity of the Impact of Progresa on IMR by Pre-Intervention Municipality Characteristics.

	Rural IMR				
	[1]	[2]	[3]	[4]	[5]
Lag of program intensity	-2.048**	-1.653*	-1.759*	-1.945**	-1.962**
	[0.909]	[0.911]	[0.924]	[0.908]	[0.900]
Interaction of the Lag of Program Intensity with an indicator variable that in 1995:					
30-100% of households in Progresa villages have a sewage system	1.818*				
	[1.020]				
75-100% of households in Progresa villages have piped water into household		-3.630***			
		[1.081]			
91-100% of households Progresa villages have electricity in the houseold			-1.617		
			[1.000]		
80-100 % of over 15 year olds are literate in Progresa villages				0.179	
				[1.038]	
0 % of the population only speaks an indigenous language in Progresa villages					-3.715*
					[2.152]
Observations	18792	18792	18792	18792	18792
Adjusted R^2	0.58	0.58	0.58	0.58	0.58
Mean dependent variable	17.56	17.56	17.56	17.56	17.56
Year effects	Y	Y	Y	Y	Y
Municipality fixed effects	Y	Y	Y	Y	Y
Health suppy controls	Y	Y	Y	Y	Y
Other municipality characteristics[1]	Y	Y	Y	Y	Y

Notes:

1. These municipality characteristics are an aggregation of the locality characteristics of Progresa areas only.
2. Standard errors in brackets. Standard errors are robust and clustered at the municipality level.
3. * significant at 10%; ** significant at 5%; *** significant at 1%
4. All regressions are weighted by number of rural/urban households in municipality.
5. Program intensity is defined as the proportion of rural household receiving Progresa benefits in December of a given year.
6. IMR=infant mortality rate, it is the number of deaths before the age of 1 per 1000 live births.